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THE
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SATURDAY, AUGUST 6, 1831.

LECTURES
ON
MEDICAL JURISPRUDENCE,

Delivered in the University of London.

BY PROFESSOR AMOS.

MURDER AND MANSLAUGHTER.

Poisoning with Prussic Acid—Medical Evidence, with Remarks on a Case noticed in Dr. Christison's book—Evidence in a Case tried at Nottingham, for poisoning a Mare—Instances of the Importance of Medical Testimony—Occurrence of Lock Jaw after a Wound—Probable occurrence of Apoplexy—Other Injuries—Medical Evidence on the Trial of Booth, at Warwick, in 1808.— CONCLUSION.

GENTLEMEN,—In the last two lectures I endeavoured to place before you the law relating to insanity: I shall now proceed to the concluding subject of my course—the law of *Homicide*; and as the medical session at the University has closed, I shall endeavour to compress into the present lecture as much original matter as I think is calculated to illustrate the subject to you.

I shall begin with a case, of which you will find the following notice in Dr. Christison on Poisons:—

" A very interesting trial lately took place in this country, when the fate of the prisoner depended, in a great measure on the question within how short a time the effects of poison must shew themselves*? The nature of the case was as follows:—An apothecary's maid-servant, at Leicester, who was pregnant by her master's apprentice, was found one morning dead in bed; and she had obviously been poisoned with hydrocyanic acid. A number of circumstances led to the suspicion that the apprentice was accessary to the administration of the poison. On the other hand, it was distinctly proved that the deceased had made arrangements for a miscarriage by artificial means on the night of her death; and it was therefore presumed, on the part of the prisoner, that she had taken the poison of her own accord. But the body was found stretched out in bed in a composed posture, with the arms crossed over the trunk, and the bed-clothes pulled smoothly up to the chin; and in her right hand lay a small narrow-necked phial, from which about five drachms of the medicinal prussic acid had been taken, and which was corked and wrapped in paper. There naturally arose a question whether the deceased, after drinking the poison out of such a vessel, could, before becoming insensible, have time to cork the phial, wrap it up, and adjust the bed-clothes. To settle the point, experiments were made, at the request of the judge, by Mr. Macauley, of Leicester, in presence of several other medical men; and on the trial, they (with a single exception) gave it as their opinion, founded on the experiments, that the supposed acts of volition, although within the bounds of possibility, were in the highest degree improbable. The chief experiments were three in number; from which it appeared that one dog was killed with four drachms in eight seconds, another with four drachms in seven seconds, and another with four drachms and a half in three seconds. For these particulars I am indebted to Mr. Macauley. The evidence of the majority of the witnesses appears to me quite correct. I conceive the medical evidence proves that some one must either have been present at the time the deceased took the poison or have arranged the body soon after death: for if she had time to cork, and wrap up the phial, the case must have been of that slower description which is attended with convulsions, so that the body would have been in a discomposed attitude; and if the case was of the sudden kind, when convulsions do not occur, she could not have corked and wrapped up the phial, and also adjusted the bed-clothes. The prisoner was found, *not guilty*."

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* Trial of Freeman, for the murder of Judith Buswell, Leicester, April 2, 1829.

So far Dr. Christison. I happen, however, to have here, in my possession, a complete report of the whole trial, taken in short-hand by a professional reporter, but which has never yet been published*. From this manuscript I will read to you the evidence of the medical men who were summoned to attend.

"*Mr. Thomas Paget.*—Is a surgeon in Leicester; was called to Mr. Biggs's house on the 12th of February, at half-past seven in the morning. Went into the deceased's room, and found her lying on her back, with her head resting on the back of the bed-board. She was dead. Judging from the rigidity of the muscles, she seemed to have been dead four or five hours; did not stay many minutes; did not remove the bed-clothes; did not turn them down; told them to let the things remain as they were, and inform the Coroner immediately; returned about four o'clock in the afternoon; the room was in the same state as in the morning; the deceased was lying in the same form, with the exception of her left hand being nearer to her stomach; proceeded, together with Mr. Wilkinson, another surgeon, to examine the body; found the body in a state of freshness; attributed her death to her stomach containing a great quantity of prussic acid; the contents of her stomach were strongly impregnated with prussic acid; she was between six and seven months gone with child; the child was recently dead.

Cross-examined. — From experiments which witness has seen tried on animals, thinks it is *possible* she might have had the power of corking the bottle.

Re-examined.—The operation of prussic acid is always calculated by seconds; cannot tell how long half an ounce would be in its operation upon the human species; it would operate sooner upon brutes; has seen a small dog rendered insensible in ten seconds by the same quantity the deceased is supposed to have taken; has also given a cat half the quantity, which he conceives fully adequate to her frame; she was thirty seconds before she fell insensible; another cat, to which a quarter of the quantity was given, fell insensible within twenty seconds; mentioned this as showing the irregularity of prussic acid in its operation; the two doses were out of the same bottle, shewing its effect on different constitutions; the last-mentioned cat was a stronger and larger cat than the one which took thirty seconds to be rendered insensible.

By the Judge.—The animal appeared conscious of what it was about till the moment it was seized with convulsions; cats and dogs walk about the room for several seconds after having taken it till they are seized with convulsions; thinks deceased might have had knowledge to cork the bottle after she had taken the poison; thinks it is *possible*; cannot form an idea as to the probability; smelling the bottle will produce sickness and insensibility in some people; it will produce a sense of sickness, and in others insensibility; its force would not be weakened by being kept in a dark place a fortnight—thinks not; druggists are obliged to leave it and other poisons accessible to their apprentices for making up prescriptions; some of the experiments spoken of were made with the prussic acid that remained in the bottle.

Edward E. Wilkinson.—Is surgeon to the Leicester Infirmary; was with the last witness when the body of the deceased was opened; concurs with him in his opinion as to the cause of her death; is unable to form any opinion of the quantity of prussic acid taken into the stomach; saw the chamber-pot; a small piece of leather in it; it appeared as if it had been tied round the top of the bottle; there was likewise a small string, which seemed to have been used for the purpose of tying the leather round the neck of the bottle; was present when experiments were performed on the Wednesday.

Cross-examined.—The piece of leather was found in the pot; did not perceive there was any sickness in the pot; it was past four in the afternoon when he saw it; should think smelling prussic acid would produce faintness; has seen experiments tried this morning; if deceased took four drachms, should think she *could not* have power to cork the bottle; saw a dog take four drachms—it died in eight seconds; from what remained in the bottle produced, should think four drachms were taken by the deceased; never heard of prussic acid being taken to procure abortion—it might be possible.

Dr. Freer examined.—Was present at the time Mr. Macauley made experiments the day before yesterday with the acid found in the bottle; has been in practice as a physician twenty years; prussic acid is the most violent poison that is known; thinks its immediate effects would be in proportion to the strength of the dose administered; by being exposed to the light it might lose some of its effect; saw experiments made with fresh acid and the acid found in the bottle; it varied very little in its effect from the fresh; sixty drops is a drachm; thinks that after three or four drachms had been received into the stomach, a person would not be able to do the most trifling act—certainly not; thinks it *impossible* for a person after having taken such a quantity to put a cork into a bottle.

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* [We have been favoured with the MS. of this interesting trial, and hope to present our readers with a printed copy of it in an early number.—E. G.]

Cross-examined.—His judgment is chiefly from analogy; saw some experiments tried upon dogs the day before yesterday; never saw any experiments tried before; there were seven experiments; lays most stress upon the sixth experiment; four drachms and a half were given to a dog, and its effect was almost instantaneous; in his opinion three seconds had not elapsed before the dog was perfectly insensible; it was a sort of terrier, a small one; should imagine its operation upon the human species to be equally as speedy as upon brutes; its operation might vary according to the constitution; so large a dose is sufficient to destroy any constitution; never attended any person supposed to have taken it; have seen it administered in a small dose as a medicine. A dog which had taken twenty drops in five or six minutes recovered; the last experiment which was performed was giving this identical dog forty drops more; in twenty seconds he fell, howling; in thirty five seconds he became insensible, but still breathing; in one minute he was gasping; in a minute and a half worse; in two minutes he appeared at his last gasp; in three minutes and fifty seconds he again gasped; by five minutes he was dead. — Experiment, No. 6. Four drachms and a half were given; in four or five seconds the dog was insensible and dead. No. 3 was a large dog, and double the size of the other.—Four drachms were given to him; half was lost in forcing the animal to swallow; he was a large setter dog; in thirty seconds he staggered; in forty seconds he fell; in one minute and a half dead; the immediate effect of the poison would depend on the dose taken; if food was in the stomach it would certainly make a difference; if it was a small dose it would retard the progress of the poison; if it was a large dose it would make little or no opposition.

Mr. John Needham examined.—Is a practising surgeon in this town; was present at the experiments to which Dr. Freer has spoken, and took the minutes; concurs mainly with him in what he has said; each animal was in a greater or less degree affected in a few seconds; one fell in half a minute; the acid taken by the deceased produced insensibility equally at the same time as the fresh, but it did not occasion death so immediately; deaths happened at forty seconds with the fresh, and at sixty-five with the other; the animals appeared in great distress, and endeavoured to eject; thinks the acid was deteriorated by the light; the comparative effect was precisely the same; insensibility was produced nearly at the same time in both the dogs; should think it was *most improbable, and next to an impossibility,* that the deceased could have had the power of corking the bottle after taking the prussic acid, and put her hands under the clothes; comes to that conclusion from the experiments he has made.

Cross-examined. — One dog had forty drops, and one twenty before they fell; dogs require the same doses in other medicines as human beings do; cannot say as to the effect of the poison; saw one experiment made upon a horse; six drachms were administered; it began to be affected in a minute; fell in two minutes; was perfectly insensible and dead; its heart was beating; the heart beat many minutes after it ceased to breathe; it was full a minute before it was affected at all; thinks a horse would take strong poisons without being affected; might take half an ounce of arsenic.

Mr. Thomas Macauley.—Is a surgeon in this town; was present with Dr. Freer and others when the experiments were tried; concurs in the opinion they have given as to those experiments; received the bottle containing prussic acid, found in the bed, from Mr. Owston, previous to commencing experiments; measured the quantity of acid; there were more than three drachms, but less than four; about three and a half drachms; eight drachms is an ounce; some of the experiments were made with the identical acid that remained in the bottle; there was a difference in the acid taken from the bottle and the fresh acid; it was very trifling; has made previous experiments without a view to this transaction; *without denying the possibility of it,* thinks it *extremely improbable* the deceased could have corked the bottle; and from what he has read of the opinions formed by eminent medical writers, thinks it *impossible* it could be done, the operation having been done in the dark; has tried experiments on dogs and cats; its operation depends upon the dose; has given five, ten, and twenty drops; some have fallen directly, and some have walked after it for a short time; don't know what quantity deceased took; has seen dogs walk moderately well after a small dose; never saw one walk after a large one.

Mr. J. P. Stallard examined.—Is a surgeon in this town; been in practice twelve years; has studied surgery all his life; was present at the experiments spoken of, and agrees in the result; reasoning from analogy, it is *not possible* for a person after taking four drachms to cork a bottle; *it is impossible.*

Cross-examined.—Has seen the effect of prussic acid upon a human being, when given as a medicine; saw its effect upon his wife's sister; seven drops were given her, diluted; tried its effect upon a horse; after giving it three ounces it fell in two minutes and twenty seconds; another horse had six drachms given to it; it was dead sooner than the one that had three ounces.—*By the Judge.*—Accounts for the quickness of the operation where the six drachms were given by having applied some previously to the eye and nostrils of the horse. *By Mr. Denman.*—"His wife's sister had a violent spasmodic affection; several persons were obliged to hold her; has seen seven drops put her in a state of semi-death, having previously been in great pain; she is now living."

Time will not permit me to go through other parts of the general evidence for and against the prisoner. He was found not guilty, it is true; but the circumstances were, by no means, so strong against him as Dr. Christison's notice would lead his readers to suppose. It did not appear in evidence that the arms of the deceased were crossed over her body, the bed-clothes pulled smooth up to the chin, nor was the phial *wrapped up in paper* as well as corked; while a material fact in the prisoner's favour, and which seems to have weighed much with the jury, was, that in order to administer the deadly dose, he should have opened three doors without noise, and passed through the sleeping-room of a fellow-resident (the shop-man of his master), in order to get at the apartment of the deceased. Nothing can be more correct than the *inference* of the learned Professor of Medical Jurisprudence; but it is to be regretted that he had not the whole of the facts before him.

I shall next mention another unpublished case of poisoning.

James Mason, the assistant of Mr. Samuel Russell, of Blythe, was tried at the Nottinghamshire assizes, in 1829, for poisoning a mare, the property of Mr. Russell, by giving her, with intent to kill, a quantity of sulphuric acid. I will read to you the medical evidence in the case, as it seems to be deserving of your particular attention.

"*Edward Robinson, a Veterinary Surgeon.*—Saw the pony on November 7th; examined her mouth; the lips swelled; the mouth ulcerated; sloughing. I think those appearances could not have arisen from natural causes. I think sulphuric acid would produce those effects.

Cross-examined.—Never before saw the effects of sulphuric acid on the mouth of a horse. It was Mr. Russell who then suggested that it might be sulphuric acid. Gave no opinion as to pony's life being in danger. *Any caustic would produce those appearances*; muriatic acid would. Never saw it applied to a horse. Sulphuric acid would burn clothes. It might be administered by a syringe, or bottle, if pony was quiet. Those appearances could not have been produced by natural causes; I have never seen them.

Re-examined. Sulphuric acid would produce such effects on other parts of a horse; I have seen it done.

Samuel Peach, a Veterinary Surgeon.—Examined the pony on November 11th. Examined head and mouth. Ulceration and sloughing. It appeared to be from application of caustic. Sulphuric acid would likely produce those appearances. Stomach also in state of ulceration. Appearances also there. Duodenum partially inflamed. The appearances were most assuredly not from natural causes; sufficient to occasion the death of a horse. If sulphuric acid administered, effects would be instantaneous; I think it could not have been administered and pony have travelled seven miles. It would not alter appearance of mouth in less than a quarter or half an hour. I should think a horse would not eat corn an hour after application of sulphuric acid.

Cross-examined.—Most likely she died of constitutional irritation, produced by the spots. Examined lungs. Air-vessels thickened and obliterated, from chronic disease. Lungs might have lasted a long time. Could not say what acid had been employed; if nitric acid had been used, animal could not have gone seven miles. Froth would be the effect of moving the tongue, not the caustic. Never had any doubts of cause of death of pony; never said he would defy any man to say whether the mare was poisoned or not. The immediate effects of acids are well known to medical men.

Thomas de Grey Brewerton.—Live at Bawtry; surgeon there; in the habit of making experiments in chemistry; examined pony after death. The appearances: mouth were as if corrosive liquid had been put on the tongue, and then ran under the tongue, over the lips, and out of the mouth; a similar effect was produced on stomach, at upper orifice of stomach, and duodenum. I speak of effects of corrosive liquid, not of inflammatory appearances besides. Took home with me part of the tongue, lip, and the duodenum, for purpose of trying to extract liquid made use of. The result, particularly as relates to the tongue, was, that it afforded, on chemical examination, a larger quantity of sulphuric acid than could be expected to be found either in saliva of horse or soft parts of the mouth, as they are not known to contain any sulphuric acid. In my judgment, sulphuric acid occasioned the animal's death. Have examined a healthy tongue; found no sulphuric acid in it. The effect of destroying the vitality of the surface would be instantaneous from sulphuric acid.

Cross-examined.—I examined a healthy tongue that I might have fact as well as theory. Prisoner had once been at my house. His general reputation and character were good. I should presume that he would know that sulphuric acid would produce those effects.

Benjamin Robinson, a Physician at Retford, of thirty years' practice.—Sulphuric acid, I conceive, would produce excruciating pain. I should think the animal would shew pain and uneasiness. I think it could not be passive. I think, if capable of living several days, would often exhibit symptoms of pain. I think it more probable that the horse would reject the acid than let it go down the throat. If the stomach was acted upon by such an acid, the œsophagus could not be in a healthy state. Cases I have heard of where the mucous membrane of the stomach has become inflamed: the result of inflammation in the mucous membrane is ulceration and gangrene. I think it possible that inflammation may take place in mucous membrane, setting aside the detection of the sulphuric acid which has been spoken of by one witness. I am of opinion that all the other symptoms might have been produced by natural causes, without human agency: mouth sympathises with stomach. From my knowledge, the statement respecting the sulphuric acid surprised me.

Cross-examined.—Lying down and getting up are symptoms of pain. These more a symptom of cholic and pain in the bowels. I should not suppose the getting up and lying down to be a symptom of pain in the stomach. If the evidence of the presence of sulphuric acid be true, it must have been introduced into the mouth. It is possible that part, by means of a syringe, might go down the throat—not physically impossible. A common occurrence in disease for mouth and fauces to be affected by sympathy with disease of stomach. I should think the appearance of the mouth could not have come on so suddenly from natural causes if horse had begun to eat his corn; appearances would be gradually coming on.

Re-examined.—If the sulphuric acid administered were through a syringe, it would affect the syringe. On a leaden syringe sulphuric acid acts slowly.

Jonas Kewney, a Veterinary Surgeon of Nottingham.—Have heard evidence of the witnesses: have heard appearances described; I think the appearances could not have arisen from natural causes; I think from application of caustic. Have frequently seen ulcerated mouths, but not that species of ulceration; have seen inflamed stomachs, occasioned by feeding on cut straw; heard gullet described; gullet would have exhibited great inflammation, if not ulceration, if sulphuric acid had passed into the stomach. From a horse lying down should think he had pain in his stomach or bowels; the lining of the gullet is not insensible, but less sensible than other parts; course of sulphuric acid would be rapid over the gullet; if the pony were eating his corn at the time, it might have passed over his gullet without producing inflammation.

Re-examined.—If given with a syringe I should think it would not pass into the stomach; gullet two feet long; if there was corn in the mouth the gullet would be the more likely to reject the acid.

John Calthorpe Williams.—I think the œsophagus could not have been untouched if sulphuric acid had been introduced into the mouth; the evidence as to detection of sulphuric acid has quite surprised me; I should have thought it impossible to have detected presence of sulphuric acid after that length of time; I should expect the sulphuric acid would be changed by changes of body; I should scarcely think the acid could be reproduced after ten days; acid cannot be reproduced so well after put on living matter as on dead; not aware that the gullet is more insensible to chemical agents than other coats; gullet contracts when food passes; it does not go down by its mere gravity."

We will next proceed to some charges of homicide, which have been occasioned by other means than by poisoning.

In a case tried at Nottingham a few years ago, a prisoner was indicted for murder, in cutting a person's throat. It was proved in evidence that the deceased's throat was cut; but that he had also received a severe blow, with a crow-bar, on the head. No medical witness was subpœnaed; but the judge asked, "Is there any medical gentleman in court?" Upon which a gentleman rose up and said, "I am one, my lord."—"Have you heard the evidence?" "Yes."—"Have you formed an opinion respecting the mode in which the deceased met with his death?" "Yes; that he died by the cutting of the throat, and not by the blow on the head?"—"What are the grounds of your opinion?" "Because the injury on the head, according to the description of it, though I think it would have produced death ultimately, would not have produced it so soon as it appears from the evidence the death must have ensued: and also because, if the deceased had been killed by the blow on the head, and afterwards his throat had been cut, so large a quantity of blood would not have flowed, as it appears from the evidence did flow from the wound." The opinion expressed by the medical gentleman was acquiesced in by the judge and the jury, who, but for his accidental presence in court, might have acquitted the prisoner.

At the last assizes at Warwick, a child was proved to have died of a fracture of the skull on the back part of the head, just above the ear. The evidence was, the child had been put out to *nurse* with the prisoner: the prisoner was proved to have come down stairs, leaving the child in a room above stairs; the prisoner returned up stairs, and in two or three minutes came down again screaming, and desiring that the mother of the child might be sent for. The child was found dead on the floor. The prisoner had been heard, a week or more before, to use expressions of dislike to the child, as, by saying, "it should eat its own dung." The prisoner's case was, that the child had fallen out of bed on the floor in a fit, and so had met with its death. The prisoner said, that, in going up stairs, she heard a "bump on the floor." The case on the part of the prosecution was, that the prisoner must have knocked the child's head against the bed-post, or some hard substance. Only one medical witness was called, who swore, that, in his opinion, it was impossible that the child could have met its death by a fall. I should tell you, that the bed, from which the child was supposed to have fallen, was two feet high from the floor; the child was sixteen months old; and the injury, as I before mentioned, was on the back of the head, above the ear. The surgeon was very much pressed by the judge, the counsel for the prosecution, and the counsel for the defence, who all seemed desirous of shaking his testimony. It appeared that he had only been three years in practice. At last the surgeon was got to say, that it was *not absolutely impossible* that the death might have been occasioned by a fall. And, upon this, the jury *acquitted* the prisoner.

In a trial for manslaughter at Lincoln, the prisoner and a constable had been struggling together, and the prisoner was striking at the constable with a spade; the spade glanced over the constable's shoulder, and hit the deceased, who was engaged in pulling away the constable. There was no doubt upon the point of law, that, although the blow was not intended for the deceased, the act would be equally as criminal as if the blow had fallen on the constable. But there was some doubt as to the question, whether the deceased died in consequence of the blow he so received. The medical evidence was as follows:—Mr. Harvey: "I am a surgeon of Lincoln; was called in to the deceased; he was sitting by the fire; had a handkerchief round his forehead; one inch above his eyebrow there was a lacerated, incised wound, from an instrument with an edge—it might have been from a spade; found the skull fractured; there were two or three spiculæ, or small portions of bone; I was called in on the 11th, within an hour after his receiving the injury; I attended him till the 17th; he died on the 18th. Opened his head after death; he died of lock-jaw; lock-jaw attends on wounds slight or serious; the ultimate cause of death was the lock-jaw; cannot say positively that the wound occasioned death, because the disease of lock-jaw is imperfectly understood even by the first of the faculty; there were nerves very near the part injured; if the nerves received an injury from the wound, it would account for the lock-jaw and subsequent death; believe that the wound did occasion the death; cannot speak positively; the wound was not sufficiently serious to have occasioned death, if lock-jaw had not ensued; if the deceased had applied brandy to the injured part, it would have produced great irritation; a very little scratch of the skin will sometimes produce lock-jaw; the brain was not injured." Mr. Best, surgeon: was called in on the 17th, at two o'clock in the afternoon (the injury occurred on the 11th, the death on the 18th); lock-jaw had not then taken place; there were symptoms of approaching lock-jaw; the lock-jaw was complete in two days after its first commencement; fair to presume that the death was occasioned by the wound; lock-jaw very likely to arise from such a wound, though it frequently arises from other causes, as *from cold;* and this was Christmas time, and very severe weather. The wound was one inch above the right orbit, and nerves were very near the wound; a prick from a needle or a thorn will sometimes produce lock-jaw. Found no cause of death but lock-jaw, and no cause of lock-jaw but the wound; had not an opportunity of observing whether any nerve was injured; should not have said, *à priori,* that the wound would have occasioned lock-jaw, or death. On which the judge asked, what would you say *à posteriori?* Answer.—Having heard what I have heard in Court, it is no matter of surprise to me that lock-jaw followed from the wound. The wound was very superficial; no sloughing, no discolouration, no extravasation of blood, nor the slightest inflammation; not one vessel turgid; no fissure or fracture, a mere superficial injury to the outer table of the skull. I considered it a very superficial blow, not likely to produce death—a very slight wound at first. The judge left it to the jury whether, in their judgment, the death was occasioned by the blow; and he said, that in his judgment the lock-jaw was occasioned by the injury to the head, as there was no account of any previous illness; and the jury found the prisoner *guilty of manslaughter;* though it was very doubtful whether the offence did not amount to murder.

I have often seen medical men very much puzzled when the question is put to them, whether they believe the death of a person was occasioned by an injury proved in court to have been received, particularly where the injury has been capable of producing death, but the death might, without any violent improbability, have proceeded from another cause. In a trial for manslaughter at Derby, the prisoner and the deceased had been wrestling. The prisoner had thrown the deceased with his head on a stone-floor by an underhold from the thigh; he then seized hold of the deceased by the throat, and beat his head several times against the floor. This was about ten o'clock at night, and the deceased died about four or five the next day. He died on a Saturday. On the next Monday the head was opened, and the scalp removed. A great quantity of coagulated blood was found between the scalp and the bone of the skull. There was a wound on the right side of the head, on the parietal bone, an inch and a half in length, and penetrating through the scalp to the skull, but no fracture of the skull. There was a quantity of extravasated blood on the opposite or left side of the head. There was a rupture of some branches from the carotid artery inside of the skull. There were two discolorations on the neck, on the left side of the windpipe, apparently occasioned by the pressure of two fingers. The laying hold of the neck might be the occasion of a rupture of the vessels, being more easily made, by preventing the blood from returning. The surgeon, after giving this description, was asked whether, in his opinion, the death was occasioned by the injury proved in evidence. He said that the death might, or might not, have been occasioned by it. The death might have arisen from other causes. An apoplectic fit might have occasioned it. The surgeon had seen blood in the cranium of persons dying from apoplexy. The effusion of blood occasioned the death. "The deceased, when I was first called into him, was not able to see; this was on account of the pressure of coagulated blood on the optic nerve; the occasion of death was the pressure of coagulated blood upon the brain; not able to speak to the *cause* of the rupture in the brain; should think it highly probable that the injury received was the cause of death—it was certainly sufficient to account for it." I should observe, that in this case the Judge was very angry with the witness *for not stating at once that he believed the injury was the cause of death.* And the Judge made game of the witness by telling him to pause; not to commit himself hastily; to take time to consider; and using other such like sarcastic speeches. But the witness might have said that the blows were a sufficient cause to have accounted for the death; but that apoplexy, also, brought on by great excitation and drunkenness, was a sufficient cause, and the appearances within the head would be the same from either (which of the two, however, was the more likely cause, under the circumstances, to have occasioned death, is not so much a medical question as an inference to be drawn by the jury from the evidence of the witnesses); and that the utmost the surgeon could say, would be, "here is a cause sufficient to account for the death; still the death might have been occasioned by another cause, and whether that other cause existed or not, it is quite impossible for me to say."

I may mention, by the way, that in this case a surgeon was called upon in the night, just after the injury was inflicted; but he did not come till the next morning at ten o'clock. When he came, he said that the man was dying, and *therefore* there was no occasion for him to stop. He went away without examining the man's head, though he had been told that the man had received a severe injury upon his head. The Judge told this surgeon that the account did not do much credit to his humanity, and asked if the people in that county had not heads or brains? Upon which the counsel said, "it appears, my lord, that the doctors in this county want both."

In another case of manslaughter, the surgeon proved that there was a swelling under the angle of the jaw on the right side. On removing the integuments from the swelling, there was a considerable extravasation of blood discovered. The surgeon then opened the head, and found a copious extravasation of blood at the base of the skull, which, in the witness's judgment, was produced by a blow under the angle of the jaw; and the surgeon was of opinion that the blow had struck upon the internal jugular vein, occasioning a rupture of a vessel within the skull, giving rise to the extravasation. Some witnesses had sworn that the prisoner had kicked the deceased on the neck whilst he was lying on the ground; and the surgeon was asked in cross-examination, if there was any mark of a foot on the neck. The surgeon said no, but observed that the deceased, having a neckcloth on, the neckcloth would prevent there being any distinct impression of a foot. The deceased was a prize-fighter, and he had been fighting in the middle of the day, some hours before he received the injury which occasioned his death. The fight was at 3 o'clock, and the kick was given at about 7 in the evening; and the counsel for the prisoner endeavoured to make out, from cross-examining the surgeon, that the deceased might have died from some injury received in the previous fight. Fortunately, however, for the ends of justice, the surgeon had not merely examined the head of the deceased, but had examined all his body; and although he found several bruises on various parts of his body, yet he could say, from their yellow colour and general appearance, that they were not recent bruises.

And though it be more a purely medical than a medico-legal point, I will mention, that in a case of manslaughter at Lincoln, the deceased had been killed by a blow on his side with a knife. The surgeon said that he found the deceased bleeding from a wound in his side; the wound was an inch long, through the cartilage of the ninth rib; part of the omentum protruded through the wound two inches, and a little more; the omentum itself was wounded by the knife; it could not be returned through the opening—the opening could not be enlarged with propriety; if the opening had been enlarged in one direction, the diaphragm would have been injured: "I cut off the omentum; I have read of many instances of persons recovering after such an operation had been performed, but I never knew of an instance within my own practice."

I shall now conclude my lecture, and the present course, with the facts of a case which occurred more than twenty years ago, and which I believe is not mentioned in any books of medical jurisprudence. I shall only read you the medical evidence. The question in the case was—whether the deceased had died from the blow of a spade, inflicted by the prisoner, or whether he had died by a kick from the hoof of a horse.

"*Thomas Owen Jones* examined.—The witness is a surgeon, and lives at Henley; was sent for to attend the deceased; got there about half-past four o'clock; Mr. Burman was there before him. Both of them immediately proceeded to examine the body of the deceased. In turning the body, a prodigious effusion of blood took place, with a small portion of the brain issuing from the nostrils. They then ordered the body to be carried up stairs, and it was; there it remained till the following day, when they examined it minutely. On Saturday, the first wound they remarked was one on the left side of the head, about five inches long, extending from the front part of the head towards the back part; along the parietal bone, that above the ear, it was straight, without contusion; it had the appearance of an incised wound, inflicted by an instrument not having a keen edge. There was another wound above that, smaller, he believed—about two inches, or not so much: it was within half an inch of the other, or thereabouts; it was of the same nature as that below—straight and incised. On the right side of the head there were three wounds, two wounds about four inches in length; those wounds were on the parietal bone also; the third wound was lower, upon the temporal bone: these wounds were rather irregular, partaking both of lacerated and incised wounds. There was one on the hind part of the head about two inches and a half long; there was not any tumefaction round any of the wounds, the integuments adhering firmly to the bones; except where the wounds were inflicted, the fracture of the skull was general throughout the right side; it extended along the back part of the head towards the left side; a small portion of the temporal bone came away. He thought those wounds could not be inflicted by a horse. The reason why they could not was, they were all distinct, and the integuments adhering so firmly it was evident that they must have been made by an instrument. If the wounds had been given by the kick of a horse, they would have been in a perpendicular direction instead of a lateral one, if the person lay on the ground at the time the wounds were inflicted. If the deceased had been standing up, all those wounds could not have been inflicted on the head in the manner the witness saw them; he must have fell from the first blow. If the horse had kicked the deceased when he was up and down, they would have been different, he thought; the wounds would have been perpendicular in some measure, and not all lateral. He had no appearance at all of being kicked, or any other wounds on any other parts of his body; there was a very slight discoloration on the breast-bone that he did not think worth notice. If the horse had kicked him on the chest, it certainly would have had other discoloration and appearance. He examined the mare that was near the body of the deceased; he examined her hind feet; there was no blood, none in the least; he looked to find if there was any most accurately. There was no shoe on the off foot behind; there was a shoe on the near foot behind; it was particularly smooth. From the state of her feet, when he viewed them, he did not think it possible for the wounds to be inflicted by that mare. He had no doubt upon that subject from his knowledge and experience.—Q. We understand that the hat was upon the deceased's head when he was first found: look at that hat, and examine it. Would it have been in that state if it had been kicked by any horse? A. In order to produce the wounds I have spoken of, the hat must have been cut.—Q. From your judgment am I to conclude they could not have been made with the hat on the head? A. They could not.—Q. You have no doubt? A. I cannot have any.—Q. Was it possible for the deceased to have put on his hat after having received those wounds you have been describing? A. Oh! no, sir.—Q. Would any of those wounds, or either of them, have killed him? A. Those on the right side would unquestionably have killed him.—Q. Would they not have produced instant death? A. They must necessarily have produced death instantly.—Q. You took the spade from Allibone? A. No, I did not.—Q. Will you look at that instrument? Did you observe any blood at the time? A. Yes, I did.—Q. Will you point out to the jury where it was? [Here the witness took up the spade, and described the blood to have been on both sides, and on the front side florid, near and close under the left tread.] A. The blood under the left tread was very florid.—Q. You say very florid? A. Yes, it was.—Q. As to the other? A. The blood on the back part was not so florid by a great deal; it was absorbed by dirt.—Q. What judgment do you entertain as to the blood there being absorbed? A. It must have been in a fluid state, otherwise it could not have been absorbed.—Q. Look at that instrument (the spade): I desire to know whether, in your judgment, it would not have produced those wounds which you saw? A. It would, or any similar instrument.—Q. You said there was no tumefaction? A. None at all.—Q. If inflicted by a horse-shoe, would there not have been a great deal of tumefaction and contusion? A. By the kick of a horse, it would have produced a great deal of it; there was none by these wounds.

"Cross-examined.—Q. What I understand you to mean by an incised wound is, one inflicted by an instrument with a keen edge, which will not bruise?. A. Yes. And that a wound inflicted by an obtuse instrument will be both cut and bruised? A. Yes.—Q. How long have you been at Henley? A. Seventeen years.—Q. Have you had fractures under your care from the kick of a horse? A. I never have been called to one whose skull had been fractured by the kick of a horse.—Q. Then this opinion is all speculative and conjectural? A. Certainly; I did not see it done.—Q. You never before examined such a fracture, therefore it is conjectural: look at that shoe, and say whether, in your judgment, a similar wound could not have been inflicted with the one as with the other. [Here he showed him the shoe and spade, and asked whether the shoe would not have inflicted a wound similar to one inflicted by the spade?] A. No, it could not when nailed to a horse's foot.—Q. Do you mean to say, none of these wounds were curved? A. They were not, they were irregular; but I must call them straight.—Q. I must ask you whether the edge of the hoof has not a sharp appearance? A. Certainly it might.—Q. Is it capable of giving a wound both lacerated and incised? A. Certainly it would.—Q. You have told us you were not called in till after the body was removed, so that you cannot tell us in what condition the hat was when it was found. Is it impossible to receive a fracture with the hat on, and it not cut? Was it impossible to receive these wounds without the hat being cut? A. I do not think they could without the hat being cut; not wounds of that nature.—Q. Do you mean to swear that they could not but without the hat being cut? A. It is so in my judgment. [Here his lordship observed, that he remembered an instance where it was proved, on a trial at the Old Bailey, that a cut and fracture had been received, and the hat not cut.] Q. Then men may receive wounds on the head of that nature, and the hat not be cut? A. I did think they could not till my lord said otherwise.

Re-examined.—Q. Supposing the body had been kicked while on its legs, would it have produced the wounds you have seen? A. No, it would not.—Q. Could those six wounds have been inflicted without the hat being cut? A. It must have been cut.

Mr. Thomas Burman examined.—Witness was a surgeon, lived at Henley; was present at the time Mr. Jones was at the house of Mr. Booth; examined the body of the deceased with Mr. Jones. Q. In your judgment by what sort of instrument were these wounds inflicted? A. Had I only seen one on the right side, I should have supposed it had been done by the kick of a horse-shoe; but what made me suppose these were not was the slight separation of the integuments from the cranium, and the wounds being distinct; I thought then they could not have been done by the kick of a horse.—Q. Was there more or less of contusion than would have been occasioned by the kick of a horse? A. There would have been greater contusion, and separation of the integuments: these were the appearances which I observed on the integuments, but I don't exactly recollect any other appearances. It appeared to me they were inflicted by an obtuse instrument.—Q. Will you look at that spade. In your judgment, could they have arisen from that? A. More probably from an instrument like that than from the kick of a horse. I have been called in to four cases, and in all of them the wounds were inclined to curve.—Q. What was the shape of these which led you to conclude that they could not have been inflicted by the kick of a horse? A. There was no inclination to curve.

Having now concluded my present course, I may take the present opportunity to observe, that next year, if there be a sufficient demand for the information, it is my intention to give lectures, not only upon what may be strictly called medical jurisprudence, but also upon the laws of the medical profession, and all those legal points which a medical man is likely to meet with in the exercise of his professional practice.—(*Loud and continued applause.*)

SOME ACCOUNT OF A

CASE IN WHICH CHRONIC TUMOR WITHIN THE ABDOMEN

Was produced by a large Accumulation of Contents in the superior portion of the Colon, expanding and rendering useless the Valve of the Cœcum; with the course adopted by Nature for preventing the inconveniences that might thence have resulted.

By John Howship,

Surgeon to the St. George's Infirmary, and Lecturer upon Surgery.

JULY, 1830, I had the opportunity of examining the body of a tall, stout woman, aged 72, who after many months confinement to bed had died, with the usual symptoms of disease in the uterus.

Upon inquiry of those who had attended upon and nursed the patient, I ascertained that she had complained of constant pain in the bowels for the last twelve months, with a sense of swelling, or tumor, within the abdomen; which swelling was very large, and, as the nurse said (who had frequently examined it), felt uniformly soft and yielding, like a sponge, being principally situated in the right side; although, in changing the position in bed, she had found it, like a great weight, fall from side to side. If, for instance, she lay on the right side, a part of it seemed to roll over to that side; and if she turned over to the left side, she immediately felt the heavy mass fall over to that side, with a dragging sensation hardly to be borne.

The frequent magnitude of this tumor within the abdomen was said to have equalled, or even exceeded, that of a pregnant woman at her full time; and this remark the patient herself had repeatedly made. After death, the existence of tumor in the right hypogastrium was sufficiently manifest to the eye; it now felt soft, flaccid, and pulpy.

Laying open the cavity of the abdomen, I found this tumor was caused by a considerable enlargement of the whole of the colon, but particularly, as it appeared, of the cœcum and adjacent part of the great intestine. The peritoneal covering also, of these parts of the intestine, was so relaxed and elongated, that the weight of the parts, when the body was laid on the left side, brought the cœcum down to the middle of the abdomen.

Passing a ligature round the middle of the transverse arch, I removed the cœcum and superior portion of the colon, washed out the contents—a very large quantity of stiff and pulpy, yet well-digested, fæculent matter—and then tying the lower opening, inflated the intestine from the ileum.

In this operation I was repeatedly struck by observing, that although the air passed down freely enough into the cœcum and colon, not any appeared to escape back again into the small intestine. This circumstance I could the less understand, as it appeared that the portion of gut, where the cœcum is parted off from the colon, was so enlarged, that the valve there situated was in all probability useless. The inflation, however, was completed, the ileum tied up, and the preparation hung up to dry.

The next day, expecting to see it dry and expanded, I found it damp, flaccid, and collapsed. The tied end of the ileum was therefore cut off, the intestine moistened in warm water, and again inflated, when it seemed that the air escaped, although I could not discover where, till Dr. P. Robertson, who was with me at the time, suggested the placing it under water; we then perceived the bubbles of air rise from a minute ulcerated opening in the cœcum, which, neither thickened nor discoloured, would otherwise have escaped detection. This secured by a ligature, the intestine retained the air, and was soon dry.

On subsequently cutting an opening into the cœcum, I was so fortunate as to find that the peculiar and very curious state of the parts was well demonstrated.

The original valve of the cœcum, as I had anticipated, was so expanded and drawn aside, by the progressive enlargement of the cavity of the gut, as to have been, for a long time, entirely inefficient; and the consequence was, that nature, ever watchful over the movements of the animal machine, had contrived (the first valve failing) to form a second, by the readiest and most ingenious device imaginable—that of simply drawing the inner membrane of the gut across the termination of the ileum, in the cœcum.

The dried preparation, which is preserved in my collection, demonstrates the enlargement of the cœcum and colon, the expanded figure of the original valve of the cœcum, and the position and appearance of the new valve closing the opening from the ileum; an opaque middle line marking the junction of the two portions of the valve.

Observations.—In some practical observations on the diseases of the lower bowels, published several years since, I endeavoured to state clearly what I had seen, in proof that permanent lodgments occasionally take place within the cells of the colon, notwithstanding the apparently free operation of active purgatives. This fact I was desirous to state clearly, because it is a point upon which I had myself been extremely sceptical; but the proofs there adduced are, I apprehend, so conclusive, as to render any further evidence of the fact unnecessary.

In the present case, however, the condition of the intestinal canal was such as was altogether new to me, and such as I could not have anticipated. The colon presented no cells or recesses, nor any portion of the great intestine a point of contraction, either from disease or permanent spasm; the whole of the colon and rectum being found, on examination, perfectly free, and moderately occupied. In fact, although the usual habits of the bowels had been those of confinement, for the last fortnight of the patient's life, they had been constantly and copiously relaxed; and notwithstanding this relaxation, the same large tumor appeared to remain, which had for a twelvemonth before induced various feelings of uneasiness, and frequently those of great distress; and this, notwithstanding there must at all times have been a free way through the midst of the mass so often as the bowels were relieved, either by spontaneous action, or the operation of medicine.

21, Saville Row, July 25, 1831.

ATMOSPHERICAL CHANGES DURING THE PREVALENCE OF THE LATE "INFLUENZA."

To the Editor of the London Medical Gazette.

SIR,

THE late "influenza" was, perhaps, modified or occasioned by atmospherical changes, or, as Sydenham would have said, by "the constitution of the year." If the following sketch of the state of the weather be worthy of your notice, it will find a place in the columns of your journal; for it may, in some degree, account for that excitement of the air-passages of the human body which has so lately prevailed, and been denominated "influenza;" and it may probably explain the cause of that mental and bodily debility which accompanied or followed that singular affection. It will be seen that the respiratory mucous membranes were first parched and afterwards moistened by the changes of the atmosphere, and that the whole body was first excited and then depressed by the same causes. Such circumstances will always control and direct the conduct and practice of a thoughtful man.

During the month of May the weather was unusually variable; the barometer rising and falling suddenly, and the thermometer standing one day at 80 deg. Fah. and a few days afterwards at 32 degrees; the wind was prevailing steadily from the north-east. The month commenced with heavy clouds, murky storms, copious precipitations of rain, and remote thunder; the thermometer ranged from 55 to 60 degrees, and the wind blew for a few days from the west and then shifted round to the north. This condition of atmosphere was succeeded by an over-cast sky, with intervals of sunshine, a keen cutting wind from N. E., frost, ice, and snow; the thermometer rapidly sinking to 32 degrees. Great-coats, which had been thrown off, were resumed, and the fire-hearth became acceptable. By the middle of the month the weather cleared and became warmer, the sky brightening; a high blustering wind prevailed from N.E., drifting before it clouds of dust from the roads, the thermometer ranging with celerity between 62 and 32 degrees. During the severest interval of this weather ice was formed upon the ponds at night, the early fruits were partially blighted, the petals of the new-blown rose dropped from the shrub, and the hedge-row box seemed, as it were, singed—"the frosty air-burnt froze." The north-east wind was heightened into a gale, by which several vessels were drifted from their moorings in the pool of the Thames; and a heavy atmosphere, a calm, and a soaking rain, followed. From this time (20th) to the end of the month, either a grey mist, with a hot sun, 80 degrees, occasionally gave way before a gelid wind 40 degrees from N. E., the usual forerunner of storms and thunder, or the soil became dry and dusty under a light sunshine with fleeting cloudlets, or a sultry sun gleamed through a hazy atmosphere. The thermometer stood at night 32 degrees, and in the day varied between 50 and 80 degrees. A pelting rain, N. 50 degrees, and a fine, clear, breezy day, W. 60 degrees, closed the month. Then followed the month of June, remarkable only for a high tem-

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perature, a singular humidity of air, a soft wind from the S.W., and a bright hot summer's sun. During the first days of the month, the sunshine was interrupted by a light rain; once by clouds, rain, and a gale from north 40 degrees; and occasionally by transient hail-storms and remote thunder. The thermometer ranged between 40 and 80 degrees, but it generally varied between 75 and 60 degrees; and this was the month in which the influenza, so well described by Dr. Burne (see Med. Gaz. July 2d, 1831), made its appearance.

All things are intelligible if we have the patience to investigate them; and although the influenza be called specific, epidemic or adventitious, it will yet be found to depend upon very simple and obvious causes. If the cholera morbus appear in England, observation, diligence, meditation, and humane experiment, will reveal its nature, develop its mode of action, discover the appropriate remedies, and ascertain the conduct of cure. Physiology, pathology, and meteorology, well applied, will supply much more practical knowledge than the idle satisfaction of asserting that many diseases are *specific* diseases.

J. A. HINGESTON.

1, Finsbury-Place, South,
July 28, 1831.

ANALYSES & NOTICES OF BOOKS.

"*L'Auteur se tue à allonger ce que le lecteur se tue à abréger.*"—D'ALEMBERT.

Medico-Chirurgical Transactions. — Vol. XVI.—Part II.

THE more regular appearance of these volumes now than formerly, shews an increased degree of activity on the part of the members; yet we fear the Society is kept up only by the personal efforts of a few—the profession at large take but little concern in it; and it would be uncandid to say that the present volume of papers, which we have a right to infer are the last which were read, is calculated to make us feel deeply the loss of those we have missed by not being present at the meetings. By far the most important communication —that of Dr. Lee, on uterine phlebitis— we have already noticed very fully in two of our previous numbers. One article will embrace all that is interesting in those which remain.

Remarks on Omental Hernia, with Cases. By JOHN MACFARLANE, M.D. Member of the Faculty of Physicians and Surgeons of Glasgow, Senior Surgeon to the Glasgow Royal Infirmary, and Senior District Surgeon to the City Poor. Communicated by ALEX. COPLAND HUTCHISON, F.R.S. L. and E. Vice-President of this Society.

This is a good paper, and shews the writer to be an intelligent and discerning surgeon: nevertheless it is much too long, being diluted with many observations fitter for the lecture-room than the halls of a learned society. The chief object of the author appears to be to point out the evils which result from irreducible omental herniæ, and the advantages of setting them free where this can prudently be done. To these points we shall confine ourselves.

"In irreducible herniæ of large size, whether intestinal or omental, the patient is not unfrequently subject to smart attacks of colic, with pains in the tumor, after taking a hearty meal. When the rupture consists wholly of omentum, the pain commences almost immediately after eating, but when of intestine, the uneasy feelings are longer in appearing, and seem to take place only when the contents of the intestinal canal are passing through the tumor. Besides these symptoms, an irreducible epiplocele is often accompanied by severe dragging or twitching at the stomach, and by repeated vomitings, in consequence of the stomach being compelled to follow the motions, communicated to the *fixed* omentum, by the intestines and abdominal muscles. These symptoms are, also, most urgent after meals, because, from the distention of the bowels, the stomach is pushed up towards the diaphragm, and the omentum put more completely on the stretch, and also rendered more convex externally by the pressure of the intestines."

"As the stomach and colon become accustomed to the restraint arising from this unnatural fixture of the omentum, we occasionally find that the urgent symptoms gradually diminish, or even altogether disappear. This fortunate result has been, however, less frequent in my practice, than from the assertions of Pott, Boyer, Cooper, and other writers on this subject, I was led to expect. It cannot be denied that the membranous viscera of the abdomen are less firmly fixed in situ, admit of greater freedom and latitude of motion, and may, therefore, be more completely and extensively displaced from their natural position, than those of a different structure. The mobility of the stomach, and that part of the colon to which the omentum is attached, is well known; yet there are limits, beyond which these parts cannot be displaced, without deranging, or altogether impeding, their functions, and of course exciting painful, and even dangerous symptoms.

"When the distention of the abdomen is moderate, an irreducible epiplocele may cease to produce any disagreeable symptoms; but when the stomach is full, the bowels constipated, and unusually distended with flatus, or fæces, when much straining of the abdominal muscles occurs, we cannot fail to meet with very distressing symptoms. It is the liability of the abdomen and its contents to great and often to sudden variations in size, even in healthy individuals, that enables us to explain the repeated recurrence of these painful paroxysms. They are generally more urgent when the omentum is suddenly, than when it is slowly, put on stretch; yet even in the latter state, they are sometimes marked and severe.

"A few years ago, a woman about twenty-eight years of age, who came under my care for a different disease, had a considerable-sized, irreducible, crural epiplocele of the left side. She had experienced for several years repeated attacks of dragging at the stomach, vomiting, and constipation; but these were mild when compared with the violent, incessant, and distressing attacks to which she was subject after she became pregnant. As the abdomen became more and more prominent, the severity of the symptoms increased. During the last two months of pregnancy, she was constantly confined to bed, and only experienced relief when she lay on her left side, with the trunk bent forward, and the thighs drawn up to the abdomen. She could not extend herself in bed, nor assume an erect position, without immediately exciting vomiting and pain at the stomach. The relief she experienced after delivery was very decided.

"When an inguinal epiplocele has been long irreducible, we sometimes find that the omentum becomes so altered in structure as to produce, by pressure and irritation on the spermatic cord, a diseased state of the testicle, with or without effusion into the tunica vaginalis.....

And again:

"We are advised by Hey, Scarpa, Boyer, Richerand, and other writers, not to separate the adhesions which the neck of an omental rupture may have contracted with the neighbouring parts, particularly when the disease is of long standing, but to cut off the prolapsed portion, and allow the rest to remain in the wound. Of these authors, some appear to be influenced by the fear of hæmorrhage taking place into the abdomen, from the cut edge of the omentum; whilst others maintain, that the hernial aperture is in this way effectually plugged up, the return of the disease prevented, and a firm surface obtained for granulations. Sir A. Cooper, even, in his valuable work on Hernia, speaks (at page 32) of returning the omentum into the abdomen, with its cut edge to the sac, so as to form a plug, and produce a closure of the opening.

"By permitting the divided omentum to remain fixed at the mouth of the sac, a temporary closure of the aperture will be effected, and the immediate descent of any portion of intestine or omentum for a time prevented. But, on the other hand, besides the danger of the intestines adhering to, or becoming entangled with, this fixed band of omentum, there is the risk of a second hernia forming at the same aperture. When the abdominal muscles are called into powerful action, the fixed omentum serves as an inclined plane, along which the intestines glide, and by which the impetus will be more effectually directed to the old hernial aperture, than to any other part of the abdominal parietes; and, of course, the risk of a secondary tumor forming be greatly increased.

The baneful effects on the stomach which, in nine cases out of ten, will be found to accompany the permanent adhesion of the omentum to the inferior hernial openings of the abdomen, are also entitled to our consideration. Sometimes the function of the stomach is only at times impaired, but in other cases this viscus has been dragged from its natural position, even below the middle of the abdomen, digestion destroyed, the most painful symptoms excited, and bad health ultimately induced. In addition to what has been already stated, I select the following cases out of many on record to prove this fact.

"M. Guerin saw, in the Hôpital de la Charité, a man upon whom the operation for strangulated hernia was performed, and a portion of gangrenous omentum removed, the healthy part being allowed to remain in the wound. The wound was healed in five weeks, but the patient continued to vomit after every meal. He was at last reduced to the necessity of eating in bed, with his legs drawn up to the abdomen, so as to relax the omentum, and prevent its dragging influence on the stomach.

"M. de la Faye opened the body of a woman in 1740, who had been operated upon for hernia several years before. From this time functional derangement of the stomach took place, and she ceased to enjoy good health. The omentum was adhering to the inguinal ring, and the stomach, which was dragged almost perpendicularly, had lost its natural shape, and assumed the form of one of the large intestines.

"Vesalius relates, in his work on anatomy, an instance of great and unnatural displacement of the stomach, from an irreducible epiplocele of from four to five pounds weight; and Dr. Robert Lowis, Physician, in Edinburgh, met with a case somewhat similar in the year 1722. The patient was sixty-three years of age, and had been subject, from his youth, to an irreducible epiplocele, which gave rise to a variety of urgent symptoms. On dissection, the greater part of the omentum was found in the left side of the scrotum, and attached to the testicle; the stomach was attenuated, inflated, and greatly displaced; the pylorus lay obliquely downwards, as far as the right side of the umbilicus, and the œsophagus entered the stomach at an acute angle.

"The necessity of returning the omentum into the abdomen whenever it is practicable, that it may regain that position in which its attachments to the stomach and colon would naturally place it, is, I think, sufficiently obvious: I must, therefore, dissent from those authors who maintain, that the permanent adhesion of the omentum to a hernial sac is productive of little inconvenience, and that, at first, although the stomach and colon may have their functions interrupted, these parts soon become accustomed to this restraint, and cease to feel its influence. It happens, on the contrary, in many instances, that the symptoms, instead of yielding, become daily more distressing, and continue to harass the patient with increasing severity during the remainder of life.

"Besides, we sometimes find, that the disorganization to which the irreducible omentum is liable, is not confined to the tumor, but extends into the abdomen. The abdominal portion of this membrane is, from this cause, as well as from the stretching, pressure, and irritation, to which it is subjected, apt to become extensively and incurably diseased. I have seen one case, and a preparation of the diseased parts of another, in which the omentum within the abdomen, as well as the portions contained in the irreducible ruptures, had lost every vestige of its natural structure, become exceedingly bulky, indurated, and tuberose, and produced death by exciting ascites."

Some Considerations with respect to the Blood, founded on one or two very simple Experiments on that Fluid. By Benjamin G. Babington, M.D. F.R.S.

There is something rather forbidding in the title of this paper, with which impression the matter is not much calculated to do away. The chief object of these "Considerations" is to shew that there exists in the blood a homogeneous fluid, to which it is proposed to apply the name of liquor sanguinis; that this, after a time, separates into two parts, viz. fibrine and serum, and that these do not exist as such in circulating blood, but that they separate when, and not until, the liquor sanguinis ceases to be under the influence of life—the liquor sanguinis bearing a striking resemblance to chyle.

"Experiment I.—Let blood be drawn in a full stream from the vein of a person labouring under acute rheumatism into a glass vessel, which should be filled to the brim. By close inspection a colourless fluid will be immediately perceived around the edge of the surface, and after a rest of four or five minutes, a bluish appearance will be observed forming an upper layer on the blood, which is owing to the subsidence of the red particles to a certain distance below the surface, and the consequent existence of a clear liquor between the plane of the red particles and the eye. Let now a spoon, previously moistened with water, be carefully immersed into the upper layer of liquid by a gentle depression of one border. The liquid may thus be collected quite free from red particles, and will be found to be an opalescent and somewhat viscid solution, perfectly homogeneous in appearance. By repeating the immersion we may collect this fluid in quantity, and transfer it to another vessel. That which I employed is a bottle holding about 180 grains, of globular form, with a narrow neck and perforated glass stopper."

"EXPERIMENT II.—Take two similar tall glass jars or phials, each capable of holding about four or five ounces, and let one of them be half-filled with olive oil; draw the blood of a healthy subject into each. That which flows through the oil will be found to have a layer of liquor sanguinis on its surface, which will form a buffed crust, while there will be none upon that which is received in equal quantity, and in other respects under the same circumstances, into the empty jar. This experiment will not always succeed, for blood sometimes coagulates so quickly, even though it pass through oil, that no buffed crust is formed. It is enough, however, for my purpose, that there is frequently a very evident difference. If this experiment be performed on blood disposed to exhibit a buffy crust, that which is formed under the oil will be twice or thrice as thick as that formed in the empty jar. The reason that blood is buffed when drawn into oil we need not now consider. It is, indeed, sufficiently evident."

Observations on the Symptoms attending the Change of a Circumscribed Popliteal Aneurism into the Diffused State; with some Particulars of an Aneurism of the Aorta that burst into the Œsophagus.

By SAMUEL COOPER, Surgeon to the King's Bench, the Bloomsbury Dispensary, the Fleet Prison, and his Majesty's Forces.

Mr. Cooper observes, that the pulsation of aneurismal tumors sometimes undergo sudden diminution, or even cease altogether, without any of those changes resulting which lead to spontaneous cure. The object of the paper before us is to illustrate this point, and shew that, on the contrary, such phenomenon may be owing to causes which are productive of a great increase of danger. The circumstance alluded to is rupture of the aneurismal sac, the integuments remaining entire. The change of aneurism, from the circumscribed to the diffused state, has been described by most writers on surgery, but Mr. Cooper thinks there are certain circumstances connected with it which require more attention than has been bestowed upon them.

In such cases, several causes tend to render the pulsations weaker and weaker, till they are at length extinguished. Thus,

"1st. The more or less impeded state of the circulation, that takes place in the limb, as soon as a considerable quantity of blood has been injected into the cellular tissue. And, in order that the extravasation may attain the degree necessary for the full production of this effect, a certain time is obviously requisite; the limited size of the opening in the sac, and perhaps also sometimes the particular situation of it, away from the main current of blood, preventing the effusion from becoming all at once copious and extensive. By degrees, however, the quantity of blood in the cellular membrane increases; and then its pressure not only creates a great deal of irritation, but actually interferes with the regular supply of blood and nervous influence to the limb. Hence, the alarming fall of temperature in the foot, and the well-known tendency to gangrene, consequent to the change of a circumscribed popliteal aneurism into a diffused one.

"2dly. Another cause, that has a powerful effect in gradually putting an end to the pulsations, is the increase in the quantity of coagulated blood and fibrine in the sac; the inevitable result of the stream of blood through it becoming more and more retarded, in proportion as the obstruction of the circulation in the leg is augmented.

"I have been led into these reflections by the observation of a popliteal aneurism, the history of which is in several respects highly interesting to the practical surgeon.

"In the beginning of last April I was desired by Mr. Nicholson, of Coburg Place, in the Borough Road, to visit Mr. Lucas, an organ-builder, in the Waterloo Road, a large robust man, of gouty habit, about 48 years of age. I found him confined to bed, with an extensive and prominent swelling at the back of the left knee. The tumor not only filled the ham, but extended over the sides of the condyles of the thigh-bone towards the kneepan, and reached some way under the gastrocnemius muscle. Its pulsations were remarkably strong, and equally manifest both to the sight and the touch. The integuments, on the back part of it, were at one point somewhat red and inflamed; the foot torpid; and the limb, from the knee downwards, of great size, from the effect of œdema. It is curious that the disease should have existed five years, without the patient having any suspicion that his lameness arose from something more serious than a gouty, or rheumatic affection of his knee; and, notwithstanding the present magnitude of the swelling, he had continued to follow his trade, with scarcely any interruption, until the day when I first visited him.

"As the disease appeared urgent, I advised Mr. Lucas to let me tie the femoral artery, and explained to him the hazard of delay. His answer was, that he should be prepared for what had been recommended in a week or ten days, at the expiration of which he should have completed some pressing engagements in business.

"Three or four days after my first visit, I learned from Mr. Nicholson, that the patient, instead of having been able to get up and attend to the engagements in question, had been severely attacked by gout in the right foot and both wrists. This additional complaint lasted about a fortnight, and, on its subsidence, it was remarked, that *the throbbing of the aneurismal tumor had undergone a considerable diminution, without any material alteration in the general appearance of the swelling and limb, except that a slight purple discolouration was perceptible a little above the internal malleolus.* The numbness of the foot was also somewhat increased.

"Five days after the period, when the pulsations of the tumor had been observed to have declined, I visited the patient again, and found the above-mentioned discolouration very manifest, though not of much extent, and somewhat resembling an ecchymosis; *the foot had become all on a sudden extremely cold;* there was some sensibility about the ankle and instep, but none in the toes; and no throbbing whatever could be perceived in the swelling. In other respects the limb remained without alteration.

"It was now a question in what way, and from what cause, the total stoppage of the pulsations had arisen? From the time that I first heard of the reduction of their force, I hoped that this circumstance might be connected with changes, which had lessened the transmission of blood into the aneurismal sac, and I was still inclined to believe, that the complete stoppage of the throbbings depended upon such transmission having been effectually cut off. As there had been *no change in the shape of the tumor, no diminution of its firmness,* and *no material increase in the swelling of the leg,* I entertained no suspicion of a rupture of the sac. However, the sudden fall in the temperature of the foot, *the increased frequency of the pulse,* and the risk of gangrene, raised doubts in my mind, whether the view which I had taken might not be erroneous. I requested, therefore, that a consultation might be held with Mr. Lawrence the same afternoon (Friday), and directed the leg and foot to be in the meanwhile fomented.

"We met at half-past six o'clock in the evening. Mr. Lawrence, having applied his ear close to the swelling, said that he could distinguish a sound in it like that produced by the action of a pair of bellows; that this sound could proceed from no other cause than the entrance of blood into the aneurismal sac; and that, on this ground, he was in favour of tying the femoral artery. The same sound was also heard by myself and two other medical gentlemen present. No doubt therefore now existed concerning the passage of a certain current of blood yet into the aneurism. At the same time, *the foot and lower part of the leg were remarked to be warm,* whereas they had been extremely cold in the morning. Although blood still entered the sac, I conceived that it might only be doing so in a very diminished quantity, and that possibly changes might be going on which would soon completely stop the influx of blood into the aneurismal cavity. Indeed, as no suspicion arose of a rupture of the sac, it seemed to me impossible to account for the cessation of pulsations on any other principle. At my suggestion, therefore, the operation was put off.

"As there was no improvement in the state of the limb on the following day (Saturday), Mr. Lawrence very properly urged the operation; but the patient could not be prevailed upon to submit to it.

"On Sunday morning, at ten o'clock, directly after my return from a professional engagement, which had taken me into the country on Saturday, I visited Mr. Lucas again, and finding that the risk of gangrene had not been lessened; that the tumor continued as tense and large as ever; that the swelling of the whole leg was undiminished; that the bellows sound was yet audible; that the apparent return of the natural heat in the foot and leg, noticed on Friday evening, had been owing to the assiduous use of hot fomentations; and that *the frequency of the pulse was increasing;* I persuaded the patient to let me tie the femoral artery, which I immediately did with the assistance of Mr. Nicholson and my nephew, Mr. George Cooper. The vessel was secured with a single ligature of strong fishing-line silk, introduced under it with an aneurism needle, without any raising of it up with the finger, or any unnecessary detachment of it from its surrounding connexions.

"As soon as the ligature had been tightened, the bellows-like sound in the swelling entirely ceased. The wound was dressed in the common manner. Without going into superfluous details, suffice it to mention, that the tying of the artery did not prevent gangrene, which shewed itself in a very unequivocal form on the fifth day after the operation, when it became absolutely necessary to amputate above the knee. The pulse was then 130, the restlessness and anxiety rapidly increasing, and the skin of a considerable portion of the leg black, or livid. The bone was sawn through about an inch and an half below the ligature on the femoral artery, which was observed not to bleed. The day after the amputation, the pulse had fallen to 110, and every thing went on so favourably afterwards, that, in the course of six weeks, the patient's recovery was complete.

"The amputated limb was taken to the house of Mr. Hooper, surgeon, in the London Road, who had kindly given his assistance in the operation, and the state of the aneurism was carefully examined. The sac, which was of unusual size, was filled with coagulated blood. As a waxen composition, thrown into the popliteal artery, readily passed amongst the coagula in the sac, that vessel must have retained a free communication with the latter part. The lower and most deeply situated portion of the sac, under the gastrocnemius muscle, was found to have given way, and the inter-muscular cellular membrane was copiously injected with extravasated blood down to the very heel, some of it actually lying on each side of the tendo Achillis."

The preceding case illustrates forcibly the danger of delaying to tie the artery when the aneurism is of considerable size. The doctrine of waiting for the dilatation of the anastomosing branches, is now, fortunately, almost abandoned, experience having demonstrated that the evils resulting from the increase in the tumor far outweigh those expected to arise from increased arterial communications. The case also shews how little change is sometimes produced in the external appearance of the parts by the rupture of the sac, and it also shews the utility of auscultation in ascertaining whether blood still flows into the tumor.

The case of aneurism of the aorta, related by Mr. Cooper, is one in which the remarkable circumstance occurred, that an individual survived nearly two months after a communication had been formed between the aneurismal sac and the œsophagus, following in this interval the laborious trade of a wheelwright.

Case of Axillary Aneurism, successfully treated. By T. CROSSING, Esq. Surgeon, Devonport. Communicated by BENJAMIN TRAVERS, Esq. F.R.S.

Case of Axillary Aneurism, successfully treated by Tying the Subclavian Artery. By CHARLES MAYO, Esq Surgeon to the County Hospital in Winchester. Presented by Mr. STANLEY.

These cases are important, as adding two to the very limited number of instances in which ligature of the subclavian artery has proved successful. In Mr. Crossing's case, the ligature remained an unusually long period, not coming away till the eighty-fifth day. On another point he observes,

"In conclusion, I would recur to what I have stated, in describing the operation, respecting the existence of a gland immediately over that part of the artery which was tied. This, I have reason to believe, is scarcely a deviation from ordinary structure, as in not less than a dozen subjects, whom I have examined, the same substance was found precisely in the same situation. I would therefore beg leave to ask whether, in most cases, this gland may not prove to us a better guide than the scalenus muscle; because from its being seated immediately over that portion of the artery which is usually tied, we are thus at once directed to the vessel, and enabled to separate it from its contiguous cellular tissue to less extent than by any other mode. At all events, should the gland be occasionally found wanting, the muscle will still remain as useful to us as ever."

Mr. Mayo, in reference to his case, remarks,

"I am happy to add one to the few instances confirming the propriety and success of this operation; and there can be no doubt that its failure has, in most instances, been owing to the advanced stage of the disease, when the operation was undertaken; the proximity of large branches to the point where the ligature is applied, as stated in my former case, may also be a cause of failure, which also must be increased by delay, as we may conclude that they will acquire a greater magnitude from the obstruction of the aneurism beyond. It strikes me that this objection may be somewhat avoided by applying the ligature upon that part of the artery which lies upon the rib, and as far as possible from the scaleni; for which reason I can see no necessity for exposing the edges of those muscles, as some inculcate; neither can I see the necessity of dividing the sterno-cleido mastoideus, which Mr. Key so strongly recommends in the valuable observations attached to his successful case in the thirteenth volume of the Medico-Chirurgical Transactions."

Observations on the use of Tobacco as a local application in Gout, &c. By JOHN VETCH, M.D. &c.

This paper is extremely short. It is now twenty years since Dr. Vetch made extensive trials of all the known narcotics in the treatment of purulent ophthalmia. All of these failed, with the exception of tobacco, and the doctor now recommends its use "in cases of acute migratory inflammation, and especially when it attacks the joints, testicle, or sclerotic coat of the eye." The infusion of the Pharmacopœia is sufficiently strong, and the only precaution required is not to apply it near the stomach, unless the production of nausea be desired.

History of a Case in which, on Examination after Death, the Pancreas was found in a state of Active Inflammation. By WILLIAM LAWRENCE, F.R.S. President of the Society.

Cases in which any connexion between symptoms during life and morbid appearances of the pancreas have been traced after death are so rare, that the following instance of this kind becomes interesting.

"I saw," says Mr. Lawrence, "in consultation with a physician and with the regular medical attendant of the family, a lady about twenty-one years of age, who had been delivered a few weeks previously of her first child. She had been very weak and excessively pale during the latter part of her pregnancy, and she became still more so after delivery. Her state and symptoms were like those of persons who have lost large quantities of blood; and her medical attendant considered that there was a defect in the process of sanguification. Under this view of the case, which was adopted by a physician who saw her soon after her confinement, cordials and stimuli, both medical and dietetic, were resorted to. No advantage resulted from this plan, and another physician was called in, who recommended calomel and opium, on the idea that inflammation had taken place in the chest, and that effusion had probably been the consequence. I saw her about thirty-six hours before death, when no hope of recovery could be entertained. She was excessively pale, with a rapid feeble pulse; hurried breathing; some fulness and uneasiness on the right side of the abdomen.

"I learned afterwards that this lady had been most singularly troubled by thirst during her pregnancy; and that her mother, alarmed by her drinking cold fluids in large quantity, had represented to her that she feared the circumstance might prove injurious to the child. She had also suffered much from pain in the epigastric region, which was sometimes so severe as to oblige her to retire to her own apartment. In mentioning this circumstance, her mother drew her hand across the abdomen in the seat of her daughter's sufferings, and she pointed exactly to the situation of the pancreas."

"*Examination, thirteen hours after death.*—The body had not lost its heat; the internal parts were warm to the touch.

"The skin was universally and extremely pale.

"No blood escaped on making the incisions necessary for exposing the abdomen and thorax, and for sawing round the skull.

"The membranes lining the abdomen and thorax, and the viscera contained in those cavities, excepting the pancreas and spleen, were extremely pale, and almost bloodless. The appearance was like that observed in persons who have died of hæmorrhage, or under the state described by the term anemia. The liver and kidneys were pale, and the several portions of the alimentary canal quite white, without any traces of blood in them.

"The heart was pale and rather large; its cavities and the contiguous large vessels contained some fluid of watery consistence, about the colour of red wine, and small portions of soft coagula. The coronary vessels contained no blood. The muscular substance of the heart was pale and rather flaccid; the structure of the organ in other respects was natural. The lungs were healthy, except that frothy fluid escaped on cutting into their posterior part. The cellular texture around the pancreas and duodenum, the great and small omentum, the root of the mesentery, the mesocolon and the appendices epiploicæ of the arch of the colon, were loaded with serous effusion. The fluid, which was transparent, bright yellow, and of watery consistence, ran out in large quantity on cutting into the parts above mentioned, which were distended in some places to the thickness of two or three inches.

"The pancreas was throughout of a deep and dull red colour, which contrasted very remarkably with the bloodless condition of other parts. It was firm to the feel externally; and when an incision was made into it, the divided lobules felt particularly firm and crisp; the texture was otherwise healthy. The part was left wrapped up in a cloth for nearly forty-eight hours after its removal from the body, the weather being then very cold. At the end of this time the hardness was gone, and the gland even appeared rather soft.

"The spleen was rather large and turgid, livid externally, brownish red internally, and somewhat soft in texture.

"The surface of the dura mater, covering the cerebral hemispheres, was lined in the neighbourhood of the falx with a very thin, soft, and almost mucilaginous layer of light red tint; it could be scraped off with the handle of the knife, leaving the membrane of its natural appearance. There was slight serous infiltration of the pia mater. The blood-vessels of the brain were moderately full. The distention of the cellular membrane by serous effusion in this instance was analogous to the œdematous swelling which often occurs round other parts when actively inflamed.

"The pancreas is not unfrequently found after death, as it was in this case, preternaturally hard; and I suppose that the gland has been in this state in the numerous instances in which we hear and read of its having been schirrhous. Although I do not know on what this hardness depends, I have never considered it as a morbid condition; because it occurs in individuals who have died of other diseases, without any symptoms referable to the pancreas; because the structure of the part is perfectly healthy in all other respects, and because the hardness soon disappears after death, as it did on this occasion."

[In some of the copies of the present article, at p. 588, *for* "which we have a right to infer are the *last* which were read," *read* "the *best* which were read;" and at p. 590, col. 2, after "with which," &c. insert "*impression.*"]

DR. GREGORY AND "M.D."

To the Editor of the London Medical Gazette.

Sir,

By the last number of the Gazette, I perceive that my old and indefatigable enemy, "M. D." is again the field, but, I regret to add, without having profited by the good advice which I formerly gave him. He still preserves his favourite *incognito*, not recollecting, that if his facts are true, and his arguments sound, he will have every thing to gain, and nothing to lose, by the avowal of his name. Nothing would gratify me more than to break a lance with so redoubtable an opponent, but I cannot consent to enter the lists with him, unless he withdraws his vizor, and meets me on equal terms. Until that event shall take place, (of which, however, after my former calls upon him, I have but slender hopes,) his *facts* will obtain that degree of credit which the world usually attaches to anonymous authority, and as for his arguments—they may safely be left to contradict one another.

I have the honour to be, sir,
Your very obedient humble servant,
George Gregory.

31, Weymouth-Street,
July 30, 1831.

ACADEMY OF MEDICINE, PARIS.

Report on the subject of Cholera.

At the meeting of the 26th ultimo, M. Double presented to the Academy of Medicine an elaborate report on cholera morbus, which had been drawn up in consequence of the minister having required their opinion as to the nature and treatment of the malady. M. Double began by observing, that in order to give a good account of an epidemic, it was necessary to remove from the immediate scene of its ravages—as, on writing a history, it is essential that the narrator should not have been a party in the transactions recorded; a position, however, which leads to the inquiry whether Tacitus did not live amid the scenes he describes, or whether Hippocrates and Sydenham described epidemics on hearsay. The reporter proceeded to enumerate the symptoms and postmortem appearances, but as, on a careful perusal of these, we find nothing to add to our Epitome, (Gazette, No. 186) we shall not trouble our readers with it. It is inferred, that the cholera of the ancients, especially as described by Aretæus—that of India and Europe—are but one identical disease, slightly, and but slightly, modified under different circumstances. In conclusion of this part of the report, we are told that "cholera is a complex, variable, and capricious affection, characterized by a general diminution of nervous function, combined with a particular *catarrhal* state;" while we are informed, that, though there be no uniform method of treatment applicable to every case, the general indications, nevertheless, are—1, to restore the warmth of the skin; 2, to combat the catarrhal affection; 3, to renovate the nervous energy. More of this report is promised; but from the specimen before us, we have no very sanguine expectations of its proving valuable.

EXTRACTS FROM JOURNALS,

Foreign and Domestic.

ON THE EXISTENCE OF HYDROCYANIC ACID IN CHEESE.

Dr. Witling has undertaken a series of experiments to ascertain the nature of the changes which occur in some kinds of cheese: the results of his investigations are the following. 1st. If cheese is well sweated, sufficiently salted, and dried at a moderate temperature, it never acquires poisonous properties. 2d. Its fermentation and alteration is in direct proportion to its moisture, especially when it is kept in a close place. 3d. When exposed to the action of water, and to the sun, in a few days it gives out an ammoniacal odour. In this state, if treated with alcohol, this liquor yields on distillation some traces of hydrocyanic acid; it appears that this acid is united to the ammonia in cheese which has become changed. 4th. At a more advanced period of the fermentation, no trace of this acid is to be found. It therefore appears the deleterious property of some kinds of cheese is owing to the formation in them of hydrocyanic acid.—*Journal de Chimie Médicale.*

CASE IN WHICH SEVERAL LEECHES WERE SWALLOWED.

Dr. Schnuhr reports the case of an individual who, while in the act of bathing, accidentally swallowed several leeches. The immediate consequences were intense burning of the stomach, loss of appetite, sickness at stomach, and frequent discharges of blood by vomiting, with which one of the animals was ejected on the third day. Pale and emaciated, he applied for relief on the eighth day. The physician directed him to take a saturated solution of muriate of soda, to be followed in three-quarters of an hour by a dose of Ol. Ricini. This had the effect of bringing away three leeches, after which the vomiting of blood and pain of the stomach subsided. —*Hüfeland and Osann's Journ. für Pract. Heilkunde.*

FORMULÆ FOR THE EXHIBITION OF HYDRIODATE OF IRON.

For a Bath.

℞ Hydriod. Ferri, ℥ij.
Aquæ, q. s. M.

The quantity to be progressively increased, for adults, by half an ounce at a time.

For Lotions, Injections, &c.

℞ Hydriod. Ferri, ℥ss.
Aquæ Distillat. ℔ij. M.

For Pastiles.

℞ Hydriod. Ferri, ʒj.
Croci Stigmat. ℥ss.
Sacchari, ℥viii.
Gum Tragacanth, q. s.
Fiant Pastil. 240.

Eight or ten to be taken in the course of twenty-four hours, and the dose augmented by one every three or four days. They are recommended in enlargement of the cervical glands, in chlorosis, and particularly in amenorrhœa.

For Ointment.

℞ Hydriod. Ferri, ℥iss.
Adipis, ℥j. M.

The size of a small nut to be used night and morning, rubbed into the thighs, in amenorrhœa, and in leucorrhœa.

For Tincture.

℞ Hydriod. Ferri, ʒij.
Alcohol
Camphoræ, aa. ℥ij. M.

For Wine.

℞ Hydriod. Ferri, ʒivss.
Vini (*Bordeaux*), ℔j. M.

A table-spoonful to be taken night and morning, by adults, in scrofulous affections, &c.

MEDICAL GAZETTE.

Saturday, August 6, 1831.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

ABERNETHY-ANECDOTE-MONGERS.

There is a remarkable feeling on the part of the low retainers of the public press against professions and professional men generally; nothing delights a penny-a-line scribe so much as a sneer at the parson, the lawyer, or the doctor. The principal causes of this fact—itself too palpable to require any illustration—are to be found partly in an interested desire to gratify vulgar prejudice, and partly in the ignorance of the writers themselves. We do not, however, complain of this: professional men, taking them as a body, monopolize an immense proportion of the intellect of the community; and a consciousness of inferiority will always beget in others envy enough to feed detraction. But we do complain of those who write gross slanders under the garb of friendship, and impute to us certain villainous sayings and doings, against which our only vindication is to be found in their broad denial. Look at the case of the late eminent Mr. Abernethy. Those insidious scribblers have marked him for their own—his reputation has fallen into their hands—and every circumstance tends to shew that they have no scruples whatever about sacrificing it. Already, indeed, has the work begun; the anecdote-mongers have fallen upon him; and in a very short time he will be transformed into so perfect a Caliban—so curious a monster—that even his nearest intimates will not be able to recognize him. And all this in the way of friendship! Most assuredly the medical profession have little for which to thank the gentlemen who "do" the articles regarding them which appear occasionally in the periodicals, whether those learned personages venture upon sketches of the living or obituaries of the dead; nor can so much blame be attached to them as the persons who employ them—the persons who give them opportunity to vent their vile commodities on the public. We have been long aware that even the most trashy aspersions and foul libellings were always welcome to such a crew as the Benbows, Wakleys, and Stockdales; but we had no conception that any respectable editor would give place to the vulgar rubbish to which we shall now more particularly allude, and especially after having got ample caution, not so very long ago, respecting the kind of people that he had to deal with. In our third volume it was our lot to dissect certain tales of fancy which had just then appeared in the New Monthly Magazine; and our *exposé* upon that occasion having opened the eyes of the Editor (for we *know* he read our remarks) had the merit of putting a stop to those damning praises which, if continued, would have effectually injured the sale of his work. What, then, was our surprise, a day or two ago, when our attention was drawn to a new periodical, proceeding from the same editorial management, to find the same hand employed once more upon the same subject, and with the same vulgarity of sentiment and style. Mr. Abernethy is once more his subject; and to the fame of this eminent man is the gross injustice done of bringing before the public, in connexion with his name, a set of the most filthy and stupid anecdotes that were ever penned; and this for the pretended purpose of *illustrating* the character of the deceased. Let us take a few specimens; and be it observed that we do not present them to the reader in a form more naked or abrupt than we find them in the precious farrago before us.

> "In all cases of obesity and repletion Mr. Abernethy was especially impatient, and indisposed to prescribe. A portly gentleman from the country once called on him for advice, and received the following answer:—'You *nasty beast*; you go and fill *your g——*, and then you come to me to *empty them.*'"

He then tells us a story about Mr. A.'s slitting up, with a pair of scissars, a young lady's stays, and thus curing her of "difficulty of breathing when taking exercise." After which, he proceeds:

> "Another young lady was one summer's morning brought to him by her mother, in consequence of the former having swallowed a spider. Mr. Abernethy dextrously caught a blue-bottle fly as it *fled* (*flew*, we suppose, he means) by him, and told the patient to put it into her mouth, and if she spit it out in a few moments the spider would come out with it."

This most of our readers will recognize as a regular Joe Miller, and, no doubt, will appreciate the writer's talent for spoiling it so egregiously in the telling.

We have also here the story of Mr. Abernethy's having said, at his own table (where it seems the writer would give us to understand *he* was), that he (Mr. A.) thought that Eton was the best public school for boys, "because it tended *to polish the manners* more than any other, perhaps;" upon which Mrs. Abernethy observed, "*it was a pity* *he had not been educated there himself!"* This *mot* we recollect having met with before, in the article which we castigated in the New Monthly; but, for the life of us, up to this day we have not been able to discover any peculiar point in it, or why it was thought worthy of a second edition.

Come we now to Crispin in another shape. After furbishing up all the anecdotes that he could beg, borrow, or steal, in the didactic form, he suddenly becomes epistolary, and breaks out thus abruptly to the Editor:—

> "He's gone!—my friend, your friend, —nay more, the friend of the human race! If, sir, I did not positively blubber, like Corporal Trim over the dead body of Sterne's Le Fevre, it is not too much for me to assert that the silent tear trickled down my rough-spun, iron cheek, when I heard that *the 'Great Creature'* in the medical world (Mr. Abernethy) 'had gone to that bourne from whence no traveller returns.' [How pathetic this is, and how poetically correct!]
>
> "His departure was rather sudden—the doctor was not exactly prepared to resist the severe attack made upon him by the ugly, ill-natured opponent of mankind; and *his specific*, so extremely successful in numerous other instances, in his own case proved vain:—*the doctor's grand antidote*, universally sought after by all ranks of society to prevent the loss of life, wanted its accustomed efficacy when applied to himself."

This is certainly what may be called plunging *in medias res—haud secus ac notas*. These two little paragraphs would afford materials for an ample commentary; for, besides informing us of the *very recent* death, as we understand it, of the "Great Creature" — or the "Great Feature," as he beautifully varies the epithet in the next paragraph — we are told that Mr. Abernethy's departure was *rather sudden*, and that *his specific* and *grand antidote* failed him in the end—as, we suppose, his *elixir vitæ* did Paracelsus, who died, as Dr. Paris informs us, "*with a bottle of his immortal catholicon in his pocket.*" Now there is much news in all this to us; for the very reverse of some, if not of all, the facts here stated, was consistent with our knowledge.

But to proceed,—there are a few more points before us not undeserving of our attention. We have here a *negative* description of the late Mr. Abernethy, of which, we confess, we do not exactly see the force —some of our readers, however, may, and for their benefit we extract it:—

> "He was not the 'sugar and water' physician, who would smile in your face—laugh at your credulity—and pick your pocket by 'friendly' prescriptions, calculated neither to produce mischief nor good. Neither did *the late doctor* wish to impose on the minds of his patients by the flourish of a *diamond ring*—the display of *laced ruffles*—the *dress coat* the *pompous gait*—the *significant nod*; in fact, he did not *dress for the character* at all."

He was right; we think Mr. Abernethy was perfectly right in not "dressing for the character:" *had he done so, he certainly would have dressed as no man* of the present day dresses—and for a character with which we confess we are, for our parts, totally unacquainted. Does the ridiculous author of the "anecdotes" mean to insinuate, that there is in society, at the present day, a single physician who cuts, or dare cut, those antics which are here alluded to? Such things, no doubt, have been heard of, and perhaps witnessed, in the olden time—in generations by gone; but we beg to inform our anecdote-monger—for we know it will be news to him—that well-bred physicians of the present day are dressed as gentlemen should be — plainly, and without any peculiarity that would attract the notice even of the curious; and that the faculty generally are far more free from any thing like mannerism, than the members of either of the other two learned professions.

We shall not waste more words upon this ignorant scribbler, farther than to mention that he winds up his trashy yarn with three tales, than which we never remember to have met with any thing more miserably pointless. The story of the fox-hunter and Abernethy is told in some dog-language befitting the kennel or "the ring"—it is certainly not English; and the butcher anecdote is the most unintelligible piece of slang jargon that we have seen even in the most vulgar police report. As for the passage between Abernethy and the player, it were utterly unworthy of notice, by reason of its stupid improbability, did it not contain a shameful reflection on the character of the deceased. In answer to his patient's question—"I believe you call this the rheumatism, doctor—what is good for it?" he is made to reply—"*I don't know*; neither do I think the wise ones, whom they term the faculty, are any *better judges* of it! I have been laid up with the rheumatism for the last three weeks, and could not wait upon my patients, and I assure you I was *totally at a loss for a remedy!*" Well may a sign of wonder be affixed to this last expression, supposed to be uttered by the lips of Abernethy. We are utterly confounded at the presumption of the writer of such mendacious absurdity. Can the Editor of the Magazine in question, we ask, be aware of the unjust and libellous trash to which he has given insertion?

PROGRESS OF CHOLERA.

The accounts from various parts of the Continent, but particularly from Russia, regarding the progress of cholera, are of the most painfully interesting description. Many persons fled from St. Petersburgh to Cronstadt on the first alarm—but they left not the disease behind them; indeed it is remarkable that an immense proportion, both of the fugitives and of those with whom they took refuge, is ascertained to have perished. At Cronstadt there occurred, within three days, above three hundred cases, and only seven cures; nearly half having died in the first twenty-four hours, and the rest remaining ill; no British subject had suffered from the disease when the last accounts were dispatched from this place.

At St. Petersburgh the evil of this dreadful visitation has been fearfully aggravated by the state of the public mind, by which society was for a time disorganized. The lower class of the people became impressed with an idea that the "foreign doctors" were employed to poison them; nor did this impression originate solely in the great mortality, but was, perhaps, still more the result of some persons, strangers in the place, having been found with large quantities of active poisons in their possession, of which they could give no satisfactory account. Two of these men were sacrificed to public vengeance. The fury of the mob was particularly directed against the medical men; one of whom was murdered; two nearly beaten to death; a fourth has destroyed himself; and when we add that not fewer than fifteen physicians, attached to public hospitals, have had the cholera, of whom six have died, it will be acknowledged that our professional brethren have had their full share in the public calamity. The medical man who committed suicide was a Jew, entrusted with the examination of those coming to the town by water; and the *on dit* is, that he suffered infected barges to pass, for a "consideration." These accounts represent the Emperor in a more favourable light than we have been lately accustomed to contemplate him. The manner in which, after the entire discomfiture of the police, he threw himself between the rabble and the objects of their fury—braving at once the excited passions of the people, and the immediate proximity of those affected with the malady, appealing to their reason, and controling their desperation—is said to have been in the highest degree imposing, and forcibly to have shewn his moral influence over his subjects. Our medical commissioners were at first assisted in their pursuits—more lately they have been requested by the authorities to abstain, for the present, from any investigations calculated to interfere with public prejudices. No postmortem examinations are carried on except clandestinely; and, in fact, physicians from foreign countries are obliged, by a respect to their personal safety, to be very guarded in their proceedings.

Of the different methods of treatment, that by sub-nitrate of bismuth is regarded at St. Petersburgh as most successful—if, indeed, success can be spoken of where so few recover; and the Emperor has gone so far as to issue a proclamation, commanding the use of this medicine in preference to others—an edict which, if it appear to us rather arbitrary, will at least have the advantage of deciding the question as to the real power of this remedy over the disease. There is reason to believe from these accounts, that ill-timed blood-letting, and over-doses of powerful medicines, have added to the frightful mortality which has prevailed in the northern capital of Russia. During the prevalence of the disease at St. Petersburgh, the wind has been easterly, and the thermometer about 21°, *i. e.* 79° of Fahrenheit.

Our accounts from Dantzic are up to the 22d of July: the disease, which had considerably abated, was again on the increase—a circumstance which was simultaneous with the setting in of close, damp, hot weather. Of 808, the total number of cases reported, 584 had died, 157 recovered, and 66 remained under treatment. The disease was spreading into the neighbouring districts—the opinion on the spot being that the *cordons* were quite unavailing. The malady appeared at Konigsberg and Memel about the 25th and 27th of July. By recent accounts from Stockholm, it is stated that no case had occurred either there or at any other part of the Swedish dominions; Norway enjoying a similar immunity. There are, however, not fewer than two hundred vessels, chiefly Dutch, driving about in the Baltic with the disease on board.

It is satisfactory to us to be able to state—and our sources of information are good—that although a few persons of the better classes have been attacked, as yet they have been but very few, and constitute exceptions to the general rule. By the "better classes," we mean those who have the means and inclination to use wholesome generous diet, without any approach to intemperance. The death of Marshal Diebitsch, and the Grand Duke Constantine, have been supposed to justify a different conclusion; but keeping in view what we have just said, it is not quite apparent to what class of society these personages ought properly to be referred.

CHOLERA *NOT* AT PORT GLASGOW.

Letters have been received from Dr. Daun fully confirming our statements of last week. The cholera, which has prevailed there, is nothing more than the disease with which we are familiar in this country. Its severity, and the number in whom it proved fatal, have been much exaggerated—in fact, it does not appear that more than one had actually died of the complaint. We subjoin an extract of a letter from a physician on the spot:—

Port Glasgow, July 27, 1831.

I am happy in being able to state, for your information, that, after due investigation, there appear no grounds for any alarm on the present occasion.

There have been a considerable number of cases of the cholera, such as generally takes place at this season, but only in the ordinary form.

It is a remark made by some of my medical friends in Glasgow, that cholera generally comes in with the new potatoes and fresh herrings; and I have little doubt that these favourite articles of food, taken in too great quantity, would be quite adequate, in many instances, to produce the disease.

I have no hesitation in saying that ——— came to his conclusions too rapidly, and on no adequate data.

EFFECT OF THE PRESENT EPIDEMIC ON LIFE INSURANCE.

About a fortnight ago, we saw it stated in one of the weekly papers that there had been a "run" upon the Life Insurance Companies, in consequence of the idea that cholera might possibly reach this country. We find upon inquiry, however, that just the reverse of this is the fact, and that much less than the average quantity of business has been done during the last few weeks. It is remarkable, too, that this has not occurred partially, nor with regard to one or two establishments, but has been so general as to have attracted the notice and excited the inquiries of some of the parties. It is evidently the result of some cause in extensive operation, and the idea entertained is, that the public apprehend that if the disease were to break out here, the Insurance Companies would be ruined, so that taking out a policy would be but throwing money away.

CAJEPUT OIL IN CHOLERA.

To the Editor of the London Medical Gazette.

London, July 26, 1831.

Sir,

Enclosed is a copy of a document which has reached me through a source so respectable that I consider it a duty, in the present state of the public mind on the subject of cholera, to transmit for insertion in your valuable Journal such evidence of the utility of a particular medicine in that disease as seems to entitle it to a farther trial.

I may add, that on the morning of the 15th inst. having been myself attacked, pretty smartly, with symptoms of cholera, such as are not uncommon in England about this time of the year, I was induced to take, in the incipient stage, twelve drops of the medicine referred to in a little warm water, and this draught was certainly followed by the immediate subsidence of the symptoms, and I have since continued perfectly well.

M.D.

[The document alluded to in the preceding consists of the examination of a native servant of a gentleman in India, in the course of which he states that he has seen many cases of cholera: that the common remedies almost always failed to give relief, while the cajeput oil almost always succeeded.

It is stated in the newspapers, that Sir M. Tierney has been in communication with the Russian and Prussian ambassadors on the subject, and that a quantity of the oil has been sent to St. Petersburgh: if so, the question of its efficacy will speedily be answered.—E. G.]

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

LONDON HOSPITAL.

Case I. — *Paralysis of the upper and lower Extremities, connected with a diseased Vertebra—Recovery.*

Fanny Sayer, ætat. 8, was admitted into the London Hospital, under the care of Dr. Billing, on the 24th December, with paralysis of the upper and lower extremities. It is stated by her parents that she became paralysed about two years ago, after an attack of typhus fever, and that a swelling appeared on the left side of the neck.

A short time after a hard tumor appeared on the back of the neck, and the child lost the power of rotating the head, which fell forwards; and to prevent the chin from resting on the upper part of the sternum, it was found necessary to make her wear a collar. At present the swelling of the glands on the side of the neck is soft; the tumor on the back is formed by a projection of the second cervical vertebra; the head is immoveable, and inclined to the left side, and the chin almost touches the upper part of the sternum. There is paralysis of the upper and lower extremities, without the loss of sensation; the contents of the bladder and rectum pass involuntarily; the intellectual faculties are unimpaired, and the general health is very good. She has been for some time under the care of a medical man, but without deriving any benefit.

Hirudines xij tumori.
Mist. Cath. Ant. ter die.

Jan. 2d.—Pressure on the projecting vertebra produces much pain. Continues the same as on admission; sleeps well; appetite good; bowels open.

Rep. Hirudines.

12th.—Is still in bed; unable to move her limbs; urine and fæces pass involuntarily; general health very good.

Hirud. iv. alt. diebus.

21st.—Can now control the contents of the bowels and urinary bladder; has in some degree recovered the use of the lower extremities within the last two days; the upper extremities still continue paralysed. There is now a small opening near the tumor, from which a thin purulent matter is discharged.

29th.—Can now move the upper extremities; there is more power in the left arm than in the right.

Feb. 6th.—She has to-day, with the assistance of the nurse, walked across the ward; the limbs are daily becoming stronger; her general health continues very good.

The child remained in the hospital till the month of June, when she was discharged cured. She had entirely regained the use both of the upper and lower extremities; and the small sinus had filled up and healed. The head remains firmly fixed, and inclined forwards. The cure may be attributed to perfect rest in the horizontal position, and the continued application of leeches around the tumor as long as any pain was produced by gentle pressure on the projecting vertebra. The general health being always good, medicine was only administered for the purpose of keeping the bowels open.

CASE II. — *Hard Swelling of the Abdomen—Melæna—Death.*

Joshua Webb, ætat. 10, of a delicate form, was admitted into the London Hospital on the 30th of December, with a swelling of the abdomen, without any evident sign of fluctuation. The abdomen, when pressed, is tender; the skin feels hot; he has much thirst; no appetite; and bowels are in general confined. The swelling commenced about two months ago, without any evident cause, since which time he has become gradually worse. The plan of treatment generally followed during the time he remained in the hospital was a steady course of purgatives, with the repeated application of leeches to the abdomen, which afforded much relief; and he also took soda in the decoction of bark frequently during the day.

On the 13th March he left the hospital of his own accord. About three weeks after he was attacked with melœna, which continued for four days, at the end of which time he expired.

Post-mortem Examination: Abdomen.—The peritoneal surface of the bowels was every where adherent to the parietes of the abdomen, and was completely studded with tubercles in the crude state. The peritoneum covering the liver and spleen was also covered with tubercles. In the bowels was found a large quantity of coagulated blood.

Thorax.—The pleura was thickly studded with small tubercles; the lungs contained only a few—in other respects they were healthy. The heart and pericardium were healthy. Head not examined.

CASE III.—*Large Tumor in the Abdomen—Paracentesis—Death.*

Elizabeth Benson, ætat. 44, a widow, of a sickly appearance, was admitted into the London Hospital on the 26th February with a great distention of the abdomen, which to the touch is hard and tense. She reports that she was a patient in the hospital about two years ago, with a large swelling of the knee, for which amputation of the limb was performed.

The swelling of the abdomen appeared for the first time about nine months ago, and has gradually increased to its present size: it was preceded by pain in the right hypogastric region; percussion indicates a slight fluctuation. There is also a hard circumscribed tumor near the spine of the left scapula, which appeared about eleven months before her admission. Tongue clean; bowels open.

Tinct. Digitalis, gtt. xx.
Spirit. Æth. Nit. ʒss. ex
Infus. Quassiæ Cath. ter die.

Pulv. Jalapæ c. Cal. ℈j. aa.

29th.—Continues much the same.

Contin. medicamenta.
Paracentesis abdominis.

3d March.—The paracentesis abdominis was performed this forenoon. About two ounces of blood, mixed with a little viscid fluid was all that passed off through the canula.

Contin.

4th.—Has felt no bad effects from the operation of yesterday.

5th.—The nurse reports that the patient became sick on the evening of the 4th, and expired at two o'clock this morning. She also states that, soon after the death had taken place, she observed a quantity of blood to escape from the mouth and nostrils, and, on removing the body, a considerable quantity was found in the bed.

Sectio Cadaveris.—The body was examined thirty hours after death, when decomposition had already taken place.

Thorax.—No morbid appearance in the chest or its contents, a few old adhesions excepted.

Abdomen.—On opening the abdomen, the lower part was found to be occupied by a large tumor, which had displaced the mass of the intestines, and had formed adhesions with the omentum. The tumor on being cut into was found to contain a number of cysts, some being filled with medullary sarcoma, and others with a thick viscid fluid; the total contents of the tumor might be computed at two gallons. The left ovarium and the uterus were perfectly healthy.

The large tumor on the back was composed entirely of medullary sarcoma.

The head and spine were not examined.

Case IV.—*Intermittent Fever—Cure.*

Thomas Drudge, æt. 40, a labourer, of a tall, spare, but strong frame, with a pale sallow countenance, was admitted into the London Hospital on the 13th February He reports, that about nine years ago he was a patient at St. Thomas's, with rheumatism, and was afterwards in the same hospital for about five weeks with intermittent fever, which was checked by the use of bark.

About three months ago he was attacked with a slight cough, which was soon followed by his present illness: and during these few last months he has lost strength, and become much emaciated. He has now one attack of ague every twenty-four hours; it comes on about one or two o'clock in the morning, the cold stage lasting about an hour, and the two succeeding ones about three hours, leaving him in a very debilitated state. He complains of great weakness, of loss of appetite, of nausea and vomiting, his stomach refusing every description of food. There is a slight tenderness of the left hypochondrium on pressure, but no enlargement of the viscera can be discovered by the most careful examination. Pulse 110; tongue white; skin dry; bowels open; urine free, but high coloured.

Hirudines xx. hypoc. sinistro, c. fotu.
Pil. Hydrarg. Submur. c. gr. v. o. n.
Ol. Ricini, om. mane.
Low diet and beef-tea.

14th.—The leeches bled freely, and the tenderness of the left hypochondrium is entirely removed. He slept well, and feels much better. Had a paroxysm between one and two o'clock this morning, which lasted about four hours; he says that it was not so severe as the former attacks. Pulse 100; tongue clean; no appetite; has taken a little beef-tea, which was not rejected by the stomach.

Cont. Medicamenta.
Hirudines x. epigast. altern. diebus.

16th.—Feels in every respect better.

Cont. Med.
Bal. tepidum adventu paroxysmi.

22d.—Was put into the warm bath last night (for the first time) on the approach of the paroxysm, and which was checked during the quarter of an hour that he remained in the bath: it, however, returned on his being put to bed, but he says not with the same severity as formerly. Slept well, and feels better this morning; cough easier; pulse 108; tongue clean; appetite good, and can now retain all his food; bowels open.

Perstet.

24th.—The paroxysm came on yesterday evening about half-past six; was not put into the bath, as ordered. Appetite good; pulse 66, and regular; tongue clean; bowels open.

Quinæ Sulphatis, gr. v. ter die.
et Contin. Pil.

March 3d.—No return of ague since the 25th ult.; his general health continues to improve.

Cont. Quinæ. Sulphatis
Omitt. Pil.

6th.—No return of ague; appetite good; tongue clean; pulse 72, full and soft; bowels open.

Cont. Quinæ Sulphatis.

9th.—Complains now only of weakness across the loins.

Emp. Roboran. lumbis.
Omitt. Quinæ Sulphatis.
Middle diet.

16th.—His health continues improving.

20th.—Is gaining flesh and strength, and makes no complaint.

Full diet.

26th.—Is now quite well, and goes out in a few days.

NORTHAMPTON INFIRMARY.

Poisoning with Stramonium—Recovery.

To the Editor of the London Medical Gazette.

Sir,

If the following case is considered sufficiently interesting, I beg you will insert it in your valuable publication, and you will oblige, sir,

Your obedient servant,
James Mash,
House Surgeon,
General Infirmary, Northampton.

On Wednesday morning, July 6th, a person applied for assistance at the Infirmary for a woman who had taken by mistake two teacupsful of the infusion of stramonium for the infusion of sennæ.

Case.—Hannah Gibbs, æt. 36, of delicate habit, swallowed, by mistake, two teacupsful of the infusion of stramonium for senna tea. About ten minutes afterwards she was seized with giddiness of the head, dimness of sight, and fainting. Her neighbours, hearing a noise in her bed-room, came to her assistance, and went to a Druggist, who sent a powerful emetic, which, however, did not produce vomiting. Two hours afterwards I saw her, and found her quite insensible; pupils dilated, and not acting to the stimulus of light; the whole muscles of the body convulsed; countenance flushed; pulse full, and rather slow. With the assistance of the Infirmary pupils, I washed out the stomach several times with large quantities of tepid water, by means of the stomach-pump, and with such good effect that in a few hours afterwards she was able to come down stairs; and when I went to visit her at three o'clock in the afternoon, I found her taking her tea, and perfectly sensible. She states that her sight is not quite so good as before, and that her head feels rather light. I should have stated that a common enema was ordered immediately after the stomach pump had been used, and which operated freely.

ON FAILURES IN LITHOTOMY.

[The following are the cases and observations alluded to in our notice of Mr. Fletcher's Medico-Chirurgical Notes and Illustrations. They will be found worthy of attention.]

Lithontrity may, by being successful in the treatment of small calculi, diminish the frequency of lithotomy, but it will never supersede this operation altogether. Its difficulties, irregularities, and failures, will, therefore, still be objects of interest, and worthy of collecting.

In performing the operation of lithotomy, it is, perhaps, too much the fashion to consider rapidity of execution as a leading excellence; but in our attempts to acquire this captivating dexterity, there is reason to fear that mischief is sometimes done—because hurried and showy movements are scarcely compatible with the gentleness and caution which are necessary to success.

The fatality of lithotomy cases recorded in the Journals, may, in a great degree, be ascribed to these attempts at brilliant practice; of counting the number of minutes Mr. ——, of great celebrity, takes to perform this operation; a dangerous guide, for, without his constant practice, none can be equally rapid, with safety to the patient!

"That operation is done quick enough, which is well done," said one of the best surgeons of our days. If rapidity be to save pain, it fails in its object, for severity of suffering, from violence of the movements, is more than a balance for a more moderate degree longer continued.

The first incisions may indeed be as quick as the surgeon may please, but when the membranous portion of the urethra is to be carefully cut, just before the prostate gland, the gland itself, and the forceps—the too often terrible forceps—used, it is humbly conceived that the operator should not be in a hurry; and this especially when he cuts with a knife a man with a fat, and a deep perineum. It is sufficiently difficult and inconvenient, in such a case, to manage with precision the movement of the knife, so that its cut shall be just what is wanted for room, and no more.

But in a hurry, and taking for granted a great deal, viz. that you have made the prostatic opening sufficiently large, without your finger, at such a distance, being exactly able to demonstrate the fact—it will at last be discovered, in attempting to extract the stone, that room is yet wanting, and then the neck of the bladder receives a fresh assault—more violence is added—the knife is resumed to touch and retouch—till room enough be obtained.

To avoid this, in such a case of a deep perineum, the superiority of the gorget is manifest, if of the requisite breadth and length for the subject. It reaches its object—makes a clean cut at once—sufficient to allow of the passage of nineteen stones out of twenty, without further violence being necessary.

If the Journals give correct reports of the mode of operating by the most eminent of the profession, the principle of non-violence is either not allowed, or if allowed, not always acted upon. Nay, further; it is scarcely defended; for one of the most experienced and successful lithotomists is quoted as a person who disregarded, as unnecessary, this principle altogether. To assist in doing away the lamentable and dangerous effects of such doctrine, the subsequent cases are chiefly recorded. For surely no fact is more unquestionable, none can be more important, or better worth remembering, than that it is to a rough, worrying, and violent method, that the greater number of failures, in this operation of lithotomy, are to be ascribed. The greater the violence, the sooner is the work of death accomplished. The nervous system, in some examples, shook to its foundation, will scarcely maintain its power over the frame sufficiently long to allow the sufferer to reach his bed.

He dies at once, withered by the stroke.

In others, this first danger being overcome, inflammation is excited in the peritoneum, bladder, or cellular membrane, between it and the rectum, and they perish soon of peritonitis, or more slowly through the irritative agency of suppuration.

If hospital surgeons, who do this operation most frequently, were to report the failures they have witnessed, and the circumstances which attended them, much might be done towards abbreviating the sufferings, and preserving the lives of patients. The history of failures is, perhaps, more valuable than that of successful cases. Dissection will trace the causes of death, with the errors committed, and point out how, in future, they can be avoided, so as to lead to more precision, and certainty of a successful termination. A broken-down sufferer of many years, who has made up his mind to submit to a terrible operation, the climax of pain and punishment, relying on our skill, judgment, and humanity, for its being safely done, should be considered as a patient of the whole profession. He has placed life, his last and most precious stake, in its hands; and every member, whose experience allows him an opportunity, should not hesitate to contribute his mite to its preservation, by recounting, as warnings, the failures that he has beheld.

It will be found that their main source is violence, generally, though not always, from the forceps, on whose blade should be engraven the motto, "Gardez bien." This violence is often unnecessary, for it is better to cut, than to bruise or lacerate, in the extraction of large calculi; to cut the bladder again and again, than to tear it open. On this principle was derived the great success which attended the operations of that celebrated lithotomist, Klein.

That it is the forceps which is the great agent of destruction, in the larger number of cases, is clear, from looking carefully over the sizes of the stones extracted by the late Mr. Martineau. He encountered no very desperate cases,—he was thus as fortunate as he proved skilful.

Out of eighty-four cases, the two largest stones weighed each four ounces only, and one of these patients was lost. Why? Because the forceps had too much to do in the extraction. When the stones were small, which in his great experience was remarkably the case, the forceps had very little to do. Hence his extraordinary success, and the detection of the true source of destruction.

Mr. Martineau, therefore, could never be justly quoted as an authority for violence in lithotomy. He seldom had occasion, from the size of the stones, to employ it; but when he had, he lost his patients, like other surgeons.

There are, however, other sources of injury besides the forceps. Such as I have seen, from that and other causes, with all the failures which have occurred within my observation, shall now be faithfully narrated.

CASE I.—*Fatal Abscess of the Pelvis, from a lacerated Bladder by the Forceps.*

A Sexagenarian from the country, tall, and very little worn in constitution by the presence of a large stone, which he had carried some years in his bladder, came under my care to have it removed. It was my maiden operation, and I was surrounded by experienced friends. From repeated examinations made in the rectum, and by sounding, it was evident that the stone was beyond a common size, and preparations were made accordingly; the muscles were fairly and freely cut, and the prostate gland divided by a full-sized gorget. The stone breaker was at hand. It was not difficult to lay hold of such a stone; the difficulty was in bringing it through its narrow channel with safety to the patient. I made gentle efforts in the proper direction, put my finger upon it between the blades of the forceps, in the rectum, and this examination assured me that it would never pass without more force and laceration than was consistent with the patient's safety, and with my notions of the mode in which this operation should be performed. In vain was the opening into the bladder enlarged by the bistoury, and a more powerful exertion made—the stone would not pass.

I looked round for the stone-breaker; I begged that it might be handed to me. "My dear sir," with a pinch on the elbow, "try again," was the reply on one side. I did so, reluctantly; another more powerful, though unsuccessful, pull was the consequence, and again I intreated imploringly for the stone-breaker: "Nonsense, don't be afraid, I have used ten times more force than you now do," was the answer from another side, (it was true, but his patients rarely survived;) one effort more, indeed, succeeded in bringing forth the stone, which was of the mulberry kind, and weighing about five ounces and a quarter, and after the patient had been upon the table three-quarters of an hour. The last adviser was not a little proud of so speedy a proof of the soundness of his advice; but he should have waited the result.

The shock of the operation the hardy veteran sustained; its immediate danger passed away; but he soon fell off, and ultimately sunk under irritative fever, at the end of the fifth week from the date of the operation.

The irritation was a large abscess in the cellular membrane, between the bladder and rectum, and which doubtless arose from the injury done to the prostate gland and neck of the bladder, which were in rags or fringes, bedewed with pus. The result of the foregoing case was of service, though not to the patient. I became particularly cautious of committing the slightest violence beyond what was absolutely necessary, rather cutting even the bladder, than allowing of any force in extracting the stone from it, and the effect was, that the next nineteen operations were successful ones. The stone, in this case, was too large to be removed with certain safety, though there would have been more chance of success with less violence. We may call for, as was done in the foregoing case, and then look at stone-breakers, but to use them is, perhaps, quite a different matter. Mr. Earle's is the best. The lithontrite appears to be inadmissible, from its want of power over large stones.

CASE II.—*Fatal Peritonitis from a lacerated Bladder.*

My second, and ultimate loss, was a young man of twenty-two years of age, and who was a good subject for the operation, that is to say, that no tendency to organic disease could be detected in him.

The following note, taken after the operation, will explain enough of the circumstances for the present purpose.

> No man should cut for the stone when he is ill; the feeling of lassitude, weakness, and want of decision, will creep into the operation. A slight oversight in the design, or defect of vigour in the execution, are quite enough to give a fatal turn to its termination. To-day I was not sufficiently alert; the gorget was overlooked; it did not cut well close to the beak, nor was it broad enough for a large adult; so that the right side of the prostate was not completely divided. The muscles too, in the deep hollow between the ischium and anus, were not sufficiently or decidedly cut; so that a straitened channel was left for the exit of the stone. Both these original errors were amended, though feebly and inefficiently; illness was at the bottom of it; the division of the right side of the prostate was completed, and the bridles of muscles touched with the knife. These subsequent corrections were not enough to prevent more violence being done than should be permitted in this operation. It was ten minutes before the stone was extracted; and though I have seen infinitely more rough exertion employed, without harm in the result, yet do I fear for this poor fellow."

The weight of the stone was four ounces.

This patient died of peritoneal inflammation five days after the operation, in spite of the most early attention, and vigorous means employed to subdue it.

The angle of the left division of the right side of the prostate gland was torn, proving that its division by the gorget had not been quite accomplished. Some pus occupied the cellular membrane in its neighbourhood, and the small intestines were glued together by active peritoneal inflammation, which pervaded the whole cavity of the abdomen.

This man, possibly, might have been saved, had the incision of the prostate and bladder been made more free for the passage of the stone, instead of wrenching it from its bed by the forceps; although, when stones arrive at a large size, there must be some doubt about the result, should the lateral operation be performed.

CASE III.—*Immediate death from extraordinary violence in the use of the Forceps.*

The spectator in an operation room must always feel more than the operator himself, who is busy with his work, and, indeed, who should see nothing else but that this is well done.

A healthy, middle-aged looking man, walked into the operation room, with a cheerful, and somewhat of a bravado manner, to be cut for the stone. Without any assistance, he mounted the table, and offered his hands and feet to be tied, with the air and countenance of one entering the prize-ring, and whose mind had a full determination to win the fight or perish, though the possibility of the last was not at all in his contemplation. Scarcely would he submit to have his eyes bandaged—he appeared as if he wished to behold the whole of that process which was to restore him to health and to his family!

There are some operators—or let us rather say there were—who, with but little knowledge, can, by a talent of imitation, perform the mechanical movements of an operation with singular boldness, rapidity, and effect, provided they meet with no irregular or embarrassing circumstance to disturb this beautiful harmony. But should any unexpected and frightful event suddenly break upon the view of the hitherto brilliant man, then he becomes astounded, and is all abroad at the very moment when complete knowledge of the subject, the most perfect self-command, the entire understanding, are necessary to the poor patient's safety. It is then, too, the alarm and confusion commence;

the requisite knowledge, and consequent coolness, are not at hand; desperate force is substituted for skill, and the patient is in the most imminent danger; that is to say, in more danger from the operator himself than from the disease which he attempts to remove.

In the present instance, the operator made his first incisions clean and correct; he reached the bladder, felt the stone, and, in a twinkling, introduced an immense pair of forceps into the passage. But there was evident difficulty or obstruction in their entrance to the bladder. Some more plunges were made with them—still their room for action was evidently confined; at last they griped the stone; but, from the wide separation of the handles, it was evident that the stone was either very large, or held in the forceps by its long axis.

"This is a very large stone, Doctor; it won't come without a great deal of force:" a great deal of force was immediately applied, (and that not in the best direction), but to no purpose,—the stone would not pass. The operator rested; the patient was calm, and complained not! The labours of the former, (his strength being recruited), now re commenced with redoubled vigour, and an air which imported a dreadful determination to succeed. His right foot was placed, in preparation for this really awful struggle, against a chair, which was supported by a pupil; the scene became animated, though horrible. The straining and creaking of the forceps, as they occasionally lifted the suffering wretch from the table, (they twice pulled him off it)—his wild, agonizing shrieks, and entreaties for forbearance, after continuing for nearly two hours, gradually became more faint, and sunk, at last, into a piteous moan—and when the stone was shown to him it was doubtful whether he saw it, or was even conscious that a period had, at length, arrived to sufferings that never were exceeded in mortal man.

He expired in a few minutes after being carried to his bed. The body was not examined.

The operator was naturally a quick and clever man, with a great deal of tact; but he here met with a stone of more than five ounces in weight, without having previously made himself acquainted with the case by repeated examinations, which would have enabled him to plan his operation better, so as to meet the peculiar difficulties which a very large stone must create.

Upon looking at the gorget, I thought it certain that it could not, from its small size, have completely divided the left side of the prostate; moreover, it cut only on one side, so that room was lost on the right side of this gland. The operator, too, having seized the stone, appeared to be unwilling to part with it, fearing it would be difficult to find it again; although he must have felt a stricture or binding upon it, which would require great force to overcome. He appeared to be mentally whispering to himself, "if I let it go I may not get hold of it again, and it shall come now it is in my power," and with this wrong understanding of the principles of this operation, the fatal pulling was continued.

This feeling of apprehension, of fear of losing the object of his anxiety, too often occupies the mind of the operator, especially in the case of a deep perineum, where the stone is at a great distance from the finger, and not easily felt or commanded by it. But no woman's fear can be more unreasonable, no surgeon's more ruinous—for it leads to acts of desperate violence. The road to the bladder cannot be lost after the forceps has once entered its cavity, and the stone will be as easily seized a second or a third time as it was at first,—at least, it will be seized, by steady and gentle efforts.

If any considerable sense of stricture, therefore, should be felt at the prostatic opening, when the operator attempts delivery of the stone from the bladder, he should desist from all violence, of a kind likely to bruise or tear—the stone should be relinquished, and an examination of the extent of the incision, already made, be carried into effect. These will then be enlarged, according to the circumstances, in those points where the stricture or bridling exists, or where the first incisions have been incomplete, or inadequate to the size of the stone.

After all, operators, in this difficult case of a large stone, are to be pitied—it must either be extracted or left behind,—the want of success is more the fault of the operation (when the lateral is chosen) than in a want of skill in the surgeon.

COLCHICUM AUTUMNALE.

To the Editor of the London Medical Gazette.

Sir,

Referring to No. 176, and No. 190, of your journal, in the former of which I informed the profession that a series of narcotic preparations had been commenced at the laboratory, and in the latter I communicated the results of my experiments on conium, I am engaged in the pharmaceutical analysis of colchicum autumnale, which will be submitted to the attention of such members of the profession as may be induced to visit the laboratory during the process.

I am, sir,
Your obedient servant,
R. Battley.

Ophthalmic Hospital, Moorfields,
August 4, 1831.

W. Wilson, Printer, 57, Skinner-Street, London.

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SATURDAY, AUGUST 13, 1831.

OBSERVATIONS ON CALCULOUS DISEASES.

By B. C. BRODIE, F.R.S.

As delivered by him in his Surgical Lectures.

Operation of Lithotomy, continued.

THE first part of the operation of lithotomy, as I have already stated, is the making an incision into the urethra where it lies in the perineum; the second is the dilating, or dividing that canal where it is surrounded by the prostate. To facilitate the accomplishment of these objects, it is convenient to begin with introducing into the urethra this solid steel instrument, which we call a staff. It is of the figure of a sound; from which, however, it differs; first, in the handle, which, instead of being smooth and polished, is made rough, in order that it may be more firmly and steadily held; secondly, in having a groove, like that of a director, on its convex side. It is, in fact, a director, and intended to answer precisely the same purpose. The staffs sold by the instrument-makers are generally of too small a size. They should be as large as the urethra will easily admit without being painfully stretched. A large staff is more easily felt in the perineum than a small one, and it admits, of course, of a deeper and wider groove. The groove ought to become gradually shallower just before it terminates at the extremity of the instrument, in order that the point may be neatly rounded off. The edges of the groove ought to be carefully rounded off also. Attention to these circumstances in the construction of the staff, renders its introduction more easy. I generally begin the operation with introducing the staff into the bladder, merely because it is, on the whole, more readily managed when the patient is standing erect, than after he is placed on the table.

The next thing is to secure the patient in a proper posture, with the perineum exposed. About two feet six inches is a convenient height for the table. The patient should be placed on it lying on his back, supported by pillows, with his shoulders somewhat elevated. He should be directed to grasp the outside of each foot with the hand of the same side; and then the hand and foot are to be bound together by several turns of these bandages, which we call lithotomy garters. If the patient be corpulent, he probably will not be able to grasp his feet, and he must in that case grasp his ankles instead. Besides the lithotomy garters, it is convenient to apply another bandage—the neck strap, which is thrown over the back of the neck, and passed under each ham. These bandages are not employed with a view to prevent the patient struggling, as persons out of the profession generally suppose, but solely for the purpose already mentioned, namely, to keep him in a convenient posture, with the perineum properly exposed. Thus prepared, the patient is drawn towards the end of the table, with the buttocks rather projecting over it.

Several assistants are required, one to support the patient on each side, holding his feet, hands, and knees, and keeping the lower limbs well asunder; a third to give you the instruments, in the order in which you want them; and a fourth to hold the handle of the staff.

It is also convenient, though by no means necessary, to have another assistant, to support the patient's shoulders. Your assistant, who holds the staff, may stand on either side, but it is usual for him to stand on the patient's left side, in order that he may take the handle of the staff in his right hand.

The surgeon himself should be seated on a stool before the patient. He is first to attend to the position of the staff, taking care that it is held nearly perpendicularly; the handle of it being, however, a little inclined towards the patient's right groin. This causes the convexity of the instrument to project a little on the left side of the perineum.

In the first part of the operation your attention is to be directed to the staff. You are to feel it with your left hand, and the knife held in your right hand is to be directed towards it. It is a sure guide; following which, you can never err, even in the deepest perineum; on the other hand, if you lose sight of it, you are cutting in the perineum, as it were, at random; you divide parts which you ought not to divide; especially you are in danger of carrying your incisions too near to the ramus of the ischium, where the arterial branches of the internal pudic artery are of a larger size than in the centre of the perineum, and therefore more liable to bleed. I have seen some surgeons endeavour to introduce the point of the double-edged scalpel into the groove of the staff at the first incision. But I caution you against this, as a great error in the operation, except indeed it be in the case of a young and very lean subject. Where there is any quantity of fat in the perineum, or any thing even distantly approaching to what we call a deep perineum, if you attempt to cut at once into the groove of the staff, the result is, that you open the urethra too far forwards; you divide the *corpus spongiosum* of the penis, which need not in reality be divided at all; and you are then certain of wounding the artery of the bulb of the urethra, which otherwise is, in most instances, avoided. Another inconvenience which attends on this method of proceeding is, that the wound being too near to the scrotum, the cellular membrane of it is in danger of being infiltrated with blood; and another still is, that a greater mass of substance is left to be divided, when you continue the incision into the bladder, than there would have been if you had cut into the urethra farther back in the first instance.

I say, then, let the opening in the urethra be made deep in the perineum, behind the bulb, and as near as can be to the prostate. Place the thumb of your left hand on the skin over the staff; and in a man of ordinary size, about an inch and a quarter before the anus. Begin your incision immediately below this, on the left side of the raphe, and continue it backwards and towards the left side, into the space between the anus and the tuberosity of the left ischium. Here you may cut freely: you can injure nothing of consequence. Then feel for the staff in the wound; direct the point of your knife towards it, and carefully cut into the groove, where it lies in the membranous part of the urethra. All these incisions are, you will observe, made low down in the perineum, that is, near to the rectum. I have already given you what I conceive to be sufficient reasons for avoiding incisions in the upper part of the perineum. I may add another, namely, that if the external part of the wound be in the lower part of the perineum, there is a depending orifice for the free discharge of the urine after the operation, which there would not be otherwise. There is also a great authority in favour of this mode of proceeding. Cheselden made his incisions in the way which I have mentioned, as is proved by the anxiety which he evinced to avoid injuring the rectum. Had he done otherwise, it would never have entered into his contemplation that the rectum was in danger.

The next step of the operation is the continuance of the incision along the posterior part of the urethra, and the dilatation of the neck of the bladder. Some recommend this to be accomplished by means of the common scalpel, with which you have made the external incisions; the point being steadily introduced along the groove of the staff, with the edge turned outwards, so as to divide the left side of the prostate. This was Cheselden's method of operating. I draw this conclusion from Cheselden's own account of his operation, not from the absurd statement published by his cotemporary, Dr. Douglas, who evidently understood nothing of the matter, and indeed describes an operation which it is next to impossible to perform. But after having incised the prostate and neck of the bladder, Cheselden introduced the instrument which I now shew you, the blunt gorget, so as to dilate the wound still further, answering at the same time the purpose of a conductor for the forceps; and, as far as I can learn, this method was followed generally by the English surgeons up to the time of Sir Cæsar Hawkins. This celebrated operator, who exercised his skill, and acquired his reputation, within the walls of this hospital, caused one side of the gorget to be ground to a sharp edge, and thus converted the blunt into a cutting gorget. The cutting gorget of Sir Cæsar Hawkins (and all those that have been since invented are but modifications of it) was intended to supersede the use of the knife in opening the neck of the bladder, at the same time that it answered the purpose of a blunt gorget in other respects. It would be presumptuous in me to say that the cutting gorget is not a good instrument, when it has been employed, not only by many of our more distinguished, but by some of our most successful lithotomists. But I cannot but think that there are some considerable objections to it nevertheless. The incision is made as it is being thrust into the bladder. In consequence of the thick wedge-like form of the instrument, the prostate, and especially a hard and enlarged prostate, offers to it considerable resistance. A certain quantity of force is necessary for its introduction; and if that force be not well applied, the beak may slip out of the groove of the staff into the space between the bladder and rectum, an accident which is too surely followed by the death of the patient. Now I know that such an accident ought not to happen, but I also know that I have seen it happen to a very experienced and dexterous lithotomist. There is, of course, a still greater chance of its happening to an inexperienced lithotomist, (and all are inexperienced in the first instance.) These considerations lead me to recommend you not to begin with the cutting gorget: you may adopt it, if you please, afterwards. For my own part, although I have very frequently used the cutting gorget, I generally make the incision of the prostate with the knife which I now shew you. You will observe that the blade is broad enough to divide a considerable portion of the prostate, as it enters the bladder, without it being necessary to increase the size of the incision by cutting laterally afterwards; and that, instead of a sharp point, it terminates in a beak, fitted to the groove of the staff. In ordinary cases, a knife of this kind, with a single cutting edge, is sufficient; but in cases of very large calculi, there are good reasons for dividing both sides of the prostate. There is no objection to this being done that I can discover; and for such cases I have been for some time in the habit of using this double-edged knife, with a beak projecting from its centre.

Having made the opening into the membranous part of the urethra, you are to insert the beak of the beaked knife into the groove of the staff. You then take the handle of the staff into your left hand, depressing it at the same time. You depress your right hand also, so that the handle of the knife, which you hold in it, lies in the lower part of the external wound. You are now to push the knife along the groove of the staff into the bladder, with its cutting edge inclined a little downwards towards the ramus of the ischium, if you use a single-edged knife, but holding it horizontally if you use one with a double edge. Let this be done slowly, cautiously, taking care that you do not lose the feeling of the beak sliding over the smooth surface of the staff for a single instant. Generally, as the knife enters the bladder, a few drops of urine escape, but never any large quantity. This being accomplished, you are to withdraw the knife along the groove of the staff in the same line in which you introduced it. Never cut with it laterally, except you find it afterwards absolutely necessary to do so, on account of the large size of the stone; for in cutting laterally, you will find it difficult to measure exactly the extent of your incision; and you may endanger your patient's life in consequence of your dividing the parts beyond the boundaries of the prostate.

The next step of the operation is to introduce your finger, directed by the staff, into the bladder, so that you may feel the parts which are divided, and determine whether the incision is properly made. If you operate on a child, or on a young and thin person, you may then at once introduce the forceps into the bladder. But if you operate on a full-grown person, and especially on one having a deep perineum, it will be prudent for you first to introduce this instrument, which we call a blunt gorget, previously to the use of the forceps. The blunt gorget is, as you perceive, an oblong plate of steel, turned up at the edges, so as to present a concave surface above, and a convex surface below. The handle is inclined downwards, and that extremity, which is opposite to the handle, gradually becomes narrower, and terminates in a beak similar to that of the lithotomy knife. The surgeon takes the blunt gorget in his right hand, and inserts the beak in the groove of the staff; then, holding the handle of the staff in his left hand, and depressing it at the same time, he carefully introduces the gorget into the bladder. Having done so, he withdraws the staff, and leaves the gorget in the wound.

The gorget is intended to answer the purpose of a director for the forceps. But it answers another purpose also—it is a dilator of the wound. The knife divides only a portion of the prostate. The gorget splits the remainder as far as its breadth allows it to do so. Do not for an instant suppose that this is any rude or violent proceeding. It is far otherwise. The incision of the prostate having been begun by the knife, the extension of it by means of the blunt gorget is accomplished with the greatest ease. If you perform the operation on the dead body in the way which I have described, and dissect the parts afterwards, you will distinguish very readily the clean smooth surface made by the cut of the knife, from the fibrous, or striated surface, made by the splitting of the gorget. You will ask, why not make such a division of the parts by cutting laterally with the knife? Why prefer the dilatation of the wound by the blunt gorget? My answer is, that the separation of the parts with the latter instrument causes no hæmorrhage; and that it ceases as soon as it reaches the margin of the prostate—that is, as soon as it reaches the condensed cellular membrane, which forms what may be called its capsule.

Before explaining the use of the lithotomy forceps, I must shew you their construction. One of the handles terminates in a ring, the other in a loop. The blades become broader towards the extremity; their opposite surfaces are concave, and armed with small pointed projections, or teeth. When closed as far as they can be closed, the ends do not exactly come in contact. Thus they are well fitted to hold the stone which they have seized, at the same time that, if the stone be not seized, it is impossible for them to pinch the mucous membrane of the bladder. This particular forceps is made according to the pattern of those which Cheselden employed on most occasions, as described by Douglas, and you will find them very generally useful. You must not, however, rely on these alone: you must have forceps which are longer and larger: others much smaller, especially for operations on children. You should be provided also with curved forceps, to be used where the stone lies in the hollow behind an enlarged prostate gland.

The surgeon, then, holding the handle of the blunt gorget with the left hand, introduces the forceps with his right, along the concave surface of the gorget, into the bladder. This is to be done cautiously, and without violence. But it is to be observed, nevertheless, that the forceps always experience a certain degree of resistance, and that some force is necessary to make them enter the bladder. You know when they have entered by the resistance ceasing, and, in many cases, by a gush of urine taking place at the time. In a deep perineum the forceps have to penetrate to a great depth before they arrive at the bladder. This is one of the sources of difficulty and doubt to a young surgeon, who is apt to think that the forceps must have actually entered the bladder, when they have, in reality, penetrated no farther than the prostate. The forceps having been introduced, the gorget is to be withdrawn.

The surgeon is not to open and close the forceps at random. He is to use them first as a sound, exploring the different parts of the bladder, until he has ascertained where the stone lies. The discovery of the stone will be very much facilitated by the introduction of the finger along the groove of the staff previously to the introduction of the blunt gorget—at least in most instances. In a case of enlarged prostate and deep perineum, where the finger will not reach the bladder, this mode of examination is, of course, of no avail. The stone being touched by the forceps, the blades are to be opened upon it, and the stone is, in general, readily grasped.

I have already mentioned a case in which the muscular coat of the bladder was ruptured in consequence of the surgeon too forcibly and hastily opening the forceps; and this will be a lesson to you as to your conduct in this part of the operation. But I conceive that the danger of such an accident as this is not the same in all cases. In some instances, when you begin the operation, the bladder is distended with urine; then when the instruments enter it, the urine rushes out, not impelled by muscular action, but by its own gravity, and the pressure of the viscera. Under these circumstances, when you introduce your finger into the bladder, you find the muscular tunic relaxed, with the mucous membrane hanging in folds; and, in consequence, they are not likely to be ruptured. In other instances the patient voids his urine immediately before the operation, or perhaps during the introduction of the staff. Here the urine, being made to flow by the patient's own efforts, the muscular tunic is contracted: it offers a considerable resistance to the opening of the forceps, and is liable to be ruptured, if they are opened rudely and incautiously. It sometimes happens that a small stone lies, as it were, concealed in some part of the bladder, perhaps beneath a fold of the mucous membrane, so that you cannot easily bring the forceps in contact with it. You will then frequently succeed in seizing it in the following manner. Expand the forceps carefully until the blades are widely separated from each other, holding them at the same time in such a position as that the blades open horizontally. This dislodges the stone, and causes it to fall to the lower surface of the bladder, and then, as you close the forceps, you find that you have seized it. In other cases, where there is a tumor at the neck of the bladder, caused by an enlargement of the prostate gland, the stone is liable to be lodged behind the projection. You feel the stone, but the forceps slide over its surface, and do not grasp it. It is in such a case as this that the curved forceps are useful, which are capable of dipping into the hollow behind the prostate. Under these circumstances, you may find it useful to introduce the finger into the rectum, and raise the bladder, by means of it, towards the pubes. It is evident, however, that this expedient can be of no use except where the bladder is within reach of the finger, which it rarely is in a case of enlarged prostate.

The next thing to be done is the extraction of the stone with the forceps; and, simple as it may appear to be, there are several things to be attended to in this part of the operation.

The forceps are to be withdrawn from the bladder in the direction of the external wound. For the most part, it is better that the convexity of one blade of the forceps should be turned upwards, and that of the other blade downwards. Attention to this point is especially of consequence in cases where there is an enlarged prostate gland, forming a tumor projecting into the bladder. The smooth convex surface of the blade of the forceps is not interfered with by the projection; whereas, if the forceps are turned in the other direction, the stone coming in contact with the tumor, becomes, as it were, entangled by it, and the extraction of it is rendered difficult. The stone must be grasped with a certain degree of force, otherwise it may escape from the forceps. But on the other hand, it is important that you should take care not to apply so much force as to crush it, for this will make the operation not only more difficult, and tedious, and painful, but also increase its danger. You should always endeavour to satisfy yourself, before you proceed to the operation, what is the probable nature of the stone, in order that you may judge how far it is, or is not, likely to be easily broken. The lithic acid calculus is of a very hard texture, and is broken with difficulty. The oxalate of lime calculus is also hard, but it is more brittle than the lithic acid calculus. If the urine be alkaline, without containing the adhesive mucus secreted by the bladder, you know that the external layer is composed of the triple phosphate, and a calculus of this kind is much more easily broken than either of those which have been before mentioned. But the most brittle of all, and that which requires the greatest degree of caution in its extraction, is the fusible calculus, formed partly by the triple phosphate of the urine, and partly by the phosphate of lime, generated by the adhesive mucus, secreted by the membrane of the bladder; and the greater the quantity of the adhesive mucus, and the larger the proportion of the phosphate of lime, the more liable is the calculus to be crushed beneath the pressure of the forceps.

If, having seized the stone, you find that it cannot be readily drawn through the neck of the bladder, you are to bear in mind, that this may be because you have hold of its long diameter. Let it then drop out of the forceps, and endeavour to seize it in a more convenient manner. In some cases you will find it expedient to dilate the wound of the prostate by a second incision. This, however, is never proper, except where you have divided only one side of the prostate in the first instance. You may then introduce a straight probe-pointed bistoury, and make an incision in the opposite or undivided side of the prostate. But this is to be done with the greatest caution. A careless incision may occasion a frightful hæmorrhage, or it may extend beyond the boundaries of the prostate into the cellular texture external to it, and I have already explained to you how much this may endanger the life of the patient.

It is scarcely possible for me to say too much as to the caution necessary in the extraction of a large stone. You must command, not only all your skill, but all your patience; indeed, patience is here the greatest indication of skill. You are to draw out the stone gradually, endeavouring to dilate the parts through which it is to pass, instead of tearing them, and it is astonishing to what an extent this gradual dilatation may be accomplished in the hands of a prudent surgeon. I have told you how important it is that you should avoid crushing the stone. But even this rule has its exceptions. A stone may be so large that no degree of gentleness and caution will enable you to extract it entire without extensive laceration of the neck of the bladder, extending into the surrounding cellular membrane, and under these circumstances, it is the smallest of the two evils that it should be broken into pieces. The fragments are to be extracted one after another, larger or smaller forceps being used according to circumstances. Some of the smaller fragments may be removed by means of this instrument—a kind of steel spoon, to which we gave the name of a scoop; and the very smallest of all may be washed out of the bladder by introducing the pipe of a syringe into it, and injecting a sufficient quantity of tepid water. You are to ascertain, at last, whether the whole of the fragments are extracted, by exploring the cavity of the bladder carefully by means of this straight sound introduced by the wound, and in most cases also by examining it with the finger.

When a fusible calculus, containing a large proportion of the phosphate of lime, is broken, it often happens that some of the fragments are of so small a size that they remain like particles of coarse sand in the bladder, even in spite of all the precautions which you can take at the time of the operation, and further attentions are required. Let the patient recover of the first effects of the operation: then once or twice daily introduce a catheter by the urethra into the bladder, and inject half a pint of tepid water through it, by means of an elastic gum bottle. The water flowing in by the catheter will flow out by the wound, carrying the particles of sand with it; and thus, at last, the bladder will be emptied of them. In a case of enlarged prostate, indeed, this plan may not answer, as it often happens that the patient is not more able after the operation to empty the bladder by the wound, than he was before to empty it by the natural passage. For these cases you must be provided with a catheter of a large size, having an aperture five times the size of that commonly made, close to the point, on the upper or concave side. The tepid water being injected by the catheter will be discharged by it also, carrying every time some of the small fragments of calculi with it, until none are left in the bladder.

It very rarely happens that you meet with an encysted calculus where you perform the operation of lithotomy. In fact, in the great majority of cases of encysted calculi, the bladder is diseased; so that they are quite unfit for the operation. However, such an event occurs occasionally. A boy, about sixteen years of age, was admitted into the hospital in the year 1816. He had suffered a long time from stone in the bladder. There were these remarkable circumstances in his case—namely, that the stone could sometimes be felt distinctly with the sound, appearing to be of a large size, while at other times it could not be felt at all; and that, sometimes, when the bladder was empty of urine, it could be perceived distinctly with the finger from the rectum, while at other times, when there was urine in the bladder, it could not be detected at all by this mode of examination. In performing the operation, when I had introduced my finger into the bladder I could, at first, discover no stone. At last I felt it on the anterior part of the bladder, behind the pubes. It was not lying loose in the cavity of the bladder, but evidently contained in a cyst, communicating with the bladder by a round opening. By means of a probe-pointed bistoury, I carefully dilated the orifice of the cyst, and then, introducing my finger, separated the membrane of it from the stone, until I was enabled to take hold of the stone with the forceps. The stone is preserved among those in our Museum. It was not only an encysted stone, but an adhering one also, for it was brought away with a portion of the membranous lining of the cyst closely attached to it. The boy recovered.

After the operation your patient is to return to his bed, where he is to be laid on his back, with his shoulders and loins as much elevated as they can be without inconvenience, so as to make the wound in the perineum as depending as possible. The thighs are to be somewhat elevated by a bolster placed under the hams, and the knees are to be a little asunder. The urine flows, not through the urethra, but through the wound; and the first, and two or three succeeding gushes of it usually give the patient a good deal of smarting pain. In many cases, where there has been a deep perineum, and especially where the stone has proved to be of a large size, I have introduced an elastic gum canula through the wound into the bladder, and allowed it to remain for the first two or three days—that is, until there was time for the surrounding parts to become consolidated by inflammation. Such a canula makes an excellent conductor for the urine. It keeps the bladder always empty, and prevents the pain which otherwise is experienced on the first passage of the urine. It prevents also that obstruction to the flow of the urine which sometimes occurs after the operation, in consequence of the wound having become plugged by a coagulum of blood. In cases in which the stone has been of so large a size as to make it probable that, in the extraction of it, the soft parts have been lacerated beyond the boundaries of the prostate, the canula will answer another good purpose, by lessening the danger of the urine becoming effused into the cellular membrane.

In ordinary cases the after-treatment is very simple. The wound requires little more than attention to cleanliness; for of what service can applications be to a wound, over which the urine constantly flows? It gradually contracts and granulates; and as it does so, the urine begins to flow by the urethra. As the wound becomes more contracted, more urine flows by the natural passage, and usually, in about a month from the time of the operation, the function of the urethra is completely restored, and the wound is healed.

In a few cases there may be a reason for applying leeches to the lower part of the abdomen, and in still fewer it may be right to take blood from the arm. Fomentations applied to the belly are sometimes proper also; and to this we may add, the precautions necessary after most other operations, with respect to, the functions of the intestines, and the diet.

There are cases, however, in which still further attentions are required. Where the bladder is in a state of chronic inflammation before the operation, secreting adhesive mucus, that inflammation is always aggravated by the necessary introduction of instruments at the time of the operation, and there is always an increased secretion of the adhesive mucus afterwards. Again, in some cases, where those symptoms did not exist previously, they are induced by the operation. Now, under these circumstances, the mucus being liable to deposit the phosphate of lime, and the whole of the urine being rendered alkaline, there is a great liability to a calculous formation, and it will often require much care to prevent this calamity coming a second time upon the patient. Opium, mineral or vegetable acids, and especially the decoction of the *Pareira Brava*, may be here resorted to with advantage:—but I need not occupy your time by a detail of the treatment which is proper under these circumstances. It is sufficient for me to refer you to what I said on this subject in the first of my lectures on calculous disorders. In some of these cases, the whole surface of the wound becomes encrusted with a white calculous deposit. Stimulating applications to the surface of it are then likely to be useful, such as a lotion of decoction of bark and tincture of myrrh, solution of the nitrate of silver, or of nitric acid. As by other means the urine is brought into a more healthy condition, these lotions promote the separation of the concretion from the surface of the wound, which then gets into a state to granulate and heal.

[To be continued.]

MEDICAL JURISPRUDENCE.

FATAL CASE OF POISONING WITH CORROSIVE SUBLIMATE;

With the Analysis and Detection of the Poison after incessant Vomiting and Purging, through a Survival of Eight Days.

By Robert Venables, M.B.
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It may, perhaps, at first sight appear that the details of a case of poisoning with corrosive sublimate could in these days present nothing of either novelty or interest to justify the medical jurist in submitting them to the consideration of the profession. In my apprehension, however, the phenomena of poisoning are not as yet so familiar nor so far established as to render any farther accumulation of facts unnecessary. On the contrary, though the details which I purpose to submit should be found to present nothing new, yet, as tending to confirm in part, at least, what has been already observed by others, they therefore appear to me of sufficient interest to claim the attention of the profession.

General History. — On Friday, the 27th of May last, my friend, Mr. Marten, of Billericay, surgeon, waited upon me to request my advice and co-operation in the investigation of the following circumstances:—On the day preceding, Thursday the 26th, Mr. Marten left home at an early hour in the morning, to visit his patients; and upon his calling in the usual course, between 12 and 1 o'clock, upon Mrs. Gladwyn, one of them living at South Green, and who had been for some time under his care, he was informed that he had been sent for at an early hour in the morning to visit the servant girl, Ann Reeve, who had been taken suddenly ill in the privy soon after getting up; but in consequence of his absence from home he did not hear of the circumstances till his visit at the usual time, specified above. Mr. Gladwyn, after acquainting him with the circumstances as above, handed to him a paper parcel, which in the course of the morning he observed lying among the filth of the privy, and which he contrived to get out. Mr. Marten on opening the paper found that it contained two other separate parcels,—the one marked "Poison," containing a large quantity of what was apparently, and subsequently on analysis proved to be, corrosive sublimate, in large crystalline caked masses; the other, about an equal weight of blue copperas, or sulphate of copper. Furnished with these articles and the particulars of their discovery, Mr. Marten proceeded to visit the servant girl. She told him that very soon after she got up, she was seized with severe vomiting and purging, with violent pains in the stomach and all over the abdomen. What had been vomited had not been preserved, and to Mr. Marten's repeated inquiries she denied being able to conceive, much less to assign any cause, for the symptoms. Mr. Marten then drew the parcels from his pocket, told her he had what would convince her that he was better acquainted with the circumstances than she imagined, and that it would be useless to prevaricate, or attempt longer to deceive him. She then stated that a fellow servant of the name of Jas. Low had procured the articles produced at her own suggestion, and given them to her; and that she had taken that morning about the bulk of the first joint of her little finger* of each, dissolved in a tea-cupful of hot water; that immediately after she was seized with violent gastric and enteric pains, to which speedily succeeded vomiting and purging. Mr. Marten finding that vomiting had already taken place to an extent sufficient to dislodge the whole of the poison, immediately administered the whites of eggs, to decompose any portion that might per-chance have resisted the decomposing power of the gastric secretions, or dislodgment and ejection by the inverted action of the stomach. Mr. Marten also communicated to me that he had received a pocket handkerchief, upon which she had been sick at a considerable interval, after having swallowed the poison. I requested him to be very careful in preserving the handkerchief till an opportunity of examining it presented. On Tuesday, the 31st of May, Mr. Marten again called upon me, requesting that I would accompany him to visit the girl, who, he told me, notwithstanding bleeding and the usual means to abate the severity of the symptoms, still continued getting worse in despite of every remedy. I accompanied my friend, and found the patient suffering from pain—not acute—generally all over the abdomen, but more severe from the umbilicus to the left loin, and extending thence towards the groin in the course of the left ureter. As the pain had abated, so the vomiting had ceased, but she was much troubled with singultus. There was tenesmus, with scanty muco-sanguinous discharges from the bowels; and she stated that the vomited matters from the stomach were mixed *with blood.* There was a cold clammy perspiration, the respiration hurried, the countenance pale and anxious, and somewhat bloated; the pulse small, frequent, quick, and thready; the tongue white and shrivelled, but she did not complain of tenderness of the mouth or gums; there was no fœtor, nor did salivation supervene throughout. There was a total and permanent suppression of urine; and that there might be no room for doubt upon this subject afterwards, Mr. Marten at my suggestion introduced the catheter, but no urine flowed. She stated that the medicine had a very disagreeable taste; but I did not think it right, at so remote a period after her examination before the magistrates, when she inculpated Low, to put *leading* questions to her, lest she might have been betrayed into framing her answers from my questions, rather than from her own sensations at the period referred to. I merely prescribed a little Ol. Ricini, with the subsequent exhibition of a dose of Liq. Opii. Sed. Nothing, however, seemed capable of arresting the fatal progress, and she died on Thursday, 2d June, having survived the ingestion nearly eight days.

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* This bulk, I find, of corrosive sublimate weighs about two drachms; of the copperas, about a scruple, or half a drachm.

On Friday, the 3d of June, Mr. Marten and myself proceeded to open the body between two and three o'clock in the afternoon.

Externally.—On separating the thighs, the vagina and anus appeared shrivelled and corrugated, and there was a kind of greenish-looking discharge, small however in quantity, about the os externum. The tongue looked white and shrivelled, with a slight appearance of desquamation. The gums were white, but not turgid; there was no appearance of previous salivation, nor of ulceration. Neither was there the slightest indication of any degree of mercurial erethysm.

Internally.—On opening the thorax, the lungs seemed generally healthy. At the root and posteriorly they seemed turgid; but this turgescence seemed the after consequences of death rather than the effects of disease during life. The heart was slightly turgid, especially the substance of the ventricles. There was no other unnatural appearance observed in the thorax; nor were those of such a character as unequivocally to infer sensible disease.

Abdomen. — On opening the abdomen, we found in the cavity of the peritoneum a very small quantity of serum, but tinged with blood. The liver was generally healthy, but on the concave surface there was a slight degree of turgescence. The gall-bladder rather diminished in size, but distended with dark green bile. The peritoneal covering of the stomach was highly vascular and inflamed, the vessels becoming more numerous and more distended towards the pylorus. The omentum, especially close to its connexion with the stomach, contained a great number of crimson-coloured vessels, much distended. On the superior surface of the stomach, about midway between the cardiac and pyloric orifices, we observed a very remarkable kind of opaque, yellowish, white spot, of an irregular form, and about the size of a sixpence. The substance of the stomach generally was soft and flaccid; but for about four or five inches from the pylorus, it was very much hardened and thickened, feeling as if some round hard substance was passed into the pylorus. This hardening and thickening extended into the duodenum, which was similarly affected through a considerable portion of its length. The duodenum externally was much more vascular than natural, but of a dark purple appearance. This vascularity extended to the beginning of the jejunum, from thence becoming much less remarkable, till at length it gradually disappeared. But throughout the length of the large and small intestines, we observed occasional patches of preternatural vascularity of small extent, the intervals being of the natural and healthy appearance. The pancreas was very much hardened, and its connexion with the viscera so consolidated, that they could not be separated without tearing the intestine. The spleen was harder than natural. Both the kidneys were enlarged, flaccid, and vascular; the *left* much more so than the right. The left kidney and its peritoneal covering were very vascular, and the vessels turgid, and the ureter leading from it in a similar condition, the vessels of a purple colour. The bladder was perfectly empty, and very much contracted in size, and the internal coat evidently in a state of irritation, from distended vessels. The uterus filled nearly the entire pelvis, but had not risen above it. On feeling it, it was found to contain a fluid, with something hard floating in it. The fundus of the uterus was very vascular, as if from the attachment of the placenta. On opening it, we found it distended with the waters, with a fœtus fully formed, excepting the genitals, which were not as yet perfectly developed, floating in the waters, attached by the cord and placenta, *not* to the *fundus*, but to the back part of the uterus. And at the place of attachment, the increased vascularity was very apparent externally, as at the fundus.

We now proceeded to remove the stomach and duodenum, for the purpose of examining the condition of these viscera internally, and their contents. In doing this, while endeavouring to separate the duodenum from its adhesions, it ruptured towards the jejunal connexion, although there was little or no force used, proving a dissolved or corroded state of the intestine. A ligature being placed a little above the cardiac orifice, and another on the duodenum, these viscera were removed, and on opening the stomach it was found to contain about ten or twelve ounces of fluid matter, containing a large proportion of blood, with a little oil floating on the surface. There were no lumps or coagulums. The fluid contents were poured into a basin, and the surface of the viscera was washed with water, the washings being added to the contents in the basin.

We found the mucous coat in a few places, where not entirely corroded, much inflamed, Internally corresponding to the portion already noticed, as feeling so hard externally, there was an elevated hard patch, of a dark olive colour, nearly approaching to black. This morbid condition extended over a considerable portion of the stomach, occupying the pylorus, and nearly a third of the duodenum. It was of an irregular figure, elevated, and the coats, as it were, thickened in this part. It was no doubt the black warty condition mentioned by Dr. Christison, as the result of extravasation of blood into the coats and substance of the viscera, from the action of the corrosives. The mucous membrane of these viscera seemed to have been almost entirely corroded and discharged, for there was hardly a trace of the rugæ or plicæ which, in the healthy state, characterize the internal surface of the stomach. The black olive patch was of an irregular oblong figure, and of about the breadth of two five-shilling pieces. It occupied the under surface, where it might have been presumed the poisonous solution lodged.

The œsophagus, at its connexion with the pharynx, was preternaturally vascular and dark; but the rest of its length was rather white, and somewhat hard, or more firm in its consistence. This finishes the case up to the period of the inquest, when we were called upon to declare our opinion as to the cause of death upon the above facts. Let us now inquire how far we were authorized upon them to criminate the prisoner, or rather to infer death from poison, and the particular or individual poison.

The first question which presents is, could the mere symptoms justify the conclusion of poisoning? My own opinion is, that the answer is in the affirmative. It is true that I was not personally a witness of all the symptoms; but medical testimony often consists in conclusions drawn from facts stated or observed by others. In the first instance, we find a woman in perfect health rise in the morning, free from any complaint, and almost immediately after swallowing a tea-cupful of hot water, in which *something* was dissolved, become affected with severe vomiting and purging. This certainly might be a case of cholera morbus, which often attacks very suddenly, without the slightest premonitory symptoms. However, suspicion is excited on finding that the symptoms immediately supervened the ingestion of some fluid or solution into the stomach. Then we are to inquire into the character of the vomiting and purging. A most important feature was the early appearance of blood in the stools and the *vomited* matters. Even at the advanced period of my visit, the mucus and blood in the stools, and the attending tenesmus, are not exactly consistent with cholera. An inquiry at an early period as to the taste, and the sensation attending deglutition, with the phenomena supervening, would, in all probability, have tended greatly to develop the nature of the case, and determine the description of poison. It may be urged, that there was no salivation: this might be accounted for variously. All the poison might have been ejected by vomiting, and none left for absorption, so as to have excited the specific action of the mineral. Again, I have met with several instances in which it was impossible to excite any mercurial action in the system; therefore, the absence of salivation, though, had it been present, it would have confirmed the species of the mineral, is no objection to the nature of the poison. Nor are these mere idle speculations; for had the woman lived, the prisoner would have been tried for the misdemeanor, and, of course, the origin or cause of the symptoms would have been a question. Of course the species of poison might have been a matter of doubt, but a diligent inquiry into the facts and collateral circumstances, would have enabled the medical jurist to arrive at a tolerably accurate conclusion.

The case having terminated fatally, we are next to inquire what information is to be derived from the appearances on dissection; and here, I think, there are strong grounds for a definite conclusion. The signs of inflammation and irritation throughout the viscera; the black warty extravasation in the stomach and duodenum; the corrosion of the villous coat, and the decomposition, or almost solution, of a portion of the duodenum, together with the blood found intermixed with the contents of the stomach, are sufficient to prove the corrosive nature of the poison, while the taste, perhaps, would enable us to decide its specific character. The reader will here perceive that I have entirely left out of consideration the articles found in the privy; but if they be brought forward, and their composition determined, there could scarcely be a doubt, without once taking into consideration the discovery of poison in the contents or tissues of the viscera.

But although the medical jurist may feel fully satisfied in his own mind, and may deem further inquiry unnecessary, so far as his own satisfaction is concerned, yet it will be always the most prudent plan to inquire into every possible contingency, and to institute every investigation of which the case is susceptible. Upon this principle I undertook the analysis of the hankerchief upon which the deceased had been sick, as well as of the stomach, &c. and its contents. The contents and washings, &c. before described, were put into a bottle, which was corked up and sealed with my own private seal, and committed entirely to my custody. The stomach and duodenum were folded up in a piece of bladder, and, with the handkerchief and the bottle of contents, put into a basket, which I brought home with me; and in a day or two I commenced the analysis. The handkerchief, and the stomach, and duodenum, were cut into small pieces; and here I may remark, that, on cutting the black part of these viscera, the blackness in most parts went through half its thickness, and in some places through about two-thirds. The small pieces were put into Florence flasks, and boiled in distilled water, the water being added to the contents in the bottle. The fluid was now treated with protochloride of tin, according to Dr. Christison's method; but notwithstanding every possible care, I could not discover the slightest particle of mercury. I next boiled the solids in a solution of chlorine, and treated the solution with protochloride of tin, as before, but I found no mercury. I now sealed up the flask containing the fragments of the stomach and duodenum, and put them by, while I instituted a number of experiments to discover the most efficient method for evolving mercury from its various combinations. After a number of varied experiments, which it would be hardly relevant to describe here, I was induced to fix upon the following, which with the result I shall now proceed to detail.

The flask, containing the stomach and duodenum cut into very small fragments, being unsealed, nitric acid in considerable excess was added, and being frequently agitated, was allowed to exert its action for several hours. In my experiments I found that nitric acid dissolved mercury under every form—from its most insoluble salts or combinations, to the actual metal itself —therefore I concluded, that if mercury existed in any form in the tissues of the stomach, the acid would take it up. Now when mercury is dissolved in nitric acid, if chlorine be passed through it, the nitrate is converted into corrosive sublimate. Therefore a solution of chlorine was added, and a stream of chlorine gas passed through the whole. The whole was now well boiled, so that the animal matter might be destroyed by the boiling nitric acid. After boiling a sufficient time, it was strained through a coarse cloth, and then filtered through filtering paper; a transparent though coloured fluid was obtained. The clear liquid was put into an evaporating dish, and evaporated down; a solution of chlorine being added as often as the fluid during evaporation became thick, or sensibly coloured. The evaporation was continued (adding chlorine occasionally) till it ceased to affect litmus paper in any very sensible degree. It was now evaporated till very concentrated, when a little distilled water being added, it was boiled and transferred into a tall glass jar. The fluid having been allowed to cool, a piece of litmus paper being thrown in was faintly reddened; and a piece of turmeric being also added, caustic potass was added till the fluid became perfectly neutral. I now poured in protochloride of tin, when a slate-grey colour was immediately struck. Upon standing, a dark-coloured precipitate subsided, which was allowed twenty-four hours to collect, when the supernatant fluid was poured off, and the precipitate transferred to a smaller tube well washed with distilled water. It was now transferred to a watch-glass, and the powder on subsiding and collecting had a *very black* appearance. The water was removed by the capillary action of cotton wicks, and the powder was then dried by the side of a stove. When dried, by the aid of a lens innumerable globules of quicksilver were discovered; but they were so minute, that with the aid of a good lens even, their globular form was not distinct to persons unaccustomed to looking at such minute globules. To myself the globular character was very satisfactory and distinct. The dried mass, however, was put into a *ball-tube*, such as figured in Dr. Christison's plate for the subliming of arsenic; and on applying the flame of a spirit-lamp, a kind of metallic dew was raised into the narrow neck of the tube. The neck was heated, and the ball separated. The closed part of the neck was now removed by a file, and, with a lens, the dew was found to consist of a number of minute globules. By applying the point of a pen-knife they were united into one, which thus gave unequivocal indications of the mercurial character. I estimated the quantity thus obtained to be about 1-16th of a grain of metallic mercury. Not being provided with decimal weights, I weighed out a grain of metallic mercury, and divided it into sixteen equal bulks, and I found that one of these and the mercury I discovered were as nearly equal as possible in bulk, and precisely equivalent in weight.

I must here offer a word or two upon the mode of analysis which I adopted. To have followed Dr. Christison's plan with the *solid substance of the viscera*, would, I fear, have left such a quantity of animal matter—and which the boiling caustic potass would by no means remove—as, by its empyreuma, would have completely prevented the development of the mercury during the distillation, or sublimation. The method of analysis here described I consider as some improvement, because the nitric acid, having in a great measure destroyed the animal matter, and the mercury being held in solution, and then precipitated from the filtered liquor, is obtained by the subsequent sublimation without any inconvenience from empyneuma. When the contents of the stomach contain parsley, cress, tea-leaves, or dust, or green vegetables, minutely divided, it is impossible to remove them; and therefore, if Dr. Christison's method be followed, the black powder having been dried, should be subjected to the action of nitric acid, to dissolve out the mercury. Chlorine should then be added; and this is an important part of the process, because, by the addition of chlorine, the nitrate is converted into corrosive muriate or sublimate, by which means precipitation is prevented by dilection. To the solution thus prepared, and if necessary neutralized, if proto-chloride of tin be added, the mercury is precipitated in the metallic form. Should these (as happens in some very peculiar cases) occur, any precipitation of animal matter, with the protochloride of tin, or should the oxyde of tin itself precipitate, the precipitate is to be treated with caustic potass, and, if necessary, boiled with it. The potass will dissolve both the animal matter and the oxyde of tin, and will leave the black powder, which is mostly metallic mercury perfectly free from animal matter, and which, on being thoroughly dried, generally runs into globules. If they should not be sufficiently distinct, the black powder should be put into the tube before described, and sublimed, and the mercury is rendered beautifully polished and pure, if it be mixed previously with a small proportion of *clean bright* iron filings, and distilled from these. The analysis in this case afforded sufficient evidence of death from poisoning by some corrosive preparation of mercury. For instance, the question might be put, would not calomel, blue pill, the grey oxyde, or any other, of the milder preparations of mercury, administered in medicinal doses, have yielded, under similar treatment, a portion of metallic mercury equal to that obtained? Undoubtedly: but that could not have been the case in the present instance, because the milder preparations so administered would have been found in the contents, or in the washings of the stomach; and not so intimately combined, or, as it were, amalgamated with the tissues and substance of the viscera. It is by virtue of its corrosive action that the mineral forms so intimate a combination with the animal tissues, and therefore none but a corrosive preparation, and that too in a dose sufficient to produce corrosion, could have yielded such results. Therefore the medical jurist is authorized to infer, that no other than a corrosive preparation could have produced the corrosion; and corrosion being proved by the mode of analysis, the dose being adequate to such an effect, must have been sufficient to cause—nay, actually did cause, the death.

With respect to the delicacy of the process adopted, I believe it will be found equal, if not superior, to any hitherto suggested as applicable to medico-legal purposes, while it is also free from many of the inconveniences which attend in particular cases all the others that have been recommended. In order to ascertain whether any mercury was lost by this process, I subjected what remained on the coarse cloth and the filter to destructive distillation, but not a particle of mercury was yielded from these materials. The evolution of one-sixteenth of a grain of metallic mercury, from its combination with the substance of the whole stomach and duodenum, I think sufficient proof of the extreme delicacy of this method of analysis; besides which, there is a great advantage in having the black powder precipitated free from any admixture that can obscure the subsequent sublimation, which is seldom necessary, unless the quantity of mercury be extremely minute.

It is not always that mercury can be detected in cases of poisoning, or at least so it is generally asserted. Undoubtedly corrosive sublimate, from its solubility, is readily ejected by vomiting, and may thus be removed beyond the reach of chemical analysis. But, perhaps, if the tissues of the stomach, duodenum, &c. were subjected to such an inquiry, I cannot help thinking, that in most fatal cases, where life was not prolonged to any remarkable length of time, mercury may be discovered. In the present case, death was protracted to an unusual length of time; indeed I believe there is no instance on record of death being so long deferred in this variety of poisoning with the corrosive preparations of mercury. Dr. Christison arranges the cases of poisoning with mercury under three heads, or varieties. In the first variety the symptoms are merely those of irritation in the stomach and bowels—namely, pains in the pit of the stomach and all over the abdomen; the pains being much increased on swallowing or taking any thing into the stomach. With these attend severe vomiting, and profuse diarrhœa. As the case advances the symptoms of cholera change into those resembling dysentery, and we have tenesmus, scanty muco-sanguineous stools, and the vomited matters are often mixed with blood. In this way the case proceeds to a fatal termination, which, according to Dr. Christison's inquiries, usually occurs in from twenty-four to thirty-six hours; but the longest duration, in fatal cases, he states to be *three* days—the shortest, eleven hours. The case, however, now detailed, unless it prove an anomaly, certainly invalidates the above rule; for it did not terminate fatally till after a duration of seven days, and, from the most accurate account I can obtain, eight hours and a half.

I have not taken any notice of the blue copperas, for I could not detect a particle of copper; and, indeed, I do not conceive that it had any share in the poisoning of the patient. On the contrary, it is not improbable that it may have acted by exciting vomiting, and have so contributed to the discharge of the greatest part of the corrosive sublimate, and perhaps may have contributed, in some degree, to defer the fatal termination to so unusual a length of time. Be that as it may, I find that the solutions of corrosive sublimate and of sulphate of copper, when mixed, exert no sensible chemical action upon each other, though allowed to remain so mixed for a month. It is probable that the deceased used the boiling pump or hard water of this neighbourhood, to effect the solution of the two salts. I need not remind the reader that, in such a medium, both salts (the corrosive sublimate and sulphate of copper) would undergo a partial decomposition. Whether the resulting compound of mercury still remained a corrosive poison, though less rapid and active in its operation, it is difficult to say; and whether the solution containing corrosive muriate unaltered was ejected from the stomach before it had time to exert any deleterious influence, I have found it impossible to obtain any satisfactory information sufficient to form even a conjecture, much less a probable conclusion.

Having now detailed the principal circumstances interesting in a medico-legal point of view in this case, I feel it a duty to offer some observations to the public upon the nature of the crime charged. It is to be deplored that the public ignorance should be so great, and that persons should risk their lives in attempting to perpetrate a crime which, as a general principle, it is impossible to commit. The prisoner in this unfortunate case, and the deceased, had indulged in an illicit intercourse; and the deceased, believing herself with child, was desirous of relieving herself of her burthen sooner than, in the natural course, was likely to happen. Accordingly she directed the accused to purchase for her corrosive sublimate and blue copperass, with the intent of procuring abortion, by taking it internally. The accused, by procuring the medicine with the criminal knowledge, rendered himself guilty of any consequence that might ensue; and death having succeeded, he of course became in *law* a *murderer*, and *amenable* to *all* the punishments attached to this crime. Now it is deplorable to witness cases of this nature, and to see individuals incurring all the responsibilities attached to crimes which they cannot perpetrate. It is a universally admitted fact, that there is *no known* medicine which, by its internal administration, can produce the miscarriage of a pregnant woman, Such a result may happen by accident, but never from medicine, unless through the death of the mother involving that of the child, by cutting off the maternal supplies necessary to the support and growth of the fœtus. In the present unfortunate case I am satisfied that the life of the fœtus was not only prolonged to the latest moment of the mother's existence, but that it even outlived its parent. Indeed it seems impossible to conceive how medicine, internally administered, could destroy the vitality of the child, without conceiving that it could, by some unknown route, enter the vessels and organs of the fœtus without passing through the maternal circulation. That there might be nothing left to question, I analyzed the fœtus in this case, but could not detect a trace of mercury. The only other way in which internal medicine could contribute to abortion, is by the inverted action of the stomach exciting or irritating the uterus prematurely to expel its contents. At an early period of utero-gestation, retching and sickness prevail to a great extent, without involving such a result; and we know of no safe means of acting on the nervous system without endangering the health, even the life, of the mother. Therefore, upon the whole, we may lay it down as an established principle, "that there are *no known medicines* capable, by their internal administration, of *suspending, suppressing, or in any way subverting*, the natural progress of utero-gestation; a principle which, I trust, the public press will not fail to disseminate, and, as much as in their power, to inculcate.

CASES OF POISONING BY SULPHURIC ACID.

By Martin Sinclair, M.D.

Fellow of the Royal College of Surgeons of Edinburgh, &c.

Case I.—John Capper, aged 27, and of a phthisical habit, swallowed on Tuesday the 29th of June, three fluid ounces of concentrated sulphuric acid, which was immediately followed by excruciating pain at the stomach, retching, and violent vomiting. When called to the patient, about fifteen minutes after the draught had been taken, I found him labouring under these symptoms in a very aggravated degree,—the pain at the stomach being most severe, insomuch that he was compelled to keep his body in a bent position, supported by two individuals,—and the retching and vomiting almost incessant. About four ounces of prepared chalk diffused in a quart of milk were immediately exhibited*, which the patient swallowed in divided portions, at short intervals. A similar mixture, with the addition of one ounce of castor oil, and two ounces of olive oil, was injected per anum. By these means the sufferings of the patient were somewhat diminished,—the pain at the stomach being less severe, and the vomiting, although more copious, and accompanied with blood, easier, and free from acrid eructations.

The patient was left in a comparatively tranquil state, with instructions to take freely mutton broth and other stimulants. The following mixture was also prescribed:—

℞ Pulveris Opii gr. viij. Cretæ preparatæ ℥iv. Lactis recentis ℔ij. Misce. Sumat æger uncias duas subinde.

6 P. M.—Four hours have now elapsed since the sulphuric acid was swallowed. The patient is extremely feeble and exhausted, the pulse quick and small, and the skin cold and bedewed with a clammy sweat; the pain at the stomach very severe, and the vomiting occasionally urgent, accompanied with the ejection of tough mucus and a dark-coloured liquid resembling coffee-grounds; deglutition considerably impaired, and performed with difficulty, in consequence of which very little of the mixture and mutton broth have been taken. The clyster has been retained.

Pergat in usu medicamentorum et demulcentium.

10 P. M.—Nearly in the same state; deglutition more difficult; the respiration also somewhat impeded.

Pergat ut supra.

30th.—9 A. M. Had a scanty alvine evacuation about eleven last night; passed the night tolerably free from pain, but did not sleep; pulse quick, somewhat fuller; heat of surface moderate; complains of the constant gnawing pain of the stomach, and of the inability to swallow; respiration still impeded.

Sumat Olei Ricini ℥ss. omni bihorio donec alvus responderit.

1 P. M.—Could not swallow the castor oil.

℞ Olei Ricini, Olei Terebinthinæ aa. ℥ij. Decocti Avenæ ℥xxviij. Injiciendum per anum partitis vicibus.

6 P. M.—Clyster retained; no evacuation from the bowels.

℞ Olei Crotonis Tiglii gtts. ij. Sacchari Albi ℥ss. Misce et divide in chartulas duas, quarum sumat æger unam statim; repetatur dosis, duabus horis exactis, si opus sit*.

10 P. M.—The exhibition of the croton oil produced an acrid burning sensation in the fauces, followed by violent vo-

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* An attempt was made to throw this mixture into the stomach by means of the stomach pump; but, owing to the excoriated state of the œsophagus, this was found to be impracticable.

* As the croton oil produces a pungent sensation in the healthy fauces, it was exhibited, in the present instance, to ascertain its effects upon the excoriated fauces.

mitings, which precluded him from attempting to take the second dose; symptoms nearly the same as before; feels extremely weak.

1st July.—9 A. M. One scanty stool last night; pulse feeble; complains much of the gnawing pain at his stomach, and of an oppression in his bowels, "from want of a free passage through him;" he hangs his head over the bed, supported on a chair, constantly hawking up a tough mucus, which produces a sense of suffocation; breathing laborious; deglutition almost impracticable, and attended with a convulsive cough. He is perfectly sensible of his situation, and prays for death to relieve him of his sufferings.

℞ Pulveris Aloes ℈ij. Decocti avenæ ℔iij. ft. enema statim injiciendum. Injiciantur étiam subinde jusculi Bovini ℥viij.

1 P. M.—No evacuation of the bowels; pulse feeble; respiration laborious; can swallow only a tea-spoonful of liquid at one time, and that with the greatest difficulty; still complains of the oppression in his bowels. The cuticle covering the tongue was detached this morning.

℞ Infusi Sennæ ℥ij. Sulphatis Magnesiæ ℥ss. Tincturæ Sennæ compositæ ℨij. fiat mistura partitis vicibus sumenda.

6 P. M.—Is much weaker; no amendment.

℞ Tincturæ Hyosciami ℨi. Tincturæ Lavandulæ compositæ ℥ss. Aquæ Puræ ℥i. fiat mistura cujus sumat æger drachmas duas subinde.

The patient gradually sunk, and expired at 9 P. M. having survived fifty-five hours. He retained his faculties to the last.

Appearances on Dissection.—The internal surface of the mouth, pharynx, and œsophagus, was completely corroded, and in the course of being detached; that of the mouth, commencing at the lips, being of a whitish, and that of the pharynx and œsophagus of a brownish, colour. The rima glottidis was contracted; the epiglottis swollen, and, with the top of the larynx, much inflamed. The cardiac orifice of the stomach was hard and contracted. The stomach contained a pultaceous mass, which consisted of carbonate and sulphate of lime, bile, and the secretions of the stomach. When cleared of these substances, the cavity of the stomach was found to be much diminished in size; its internal surface was contracted, corrugated, puckered up into folds similar to the valvulæ conniventes of the small intestines, but much larger. On the great arch, towards the cardia, a large patch in a high state of inflammation was observed, where the sulphuric acid seemed to have expended its influence; the colour was in some parts of a deep red, in others of a chocolate brown, and in others verging to black; the consistence of this patch was firm, without any separation of its layers; its size was about four inches in diameter. On the great arch, also, about three inches from the pylorus, which was healthy, a softening and corrosion was observed, but without any appearance of surrounding inflammation. This alteration of structure did not appear to arise from the action of the sulphuric acid, but to originate from the post-mortem agency of the gastric juice. The intestines, principally the smaller, were distended with flatus, and highly inflamed on the peripheral surface, which was tense, shining as if varnished, and beautifully arborescent from the injection of the minute vessels. The internal surface was healthy; the omentum was also inflamed; the remaining abdominal viscera were healthy. The brain and thoracic viscera were not examined.

The above case is not presented to the notice of the profession as containing any thing novel either in the symptoms or in its mode of treatment, but as exhibiting an incontrovertible instance of the effects of sulphuric acid upon the human system. The subject of the case, John Capper, was a young man of regular habits, and of considerable genius; but having been seduced from the paths of rectitude and virtue by a married woman, who taunted him with her affection, promising him marriage when her husband died, and afterwards deserting him, he was impelled to commit this suicidal act, "to be revenged upon himself for having placed any affection on such an unworthy individual."

The case, however, is interesting to the physiologist and practical physician. To the physiologist it exemplifies in a remarkable degree the conservative energy of the stomach in resisting the influence of the most potent irritants; and when we contrast this with the post-mortem influence of the gastric juice, in causing erosions and perforations of the stomach, as exemplified in the writings of Hunter, Gairdner, and Carswell, we cannot but exclaim that the body is "fearfully and wonderfully framed." As Dr. Christison observes *, "When made aware of the rapidity with which other irritating poisons prove fatal, and the slight signs they commonly leave of their operation, we cannot fail to be struck with discovering what the animal frame will sometimes endure from these the most violent of all irritants, and nevertheless recover."

To the practical physician, also, the case is not devoid of interest, as exhibiting the effect of treatment by chemical antidotes; and it also teaches us the lesson to administer our remedies, in every case of poison, with the utmost promptitude; and in no case to despair of doing good to suffering humanity. By medical writers, the carbonate of magnesia is recommended in preference to the carbonate of lime, and is certainly superior, by producing a saline aperient, by combining with the sulphuric acid. In the present case, however, the carbonate of lime was preferred as having a greater affinity for sulphuric acid, and because, being more easily miscible with liquids, it could be administered in greater quantity in a short space of time.

I omitted to state that Capper was fasting when he swallowed the sulphuric acid—having taken no food that day excepting a cup of coffee, without bread or any other solid, at 9 A.M.

CASE II.—On Tuesday, the 8th of November, I was instructed by the coroner to open the body of John Hilton Morrabin, aged 4 years 10 months, who died at noon, the 4th of the same month, under suspicion of having been poisoned by his step-father.

The body externally presented nothing unusual, excepting at the upper lip and angles of the mouth, where several spots about the size of a small kidney-bean were observed. These were of a brownish colour, such as is produced upon the cutis by the action of concentrated sulphuric acid. The lining membrane of the mouth and tongue was of a greyish or rather ash colour, and in the course of being detached; and two erosions about the size of a sixpence were observed on the tongue. The pharynx was so contracted that a probe could scarcely pass the strictured part. Upon laying this open, the internal surface was of a reddish-brown colour, and the lining membrane in part detached, and where it remained it had the appearance of moistened parchment, and came off with the greatest facility. The internal surface of the stomach was corroded in some parts, charred in others, and in the great arch a perforation, about 1½ inch in diameter, was observed. The coats of the stomach were much attenuated towards the perforation, and externally were of a very dark colour, nearly three-fourths of an inch around it. The charring could not be removed by washing; and upon part of the charred substance being picked off, the subjacent texture was found of a pinkish or rather delicate crimson colour. The cardia and pylorus were not contracted or otherwise affected; the omentum, liver, intestines, with the peritoneum, were in a state of high inflammation; about six ounces of a deep dark chocolate-brown liquid were found in the stomach and the general cavity of the abdomen, whence it had been effused through the perforation mentioned above. This liquid was, by the instructions of the coroner, analyzed by Mr. Davies, lecturer on chemistry, and was found to contain a considerable quantity of sulphuric acid.

The trachea, larynx, and epiglottis, were much inflamed, and the investing membrane of the latter, as well as of the glottis, was of a whitish colour, and elevated, presenting the appearance of having been boiled. The aperture of the glottis was not, however, contracted.

The symptoms exhibited by the child during life, so far as they were elicited at the trial, were, frothing at the mouth, burning in the throat and stomach, constant writhing of the body, inability to swallow any liquid (from its being ejected by the nose as soon as taken into the mouth), and convulsions of the whole body. These symptoms continued until the child's death, which took place about four and a-half hours after, as was supposed, the administration of the poison. No medicines were administered, as the father "stood over the child till he saw it fairly dead," and "would not send for any doctor."

The step-father, Moses Fernely, who had always been cruel to the child, was tried for the murder at the late spring assizes at Lancaster; was con-

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* On Poisons, page 116.

victed by the clearest circumstantial evidence; and was executed for the awful crime on the 12th of March, and his body given for dissection. It appeared in evidence that the child was quite well about seven o'clock; that it was found labouring under the symptoms described a little before eight; and as it died about twelve at noon, it consequently survived the administration of the poison about four hours and a-half, as has been noticed above*.

Manchester, 4th May, 1831.

EFFECTS OF NITRE ON THE BLOOD.

To the Editor of the London Medical Gazette.

Truro, August 6, 1831.

Sir,

In reading the "Epitome of the Symptoms and Treatment of Cholera," lately published in the London Medical Gazette, my attention was particularly arrested by the important fact therein stated, that the blood drawn from patients labouring under the disease is so black as to constitute a remarkable peculiarity, and to lead to this observation, "that one of the most striking phenomena of the disease consists in the imperfect arterialization of the blood." For this condition of the blood there is, I believe, a specific remedy to be found in nitre—a fact which has long been known to me, and acted upon in the course of many years' practice. There is no medicine, perhaps, in common use, so vaguely administered as this is; but however absurd it may be to expect that the same medicine should be capable of meeting the most contradictory intentions, or be equally applicable to hæmoptoe, the precursor of phthisis, as to sea-scurvy, yet it is but fair to expect that there must be some considerable virtue inherent in a salt which has gained such extensive reputation; and what I am now anxious to press upon public attention is its marvellous effect upon the vital fluid, when in that state in which it is said to be in the prevailing cholera. In the fifth volume of your Gazette there is notice taken of a report from Mr. Charles Cameron to the Navy Board, relative to its astonishing efficacy in scurvy; from which it would seem capable not merely of supplying the place, but of superseding the use of vegetable acids altogether. This report was particularly interesting to me, from the confirmation it afforded of my own previous views, and I have fully expected that additional accounts of the employment of nitre under similar circumstances would, ere this, have appeared; but, having been hitherto disappointed, I feel it the more necessary to endeavour to excite the further attention of medical men to a medicine which I have invariably found of the greatest service in cases where the blood has degenerated into that state in which it exists in purpura hæmorrhagica, in the melœna of malignant fevers, and, as I believe, in sea-scurvy, and in the Asiatic cholera. It is probable that in all these cases the air, conformably with the experiments of Dr. Davy, ceases to undergo the usual healthy change in respiration; but it would be extremely difficult to shew, whether the first departure from health takes place in the functions of the lungs, or in the blood itself, from contagion, intemperance, poverty, and filth, mental depression, and consequent imperfect chylification. Not unfrequently these causes are combined; and imperfect nutrition, vitiated air (whether the vehicle of some specific contagion or not), and depressing mental influences, are associated in one common warfare, and are formidable in proportion as they are so. And, although it would be going much too far to suppose that any medicine whatever should be capable of arresting the fatal progress, in many instances, of such concentrated mischief, yet it may be very possible that, some specific being found for that condition of the system on which fatality mainly depends, the utmost point may be attained that can be expected from medicine. But, even then, attention will be required to all the other auxiliary means and appliances which the stage of the disease, or the modification of particular cases, may demand; which, in fact, is no more than is due to the successful exhibition of all specifics. Accordingly, there seems to be equally strong evidence in favour of the lancet in cholera, provided its use be opportune, as of the fatal tendency of venesection when recourse has been had to it injudiciously. And is not this equally true with regard to malignant fevers and many of the profluvia, the tendency of which is to degenerate into a supercarbonated state of the blood?—into that state where the effect of nitre is so extraordinary. The same remarks apply to mercurial and other purgatives, which have not wanted their full share of commendation. Whilst, therefore, it must be allowed that hundreds may have been saved from the yawning jaws of cholera by the use of the lancet, and others by the powerful action of calomel on the system of the vena portæ, yet still it continues to kill its thousands and tens of thousands; and to this tremendous residue of victims our utmost attention is called, and I have heard of no medicine likely to be of so great service (I will not venture to say, judging from analogy, how great) as nitre. It may have been already tried, and may have failed, and, if so, these remarks can do no harm; but I should even then require to know whether it has been used with due regard to the symptom which it seems to be so calculated to meet, and without neglecting other auxiliaries.

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* Edinburgh Medical and Surgical Journal.

The report of Dr. Stevens on the treatment of Malignant Fevers in the West Indies, for a knowledge of which I am likewise indebted to your valuable Gazette, is very much in favour of these views, only that, with reference to the supercarbonated state of the blood, my recommendation applies to the use of nitre exclusively of the other neutral salts. The carbonate of soda, or the aperient combinations of soda or potash with any of the vegetable or mineral acids, may have their appropriate advantages, but they have not the peculiar effect of nitre upon the blood. Its effects on persons labouring under sea-scurvy are, in fact, described by Mr. Cameron as almost miraculous; and I was as prepared to give the fullest credence to his testimony, as I was gratified by the perusal of it. Combining, therefore, the evidence of Dr. Stevens and others, of the efficacy of neutral salts in the malignant fevers of the West Indies, where the state of the blood is supposed to be similar to that in cholera, with the almost miraculous efficacy of nitre in scurvy, it is at least very desirable that the experiment of its effect in cholera should be fairly made—made with due regard to all the symptoms and varying circumstances of the disease exhibited in different cases. When there is time, and the condition of the patient may seem to require it, venesection and an aperient, as in the fevers of the West Indies, may precede, if they do not supersede, its use; but where the stomach and bowels have already given way, and the powers of life are sinking, the specific, as I would fain consider it, should be had recourse to instantly. It has agreed best when given, in cold water a little sweetened, in a dose of from ten grains to ℈j. every two or three hours; and, in urgent cases even more frequently; but, ordinarily, the repetition of the dose three times a-day will suffice. Mr. Cameron's dose was ʒj. which, I believe, would often be more than the stomach would bear. Still it is impossible to lay down any rule for the extent to which nitre may be given in any particular case; and it may be said generally, that, as long as the stomach is not offended, there is no danger from excess. With respect to diet, the lightest possible food, consisting chiefly of thin farinaceous preparations, is best, and should be persisted in until convalescence is established.

I have further papers by me, illustrative of the above remarks, but there is already evidence enough before the public to justify the trial of a remedy in cholera which possesses such extraordinary power in analogous diseases; and, in the sanguine hope that it may be found of vast importance in correcting that desolating scourge of a widely extended portion of the globe—getting, as it likewise is, daily nearer and nearer to our own shores—

I am, sir,
Your very obedient servant,
C. CARLYON.

CASE OF

SEVERE CHOLERA.

TREATED BY JOHN FORREST, M.D.
Assistant Surgeon, 23d Regiment.

Drogheda, 30th July, 1831.

I WAS last night requested by Dr. Pentland, Surgeon to the Civil Hospital of this place, to see a case of cholera morbus.

The patient was a tailor, about twenty-two years of age. I saw him at four P.M. when he stated, that on getting out of his bed in the morning he was suddenly seized with giddiness, so that he was unable to stand, and which was shortly afterwards followed by severe purging and vomiting, and cramps of his legs.

July 29th, 4 P.M.—Surface cold, but not moist; pulse at the wrist and temples quite imperceptible; constant desire to drink cold water; feeling of load and oppression on his stomach; great restlessness; anxiety of countenance; and severe cramps of his legs, confined principally to the gastrocnemic muscles; his fingers were also occasionally cramped. The only medicine he had taken during the day was a mixture of tincture of rhubarb and peppermint water, but which his stomach immediately rejected.

5 P.M.—He was put in a warm bath, which seemed to give him some relief, but had not any effect in allaying the irritability of the stomach. He then took a pill, consisting of two grains of opium, and five grains of calomel; to be repeated every hour; to abstain from drinking, which has hitherto invariably induced severe retching and vomiting.

6 P.M.—Sinapisms were ordered to be applied to his legs, and to the epigastrium.

7 P.M.—Is much in the same state. The sinapisms having produced a good deal of pain, he tore them off. Cramps not abated; still very restless; stomach not so irritable.

In taking one of his pills just now, we gave him a table spoonful of spirits and water, containing 30 drops of laudanum, which has induced severe vomiting and cramps of his legs.

Another pill without any liquid was shortly after given to him. At this time it was deemed advisable, to prevent after reflection, in the event of the disease appearing more generally in this neighbourhood, to remove the patient from his lodgings to a ward in the Civil Hospital.

10 P.M.—Has not had any cramps during the last hour; stomach less irritable; great desire to drink cold water. Pulse at the wrist perceptible; anxiety of countenance not so great.

A table spoonful of hot brandy and water to be given occasionally during the night. Pills to be continued.

July 30th, 8 A.M.—Took two pills last night after we saw him, when he fell asleep. No vomiting, purging, nor cramps; is greatly improved this morning; countenance not so contracted; surface warm; pulse quick and weak; complains of great thirst.

10 A.M.—Has taken a small bit of toasted bread and some tea; feels himself greatly better, but very weak.

5 P.M.—Continues free from spasms; no vomiting; slight headache; thirst not so urgent; pulse very much improved.

The patient cannot account for his present illness; was never similarly affected before. Is not a dissipated character; and the only malt liquor he took on the day previous to his attack was a pint of porter. Has been in the habit of drinking porter daily.

5th August, 1831.

I have the honour, in reply to your letter of the 2d instant, to state, with reference to the case of cholera morbus, that at the commencement of the disease the matters vomited and passed by stool were of a bilious nature, greenish brown colour; and that as the disease progressed, the matter vomited became of a clear colour, frothy, and seemingly containing a great proportion of mucus, from its tenacious quality, without any admixture of bile.

As to the excrements, they were all the time very watery, and of a highly bilious nature; although, at the time I saw the case, the desire to go to stool was by no means urgent.

He states, as well as the person who attended on him from the commencement of the attack, that he did not make a wine glassful of urine during the day; and what he did void was of a deep brown colour. I inquired into this point before, but omitted making mention of it in my former letter.

I am happy to state, that the man is now perfectly recovered.

TWO CASES OF VOMITING, WITH CRAMP,

Apparently cured by Cajeput Oil.

By R. Macleod, M.D.

Henrietta-Street, Cavendish-Square,
August 8, 1831.

The subject of cholera occupies so much attention, that facts and cases, in any degree connected with it, and such as at another time might, perhaps, be deemed unimportant, excite sufficient interest to warrant their publication at the present moment. It is this feeling which induces me to give the following short account of two cases, which presented some peculiarity in the symptoms, and in which benefit was apparently derived from a remedy which has lately been asserted to prove efficacious in the more formidable disease of India,—I mean cajeput oil.

On Wednesday, August 3d, I was sent for between seven and eight in the morning to Mrs. R. a rather delicate woman, about 30, the wife of a respectable tradesman in the neighbourhood of Cavendish-Square. She was in bed, and appeared to be very faint, her countenance being pale and anxious. She had just been sick, and on examining what she had brought up, I found it to consist of about a teacupful of transparent, colourless fluid, like water, except that it had some degree of viscidity. She complained of severe heavy pain across the brows, and of "stupidness." The skin was cool and perspiring; pulse 90, small, and soft; tongue clean; thirst urgent. On questioning her attendant, I found that she had been seized between three and four hours previously with the above symptoms, having several times vomited matters similar in appearance to those above mentioned. The bowels had been freely opened the preceding day by means of laxative pills, which she was in the habit of taking; and by her own account she had not committed any imprudence in diet to account for the attack. I ordered her fifteen drops of laudanum and double that quantity of spirit of sulphuric æther, in a little cinnamon water, to be taken immediately, and repeated in an hour, if the vomiting continued.

I had scarcely returned home when I was sent for again, in consequence of my patient having been seized with "spasms." More than half an hour elapsed before I was able to see her; she was then in a state approaching to syncope, and had just vomited half a pint of fluid, resembling the above, except that there was a perceptible shade of green in it. She said that she had felt a tingling in her feet, almost immediately after which she was attacked with cramp in the limbs and at the stomach. Her attendant states that she was "hard and stiff all over." Her medicine had not been procured when the cramps came on. Thirty drops of laudanum, and a drachm of the spirit of sulphuric æther, were immediately given; and the first dose having been rejected, the medicine was repeated. The limbs and abdomen were diligently rubbed with soap liniment warmed, and with a sixth part of laudanum added to it; after which cloths, wrung out of very hot water, were applied to the abdomen. Half the quantity of the medicine was again exhibited at the end of an hour. She now enjoyed a respite from suffering, except that of great exhaustion, for about three hours, when the vomiting and cramps returned. The fit lasted for half an hour, during which time respiration seemed to be impeded, by the muscles about the chest participating in the cramp. The same dose of medicine was given as at first, and it was directed to be repeated in half the quantity on the least recurrence of the symptoms; and every two hours at all events. The frictions were renewed, and a sinapism applied to the pit of the stomach.

She continued to have slighter returns of the cramps, with some vomiting, every two or three hours, but with increasing intervals, till next morning, when the medicine having been omitted, the symptoms returned with violence. She complained of being very chilly, and the skin felt cold and damp; the pulse being 80, and small. After the more severe attacks she lay exhausted, in a state bordering upon syncope. The medicines and other treatment of the preceding day were vigorously resumed, but without the same result; and a very violent return of general cramp, with vomiting, having come on in the forenoon, I was induced to order twenty drops of cajeput oil to be taken instantly, in an ounce of cinnamon water, and repeated in an hour, if the vomiting or cramps returned. The medicine was retained, and the second dose taken—though the symptoms had not returned—apparently because the patient had liked the first. From this time she went on well; the vomiting was not renewed at all; and though she had some cramps, they were slight and partial in comparison to what had previously taken place.

She made water several times, but in very small quantity and high coloured, during the first and second days. The bowels did not act until after the third day, when all the symptoms had been gone for twenty-four hours, and the effect was then produced by senna and salts. An enema administered the second day was returned without any alvine evacuation. The convalescence has been rapid and uninterrupted.

The servant of this patient was seized on the second day of her mistress's illness with nausea and cramps in the calves of the legs, which speedily yielded to calomel and opium, followed up by senna and salts. Two days previously two lads in the shop were sent home, having been attacked with vomiting and purging.

On Wednesday, the 3d of August, I was also requested to see Miss B——, a lady about thirty-five years of age, and rather stout. Three hours before I saw her, she had been seized with vomiting, accompanied by cramp about the muscles of the abdomen and chest, so severe as to keep her in a bent posture, and at times to interfere seriously with respiration. The vomiting recurred about every half hour; the egesta resembled water, with a little mucus; there was not a vestige of bile. The countenance was shrunk and anxious; the skin cool; and the feet so cold that she had had hot water applied to them before I saw her:—the pulse 80, and small. She was so languid as scarcely to be able to answer questions, and the slightest movement instantly brought on vomiting and an aggravation of the cramps. I had attended this patient about ten days before for an attack of bilious vomiting and purging, with cramps of the lower extremities, which had speedily yielded to mild mercurials, preceded by moderate doses of tincture of opium and sulphuric æther; and on the second occasion, she had taken before I saw her, a draught, containing ♏xxv. of the former, and two fluid scruples of the latter, but without any relief having followed. She now had ♏xx. of cajeput oil in cinnamon water, with instructions to repeat it in an hour if necessary. As in the preceding case, both doses were taken, though no return either of cramp or vomiting had occurred; but the patient, finding it grateful in its effects, had begged for its repetition. She described the relief as being immediate. There was no return of her symptoms till late in the evening, when it was attempted to move her to her bedroom: the exertion which this occasioned brought on slight sickness and a renewal of the cramps, and she remained all night on the sofa in her drawing-room, where she had been taken ill. In this case, also, the bowels had been open the preceding day, but remained without acting till the symptoms had been entirely subdued for twenty-four hours, when they were moved by means of rhubarb and magnesia. She acknowledged having eaten "a plumb and a half" the day before she was taken ill.

These facts appear to shew that the cajeput oil has some power in allaying the kind of disturbance above described: whether it has more efficacy in this way than certain of the other volatile oils, remains to be proved. The cases related differ from what in this country we are accustomed to call cholera, inasmuch as there was no purging, and the matter vomited was not "bilious:"—they differ from the Indio-Russian cholera in the absence of those symptoms of asphyxia which appear to constitute the chief peculiarity of that deadly and perplexing disease.

REJOINDER OF "M.D." TO DR. GREGORY.

To the Editor of the London Medical Gazette.

Wednesday, August 10, 1831.

Sir,

I have to thank you for the promptness and the impartiality with which you have hitherto given admission to my communications into your instructive pages. Allow me, now, to trespass on you (and it shall be but for a moment) with a very few words, rather as a running commentary on the *dicta*, than in reply to the magnanimous challenge of Dr. Gregory, contained in your last number; which, by the way, from some unexplained delay in town, has only just reached me.

I am glad that Dr. G. has found me "indefatigable" in the defence of integrity and truth: but he mistakes me much when he either thinks or calls me his "enemy." Indeed, had he "profited" by my former "good advice," he ought, long ere this, to have numbered me on the list of his best friends. I regret, on his account, that he has allowed himself to lose his temper somewhat: this has led him to speak with no little asperity of what he is pleased to call *my* "facts" and arguments. The facts I have brought forward are not mine: they belong to "history;" and require not the additional blazonry of a *name*. Of the propriety and cogency of my arguments I entertain no fear: both are before the tribunal of the medical public, to whose decision Dr. Gregory and I must alike submit. I would not willingly lay to the charge of Dr. G. or any other "trusty brother of the trade," even the slightest tendency towards the *mala fides auctorum*; but this I feel warranted to say, that his premises are, too often, ill founded or incorrect; and his conclusions illegitimate or inconsequential.

In complaining of my *incognito*, Dr. Gregory dares me to descend into the arena of controversy, and break a lance with him. With such a "redoubtable" champion even defeat might, perhaps, be not altogether inglorious to me; but I have long since hung up my arms in the temple of peace; and have, besides, a perfect horror of the *odium medicum*. It is, therefore, not at all likely that he and I can ever come into professional conflict. Should we, however, at any future hour enter the lists, *propriis personis*, he shall find me "with my vizor up," yet armed *cap-à-pie*. On the present occasion, however, with entire good feeling towards him, and the most grateful regards to you, I take leave to continue my accustomed signature, and remain, sir,

Your very obedient servant,
M.D. Oxon.

MEDICAL GAZETTE.

Saturday, August 13, 1831.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—Cicero.

FRENCH EXPERIMENTALISTS ON CHOLERA.

Of a truth the French are the most indefatigable and hot-headed advocates in the world; whether they take up a question of politics, religion, or medicine, it matters not—they carry on the war of discussion with all the imposing earnestness of inquirers after truth, combined with an unaccountable quantity of the zeal of fiery partisans. The problem of contagion or non-contagion in cholera is now the prominent question among them, and it seems to engross fully an equal share of the public notice with the inquiry into the best method of treating that appalling disease.

We have before us a whole pile of French documents, in the shape of letters and communications on cholera, addressed to government, to the public, and to private individuals; and as the writers of those documents are all medical men, we should naturally expect to add to our stock of knowledge of the malady by their perusal. This, however, proves rather to be reckoning without our host: with a very few exceptions, the mass of the correspondence to which we allude is good for nothing: it generally begins with a pompous opening, is carried on with a laborious effort at importance, but invariably concludes with some mountain-begotten mouse for its upshot. Here, for example, is a serious epistle on cholera, just presented to the medical world, by a Parisian physician:—

"Sir,—An illustrious Spaniard, Don Callos, a medical man and citizen of the world, who spent one half his life in England, and the other in traversing the various regions of the globe, happening to be in Paris a few days ago, was greatly astonished at the fears entertained of cholera morbus in this capital. He had been often in places where this dreadful complaint raged epidemically, and being a profound observer, devoted much of his attention to the peculiarities which it presented in its mode of invasion and progress, which he thought was even *more* important than the method of cure. And the result of his observation was this—that cholera always spares a certain number of persons, and of these invariably are those individuals who have—*the itch!* For which reason he considers the itch to be a certain preservative against that cruel scourge which threatens to lay waste the west and south of Europe, in spite of all the quarantines, lazarettos, and cordons, that can be opposed to it.

Theoretically speaking, sir, I should be disposed to embrace the opinion of Don Callos; for the psoriacal eruption throwing the external tegumentary apparatus into a state of permanent excitement, &c. &c. is peculiarly adapted to obviate the deficiency of innervation in which the essence of cholera seems to consist, &c.

But, sir, the very notion of such a remedy is, I most confess, enough to inspire a repugnance against its adoption. Suppose we have a young beautiful and amiable lady for a patient—one who every evening constitutes the attraction of a brilliant circle—how should we propose to her to exchange her fair and soft complexion for the *hideous asperity of pimples*, the very name of which *chills the blood* of people of fashion? here is the difficulty; yet if the remedy be specific, there can be no balancing of opinion; it must be adopted. [Here an eloquent contrast is drawn between cholera and the itch, after which the writer concludes]—seeing that the disease continues to extend its ravages, it is surely the duty of government to put to the test the observation of the learned Spaniard—and it can be readily done through the medical commissions now in operation in Poland and Russia. At all events, I feel that *I* should have been wanting in my duty to the medical public, had I concealed from them the fact of Don Callos's experience. Wishing, therefore," &c. &c.

How characteristic is the gallant Frenchman's horror at the idea of proposing so *hideous* a remedy! even though his fair patient be threatened by the fell destroyer, and the cutaneous eruption be a specific! But there is nothing more true than what Sterne says of the nation—that they are the most *serious* people under the sun.

Let us take another example of the national feeling, exemplified in that sort of enthusiasm which prompted Desgenettes, and latterly Foy, to the hazarding of their lives for the *glory* of attempting to establish their opinions.

A. M. Chervin, a physician of Paris, writes as follows to the Minister of the Interior:—

"July 1, 1831.

"Sir,—Amid the many important questions which at this moment fix the attention of Europe, there is one which above all others is of an overwhelming interest—*cholera*, that frightful disease which day after day is extending its ravages farther and farther, and menaces with destruction the nations of the west! How is its progress to be arrested? What barrier is to be opposed to it? Cordons and quarantines have been established by many governments, even to the distressful inconvenience of the people they govern. But with all due approbation of their zeal for the public safety, may we not be permitted to ask, how far such measures are calculated to check the progress of the scourge? Experience hitherto seems not to warrant much trust in their usefulness. What, then, is to be done? Why, assuredly, to ascertain whether we are proceeding by the right method; whether our system is well founded; in short, whether cholera is a contagious disease, from which we may be preserved by *cordons sanataires*, by quarantines, and lazarettos, or whether it be not the result of an epidemic constitution. This weighty question once decided, we shall, at least, have gained one step towards our protection from the new and awful calamity.

Much light, no doubt, will be thrown on the grand problem of the contagion or non-contagion of cholera, by the labours of the numerous French and foreign physicians engaged on the actual theatre of the epidemic; but it is to be feared that it will not finally be set at rest through that quarter, except after the lapse of much time. It becomes, therefore, a matter of vital importance to come at once to a positive conclusion by direct experiment, on a large scale, and beyond the interference of any thing epidemic, which can never be effected in situations where the disease prevails. Hence, sir, the paramount interest of the proposition which I have the honour to make to you, and to which I am persuaded you will grant all that attention which it merits.

Now, in order that the proposed experiments should be conclusive, they should be made far from those countries which are a prey to cholera: they should be on an extensive scale, and capable of being sufficiently varied; and, finally, they should be attested by the presence of persons of the highest credit, and who are beyond even the suspicion of prejudice.

The north-west extremity of France is, then, the very place where, with all due precaution for the public safety, a complete course of experiments might be tried; we might proceed there exactly as in a lazaretto devoted to the most contagious of diseases. From the several situations along the coast of the Baltic where cholera is to be found, various articles, such as the shirts, drawers, bed-clothes, &c. of the diseased, might be procured. The articles should be in the greatest state of impurity possible—regularly authenticated—hermetically sealed up—and shipped off with the utmost expedition to the place appointed (a steam-boat would be most available for this purpose); so that in a very few days after the death of certain assured victims of cholera, healthy men should be clothed in their garments—those garments which had been in immediate contact with the bodies of the deceased, and impregnated with more or less ——, matter which should be also procured separately, for the purposes of the various experiments. Perhaps, too, it might be possible, in spite of the rapidity of the course of the disease, to *import some of the sick themselves* for experimental purposes.

I, for my part, *am ready to expose myself the first to these experiments*; and, I am sure, there will not be wanting *other physicians* ready to submit to the same course, eager *to sacrifice even their lives* for the interests of science and humanity!

Thus neither materials nor subjects can be wanting; and it will only remain for government to express a desire that the experiments be performed.

If the individuals submitted to the experiments become seized with the complaint, while the whole surrounding county is free from it; and if the complaint remain confined to those individuals, in consequence of the preventive measures employed against its spread, then may we conclude that cholera is transmissible, at least under certain circumstances and conditions. But if, on the other hand, the persons submitted to the experiments, &c. enjoy a perfect immunity, there will, of course, be sufficiently strong reasons for the inference that cholera is incommunicable, and is propagated in other ways than by contagion. . . . . . .

The theatre of the epidemic is *not* the place for making those experiments which should clear the matter up. For let us suppose that Dr. Foy had been attacked with cholera after his inoculations, and other steps practised by him at Warsaw last month—why, no doubt, his malady would have been attributed to contagion, by the rule of *post hoc ergo propter hoc*; whilst, *in fact*, it might have been no more than the common result of the epidemic influence to which the experimentalist, as well as all the other people of Warsaw, were exposed. And this was the very case with the unfortunate Valli, who, in September, 1816, fell a victim to yellow fever, at the Havannah, *after* he had put on the shirt of a man who had just sunk under the complaint. It was looked upon at once as an incontestible proof of the contagiousness of yellow fever, without reflecting that thousands of individuals were attacked with it at the same place without having been in any way exposed to its pretended contagion.

In conclusion, sir—as the result of the experiments which I have the honour to demand, must be interesting in the highest degree to the different nations of Europe—I would suggest the propriety of calling upon the governments which are nearest to us to appoint commissioners, who should take a personal part in the proceedings, note the exact details, and be eye-witnesses of the results, in case their testimony might afterwards be required.

The importance of my request, as it concerns the interests of science, commerce, and humanity, encourages me to hope that it will meet with a prompt reply.—I have the honour to be, with the greatest respect, &c. &c.

With all its high-sounding, chivalrous, bold announcement of self-devotion—bordering on the bravado or gasconade—this letter must be admitted to contain a considerable share of plausible truth. But *the* experiment—and the place and the persons allotted for its due performance—these are the touchstone by which the propriety of its adoption should obviously be tried. *We* may look the more coolly at the proposal, because it is not the south-west or north-west corner of *our* island that is sought for the *locale* of the experimental pesthouse; but we can very well conceive the horror likely to be produced among the less enthusiastic, or the less informed portion of the French community, by the very notion of such perilous experiments. It may be observed, too, that M. Chervin says nothing in his letter of the alternative of the individuals becoming infected by the imported materials, and *the preventive measures* for isolating the disease proving *insufficient*. The following is the reply which M. Chervin received from the French government:—

Paris, July 20, 1831.

" Sir,—I laid before the Superior Council of Health the letter which you did me the honour, &c.

The Council are of opinion that the experiment to which you offer to submit yourself could not be performed in this country under the existing law of the 3d March, 1822, which makes it punishable with death for any individual to expose himself to the contact of things or persons not admitted to free *pratique*.

The Council add, that even though the government should deem it expedient to compromise the principle of the law, and to institute inquiries whether cholera is contagious or not, it could not possibly be by methods already formally condemned, where the infected articles, sent from the colonies for the purposes of experiment in yellow fever, were caused to be destroyed. They, moreover, cannot admit that it is justifiable by the laws of morality to hazard the lives of persons who, even voluntarily, come forward to submit to the experiments proposed.

I cannot but hold in the highest consideration these opinions of the Council; nor should I think it warrantable to neglect the precautions advised by them, for the preservation of France from the cruel scourge of cholera. That scourge, most fortunately, has not yet crossed our barriers; and since it has no footing amongst us, there is a most decided impossibility that the experiments you propose should be conducted on French ground.

And as to other countries, I have nothing to say—nothing to recommend or to forbid.

I entertain a due sense of the generous feelings which dictated your proposal; and you will allow me, in conclusion, to assure you, &c. &c.

Count D'Argout,
Peer of France, &c.

To this communication Dr. Chervin has published a voluminous reply (which we do not intend to censure or applaud), commenting on every sentence of the minister's letter, and praying, in conclusion, for further attention to his proposals. M. Chervin may not be much to blame for putting in his rejoinder to a letter which dashed all his doughty determinations; but we really cannot see what more he could reasonably have expected, by way of answer to his project, than the polite apology contained in the above letter. In short, we fear that, were we the minister, so harebrained a project, as we should take that of Chervin to be in the first instance, would not have come off with so much consideration from us, or without some small share of wholesome severity. But the man was *serious*, we have no doubt; and would have had, we firmly believe, no hesitation in undergoing the ordeal of his own choosing. We judge from the deeds of Desgenettes and Foy.

By the way, we have before us the latest letter of Dr. Foy from Warsaw. It contains rather a lengthy detail of the morbid anatomy of cholera, founded on the examination of twenty bodies of patients who had died purely of that disease. We do not, however, think it necessary to enter into the particulars, since we find that the appearances do not differ in the least material degree from the summary we gave of them in this journal lately, (Med. Gaz. No. 186); but we shall give M. Foy's postscript; it is worth preserving, though it has gone the round of many of the newspapers, French, English, and foreign:—" During the last five or six days the cholera has made frightful ravages at Warsaw. The plague could not have carried off its victims more suddenly. Within the short space of four or five hours patients are no more; and this whether in hospital or in their own houses. Two days since I lost an officer in my ward, in the course of five hours; and yesterday (July 18), during my visit, a patient who had been answering all my questions, died as suddenly as if he had been touched with hydrocyanic acid. We no longer know what to do, in the way of remedy, for so sudden and terrible a scourge."

PROGRESS OF CHOLERA.

Accounts from St. Petersburgh were received in town on Tuesday night. They are considerably more favourable than those we gave last week. The number of deaths had perceptibly diminished; and the only fresh places at which it had broken out were two small towns of the Gulph of Finland. A curious and important fact is mentioned, namely, that no instance has occurred in which the disease has broken out, in any vessel, more than four days after it had put to sea.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

GUY'S HOSPITAL.

Diffused Aneurism in the posterior part of Right Leg—Operation, Gangrene, and Death of the Patient.

George Charles, aged 49, was admitted into Guy's Hospital on the 10th November, 1830, under Mr. Key, for a large diffused aneurismal tumor in the calf of the right leg. The origin of it is very obscure, and the account the man gives exceedingly indistinct and imperfect. All that can be learned is, that while walking one afternoon, about five weeks before his admission, he felt for the first time pain and a sensation of numbness in the lower part of the calf: in a few days he perceived the foot and ankle to be swelled, which gradually increased and extended up the leg; this, however, was not sufficient to prevent his walking as usual, until a fortnight after the first occurrence of pain, when he was conscious of a sudden increase in the swelling, and particularly about the calf: since then he has been unable to walk, has suffered considerable pain, and thinks there has been pulsation in the tumor, but of this he cannot speak positively.

Previous to his coming to the hospital he had been seen by a practitioner, who, from the swelled and inflamed state of the limb, overlooked the aneurism, and supposed the disease to be erysipelas. Leeches in abundance were applied to the lower part of the leg, and purgative medicines given freely: by these means the inflammation was much reduced, as was also the man's general health. Two or three rigors having occurred, matter was thought to be forming.

When examined on admission, the aneurism was found to be of a very large size, commencing immediately below the knee, extending along the whole calf, and nearly encircling the limb. The tumor is very tense, and the integuments red and inflamed: most of the blood appears to be in a fluid state, as by pressure the size of it may be much reduced, and, when removed, it instantly refills; a pulsation synchronous with the arteries is plainly perceptible over the middle of it, but not nearly so distinct at the sides. The lower part of the leg and foot are very œdematous, and sensation is not very good in them. He is unable to bear upon the leg, lies with it in the bent position, and complains of much pain on the least attempt to move it. He is in ill health, strength much reduced, no appetite, tongue furred, pulse feeble and rather quick, but without any aneurismal thrill.

Cold lotion was ordered to the tumor, but in consequence of the leg and foot soon becoming cold, it was discontinued, and warm flannels applied to the foot.

11th.—1 p. m. The tumor increased in size since yesterday, is more distended and tense than then, and pulsation less evident. It being thought something should be done immediately, a consultation was held as to the propriety of amputating at once, or whether an attempt should be made to save the limb. It was a matter of considerable doubt if it were an aneurism of the lower part of the popliteal or of the posterior tibial artery: on the one hand, the situation of the tumor seemed to point out the posterior tibial as the affected vessel, and on the other, the extreme rarity of spontaneous aneurism of that vessel, and the possibility that in this case the blood, contrary to what is usually found to take place, might have descended, inclined some rather to consider it as popliteal.

Independently of the doubt as to the affected vessel, it was considered an unfavourable case for tying the artery. In the first place, if the femoral artery were tied, the patient's low state, the largeness of the aneurism, and the swollen state of the limb, rendered it probable there would not be sufficient circulation to maintain the life of the leg. 2dly, if, instead of tying the femoral artery, the aneurism should be opened and the coagulum turned out, the difficulty there might be in securing the ruptured vessel, and if found to communicate with the posterior tibial, the deep situation of that artery would render it extremely difficult, if not impracticable. 3dly, if the affected vessel were secured, it was hardly to be expected that, reduced as the patient was, he would be able to support the immense suppuration which must necessarily ensue.

In favour of tying the femoral artery, it was said, that by passing the ligature round it, as low down in the thigh as could be, just as the vessel passes through the tendon of the abductor muscle, probably the supply of blood to the tumor would be cut off, or at any rate so much lessened, and the impetus of that sent so diminished, as not to prevent the curative process being set up, while the anastomosing branches would be sufficient to maintain a due circulation in the limb; the absorbents might then possibly remove the effused blood; and if this did not take place, the bulk of the tumor being much diminished, the consequent suppuration would not be greater than the man could bear. By adopting this plan, the only chance of saving the limb would be given; and if it did not succeed, recourse could then be had to amputation. It being determined to adopt the latter plan—that of tying the femoral artery low down—the man was placed upon the operating table, and an incision made through the integuments, commencing about the middle of the thigh, at the outer edge of the sartorius, and running parallel to the fibres of this muscle, for three inches in length. The fascia was next divided, and the sartorius pulled to the inner side of the wound, when the sheath of the vessels was distinctly seen; this was next divided. The saphena nerve lay most superficial upon the vein which was immediately beneath it, and to the outer side of the vein was the artery. Two large branches were given off just as the artery passed through the tendon of the triceps; the ligature was placed round it directly above them, it being feared secondary hæmorrhage might occur if placed below them, unless the tendon of the muscle were divided, and this it was thought better avoided. In this case, the inner coats of the vessel were most distinctly seen to give way when the ligature was tightened. The wound was closed by a dossil of lint and straps of adhesive plaister.* Scarcely any blood was lost during the operation.

10 P.M.—Immediately upon the vessel being secured the tenseness of the tumor was much diminished, and not the slightest pulsation could be felt. Pulse 104, and good; feels easier than before the operation, and says he has a sensation of tingling in the great toe. The distention of the tumor has rather increased since directly after the operation, but not to the same extent as before.

12th, A.M.—Has passed a good night—slept the greater part of it; tumor much the same; leg and foot warm, not quite so much swollen; pulse 120; tongue clean; no thirst; head free from pain; bowels not opened since the operation.

> To take Magnesiæ Sulphatis, ʒij. Liquoris Ammoniæ Acetatis, ℥ss. Aq. Menthæ, ℥iss. statim.

P.M.—Has had two free evacuations. Pulse 116, fuller, and jerking. Foot and lower part of leg warm. There is a slight pulsation in the aneurism, the integuments over which are red and hot. To be kept wet with saturnine lotion. Still complains of pain in the limb.

13th, A.M.—Has not had much sleep, and has been rather delirious, continually talking. Passed four more motions. Pulse 124, small, and jerking; tongue a little furred; skin moist; thirsty; tumor much the same; leg and foot warm, and not so much swelled.

Noon.—Lotion to be discontinued, as the foot is becoming cold.

P.M.—Not much pain in leg; foot, and particularly about the ankle, cold; pulse 124, fuller, and rather sharp; skin hot and dry; tongue furred. There is a slight discharge of ill-conditioned matter from the wound in the thigh.

14th, A.M.—Has had some sleep, and feels better this morning. Pulse 112, sharp; tongue moist; but little pain in the leg; the tumor hot; the lower part of leg and foot are cold to the touch, though, he says, to him they feel warm.

> To take Liq. Ammon. Acet. Aquæ Menth. Vin. aa. ℥ss. Magnesiæ Sulph. ʒss. ter die.

P.M.—Pulse 112; skin hot; tongue more furred, moist. Has had some beef-tea. There is a good deal of difference in the temperature of the tumor and the limb below it—the one communicates the sensation of heat, the other of cold; however, the man says he does not feel any difference between them. Sensation, when touched, is not very acute.

15th, A.M.—Slept well; pulse 104, and feeble; no pain; lower part of leg and foot continue cold, and have a mottled, purplish colour. Warm water to be applied to the foot. Mr. Key thinks amputation should be performed to-morrow, if symptoms of gangrene increase.

5, P.M.—Seen by Sir Astley Cooper, who thinks the livid appearance arises rather from obstruction to the cutaneous circulation than from incipient gangrene, and wishes gentle friction to be used: even should mortification ensue, Sir A. Cooper thinks amputation had better not be performed, hoping it will not extend beyond the calf, and thus perhaps the knee-joint may be saved.

11, P.M.—Friction has been continually used without increasing the heat of the limb; there is but little sensation in the foot, and none in the great toe, at the end of which, and round the nail, there is a slight blackness, and on the leg there is a small vesication; bowels opened.

16th, A.M.—Has slept well; pulse 104, feeble; complains of feeling languid and faint; tongue moist; bowels opened; the leg has been rubbed the greater part of the night, and is rather warmer just now; still of a livid hue; he does not know when the foot is touched, unless pressed upon hard; a few more vesications have appeared upon the leg; the medicine to be discontinued, and to take gr. ij. of quinine, in some wine and water, directly.

P.M.—Feels rather better; plaister removed from the wound in thigh; pus ill-conditioned; a poultice to be applied; tumor much the same; most of the blood in it continues in a fluid state; leg and foot much the same; friction to be continued; tongue dry and brown; skin hot; pulse 104, feeble.

Four ounces of wine daily.

17th.—Not much sleep; pulse 96, small and feeble; tongue rather better; the vesicles have given way, and serum is discharging from the surfaces underneath, to which cotton wool is applied: bowels open.

Wine increased to six ounces daily.

19th.—Sleeps pretty well; bowels open; pulse 100; tongue moist; foot and lower part of leg in much the same condition; aneurism not quite so tense, the integuments over it are thinner, and a more decided fluctuation may be perceived in it.

21st.—The leg is more decidedly gangrenous, has a fœtid odour, a wrinkled appearance, and the cuticle is to a considerable extent separated from the cutis by a brownish coloured fluid; the foot is entirely without sensation; the gangrene does not extend quite up to the calf.

25th.—Not much difference; to have, in addition to his wine, a pint of porter and twenty drops of træ. opii at bed-time.

27th.—The action in the leg is more active; he complains of pain in the foot, and that part of leg immediately above the gangrenous portion is a little inflamed; there is a great deal of ichorous, dark coloured, and excessively fœtid discharge from the leg and foot; the ligature has not yet come off from the femoral artery; pus discharged from the wound healthy; pulse 100, and weak; hot water to the foot to be omitted, and the dose of opium to be increased to thirty-five drops of the tincture.

Dec. 2d.—The gangrene does not seem inclined to extend higher than the calf, where matter appears to be forming; he has complained of great pain in it, in consequence of which a hemlock poultice has been used; last night he had a rigor, which lasted for some time; tongue brown; pulse 108, and continues weak; the sensation of fluctuation in the tumor is very evident, and no doubt can be entertained about matter being there. Twelve ounces of wine daily. Ligature removed without any bleeding.

4th.—An opening has formed at the under part of the calf, directly above the gangrenous portion; this communicates with the tumor, from whence a large quantity of excessively fœtid half-liquid blood, mixed with a small proportion of pus, flows; a distinct line of separation between the living and dead parts is now perceptible; pulse 104, small; tongue clean; wound in thigh slowly healing; the calf of the leg is lightly supported by means of strips of empl. saponis; the man complains of not being able to drink his porter; to be omitted, and to have a pint of wine daily; the conium poultice to be discontinued, and a common one used.

8th.—The discharge from the calf is very great, and of the same kind, except that it is not quite so offensive; pulse 108, small; bowels opened; tongue moist; the separation of the dead lower part of the leg is going on slowly; the poultice to be discontinued, and a cloth wet with the solution of the chloride of lime used.

12th.—Pulse irritable; appetite not so good; bowels not opened; the irritation appearing to be greater since the poultice was discontinued, it was ordered to be used again; the discharge is not quite so copious, the contents of the calf being now evacuated, and the muscles and integuments left quite hollow. This evening he had a severe attack of vomiting, which was relieved by brandy.

20th.—Since last report he has continued much the same; the soft parts, which are dead, are nearly separated from the living; the fibula is completely bare to a considerable extent; the tibia is partially covered,—the integuments over it not having separated, abundance of ill-conditioned pus is discharged. Pressure is made on the calf by means of strips of plaister. Pulse 108; tongue moist; bowels opened. He is taking 35 drops of laudanum at bed-time, and a pint of wine with six ounces of brandy daily.

26th.—The wound in the thigh does not heal, and discharges a large quantity of ill-conditioned pus, some of which has burrowed into the ham, from whence it has been let out by puncture, and a poultice applied. The suppuration from the calf is also very considerable, and of an unhealthy character. The integument over the tibia, connecting the living and dead parts, is of the same size as it was a week ago, the system being apparently too much reduced to complete the process of separation.

℞ Quininæ Sulphatis, gr. ij. 6ta. quaque hora sumend.

30th.—Large quantities of pus are discharged both from the wound in the thigh, and also from the calf; there is little alteration in his symptoms, and he gradually becomes weaker. From this time until the 16th of January, 1831, he continued progressively sinking, when he died completely exhausted by the excessive discharge and irritation, nature being unable to complete the separation of the gangrenous parts. On the 10th he was ordered, with some little temporary advantage, fifteen grains of musk, with ammonia, every four hours, and his dose of tr. opii was increased at bed-time to fifty minims. No permanent good was, however, gained by these remedies. No post-mortem examination was allowed.

In this as in the case of Bond, reported a short time since in the Gazette, the femoral artery was tied unusually low down in the thigh, and a fair opportunity afforded of judging of the comparative merits of the two situations proposed for passing a ligature round the artery; either to the inner side of the sartorius in the upper third of the thigh, the situation proposed by Scarpa, and now usually adopted; or on the outer margin of this muscle, immediately before the artery passes through the tendon of the triceps, as recommended a few years ago by Mr. Hutchison. Although the result in both these instances has been fatal, no argument can be drawn from them against the plan of tying the artery low down in the thigh, as little doubt can be entertained that the nature of the cases was such as would have rendered any operation, short of amputation, unavailing. In the one, a comminuted fracture of the tibia, extending into the knee-joint, and accompanied with considerable extravasation of blood; in the other, the patient's debilitated condition, the œdema of the leg, and the immense size of the aneurism, were such unfavourable circumstances, as hardly to leave nature the power of reparation.

In the case above reported, owing to the emaciated state of the patient, no great difficulty was found in passing a ligature round the artery; but in Bond, who was a very stout muscular man, the vessel lay at such a depth as to render the operation one of by no means easy performance. This must always occur to a greater or less extent, and forms a powerful reason in favour of securing the vessel in the upper third of the thigh, where it lies much more superficial, in ordinary cases of popliteal or femoral aneurism, where experience has shewn that it is not absolutely necessary totally to prevent the flow of blood through the affected vessel to secure the obliteration of the sac. But in cases like the present, where the vessel has suddenly given way, or the escape of the blood had been occasioned by external violence, it is, of course, desirable to tie the artery as near as possible to the opening, that the flow of blood through that part of it may be the more effectually prevented; it being a well-known fact, that though an artery is obliterated for some distance below the ligature, it is by no means an unfrequent occurrence, when the ligature has been placed some distance above the aneurism, for the blood, by means of the anastomosing branches, again to enter the principal trunk, and thus pursue in part its usual course.

MEATH HOSPITAL AND COUNTY OF DUBLIN INFIRMARY.

Cases treated by Mr. Hamilton, *under the superintendence of Dr. Stokes; with Mr. Hamilton's remarks.*

Pneumonia, with Pleuritis—Danger of an overdose of Tartar Emetic—Death.

Sophia Collins, ætat. 20, stay-maker, of a lymphatic temperament, was taken into the hospital February 21st, 1831. Has been subject to slight dry cough ever since she had the measles in her childhood, aggravated occasionally by cold.

About three weeks since, during the heavy snow, she contracted sore throat, for which she was bled and blistered. After recovering from it she felt very weak, and was seized, about a week ago, with severe pain in the lower part of the left side of the chest, shooting up into the axilla. It was not preceded by shivering or other symptom, was worse on coughing or full inspiration, and at night, but does not appear to have been very tender. Along with this she had cough and expectoration, tinged with blood, and oppression of the chest.

At present the pain in the side is slight, occurring only in occasional stitches, but the cough is severe, with thin, frothy, watery sputa. Breathing hurried; pulse 135; tongue dry, and bright red; sweats at night, when she gets little sleep from coughing; appetite good.

On percussing the chest, the sound is less clear under the left clavicle; but no difference can be perceived in the respiration under the two clavicles, where it is very loud. In the upper third of the left side posteriorly, the respiration is puerile; at the two lower thirds feeble, with occasional crepitating râles. The sound on percussion is dull. The same phenomena are observed at the inferior part of the right side posteriorly, but to a less extent. Resonance of the voice most distinct on the left.

Thursday, 24th.—Dulness on percussion less; crepitating râle on both sides inferiorly very distinct. In the seat of the pleuritic pain, on the left side of the chest laterally, respiration feeble, and sound dull nearly up to the axilla. Other symptoms much the same, only the expectoration being now tinged with dull red. There being no signs of any abdominal affection, she was ordered,

Antim. Tart. gr. vi. more solito. in die consumend. et Hir. xii. regioni subscapulariæ sinistræ.

25th.—Yesterday, after dinner hour, took, for the first time at once, two ounces of the tartar emet. mixture. Vomiting immediately followed, succeeded by distressing nausea; and, on every fit of coughing, a fresh inclination to vomit. After some time, griping and purging came on. She also experienced a severe pain, like cramp, at the pit of the stomach, lasting a few minutes, and recurring at short intervals during great part of the night, in which she got no sleep, but felt weak, languid, and chilly. Towards morning a shivering came on. When I saw her, at eight o'clock, she was still shivering; her skin cold and moist; pulse weak and fluttering, 162, or even more, being so quick as to be counted with difficulty; breathing short and hurried; complains of thirst; no pain of epigastrium, or tenderness.

Two hours after (at ten o'clock), the face was flushed; skin hot and dry; pulse fuller, but of the same character, and the other symptoms the same.

℞ Liquor. Acet. Opii, gt. xx.
Aquæ Cin. ℥j. statim. sumend. Omit. Tart. Emet.

26th.—Countenance natural. The breathing is much easier, and oppression less. Cough and expectoration diminished; pulse 142, soft, but not full. Yesterday, after taking the draught, felt composed. Heat of skin natural.

℞ Calomel. gr. j.
Pulv. Dov. gr. iii. Fiant. Pil. xii. Sumat unam omni tertiâ horâ.

28th.—Was griped and purged yesterday.

Omit. pil. et habeat potus mucilagin. cum succo limonum.

After a stethoscopic examination Dr. Stokes remarked that she was better; the crepitating râle being larger, and the respiratory murmur more audible.

March 1st.—Cough and breathing easier, but expectoration copious, of a white frothy mucus. Pulse 160, not so strong; tongue red and dry; purging still continues; slight epigastric tenderness.

Rep. Potus Mucilag.

2d.—Slept well during the night; purging, however, has not stopped, having had five motions yesterday, but without griping; pulse 140;

Vesicat. Abdomini. et injiciatur enema opiatum.

For a few inches at the lower part of the left lung posteriorly, the sound is dull and respiration scarcely audible; the sound then becomes clear, and a crepitating râle is heard, mixed with a good deal of the respiratory murmur. At the superior third, respiration puerile; at the lower part of right lung posteriorly, respiration feeble, with occasional crepitating râle, and sound dull.

3d.—Purging stopped; expectoration watery, mixed with dark-coloured blood.

Potus mucilag.

4th.—In a profuse perspiration; countenance improved; pulse 100, hard and full.

7th.—The patient's face looks flushed and anxious, and she occasionally answers questions vaguely. After speaking, a tremulous motion is observed in the lips. The pulse is small, rapid, weak, and fluttering; respiration 50. She complains of weakness, and great thirst; tongue red and moist; skin hot and dry; a small patch of redness exists about the eighth or ninth rib; another also on the buttock of the left side.

She has little cough, and does not complain of difficulty of breathing or pain any where, but asks if she is going to *die*. She *raved during the night*. *Purging still continues*; expectoration reddish-brown.

℞ Sulph. Quin. gr. ii.
Opii, gr. ss. ter in die. Vini, ℥iv. in die.

8th.—Countenance pale; *excited*; expectoration viscid, brought up with great difficulty; pulse weak, rapid, fluttering; two red patches are observed on the right side and buttock, of a dark purple colour; the breathing short and hurried, but does not complain of dyspnœa; she raved violently during the night, and asks *repeatedly if she shall die*; she is constantly dozing.

Beef-tea, et rep. alia.

11th.—Sunk, under a fresh access of diarrhœa; no *postmortem* examination allowed.

Remarks.—If the former case presented a well-marked instance of simple acute pleuritis, with effusion, we have here one of the same disease complicated with pneumonia, and which shews, in a striking manner, how much the danger and difficulty of the case is increased thereby.

Double pneumonia, complicated with pleuritis and effusion, occurring in a girl of a naturally bad constitution, and further debilitated by a recent illness, and the means employed for its relief, could only suggest a most unfavourable prognosis. Accordingly, Dr. Stokes, on feeling the great rapidity of the pulse, at once expressed his fears of an unfavourable result. The same opinion is also advanced by Andral, who states, that in pneumonia a pulse much above a hundred is generally a fatal sign.

But the great interest of the case consists in the fact, that to these unfavourable symptoms was added an accidental circumstance, which there can be little doubt served in a great measure to hasten her death: I mean the fatal mistake she made of taking too large a dose of tartar emetic.

In strong constitutions, much larger doses have been given at a time than the one taken by this girl, (which could scarcely have exceeded two grains) without any bad symptom. But in her the effects, as shewn by the report of the 25th, were most violent; great vomiting and purging, cramp in the stomach, depression of strength, cold sweats, &c. being the effects (though in a greater degree) described to be usually present in a case of poisoning from this medicine. Though the immediate danger was subdued by the means employed, the intestinal mucous membrane appears to have been left in a state of such irritability as to be ready from the least exciting cause to run into inflammation; and the calomel, though exhibited in very small doses, and combined with Dover's powder, seems to have been quite sufficient to produce this effect; as, soon after she had taken a few grains, griping and purging came on, which, though put a stop to for a time, eventually returned, and only terminated in death.

The case, therefore, though proving nothing against the utility of tartar emetic in pneumonia, yet by presenting a well-marked instance of the bad effects following a mistake in its use, strongly inculcates the necessity of caution, and is hence a case of considerable value to the student, as, indeed, any case must be which sets in a strong light the necessity of caution in the use of powerful remedies.

The remedy in question, though acting thus violently and malignantly on the stomach and intestines, appears from the examination on the 28th, to have acted beneficially on the lungs. It is much to be regretted that a postmortem examination could not be procured.

APOTHECARIES' HALL.

To the Editor of the London Medical Gazette.

Sir,

I am instructed by the Court of Examiners to inform the Editors of the Medical Journals that the names of gentlemen to whom the Court grant certificates of qualification, on each day of examination, will in future be placed in the Beadle's Office on the following morning, and that each list of names will remain for public inspection (during office hours) for the space of one week.

I have honour to be, sir,
Your obedient servant,
John Watson.

Apothecaries' Hall,
August 4, 1831.

Names of Gentlemen to whom the Court of Examiners granted Certificates of Qualification on Thursday, August 4, 1831:—

John Bell.
Chas. Hen. Devonshire.
Thomas Alder Dixon.
Noah Philip Foley.
David Hughes.
John Allen Ramsey.

NOTICES.

Mr. King's letter to the Council of the London University can only be inserted as an advertisement.

A Country Surgeon.—After the completion of the present volume.

BOOKS RECEIVED FOR REVIEW.

Practical Observations on Prolapsus of the Rectum. By Frederick Salmon, F.R.C.S.

An Account of Inventions and Improvements in Surgical Instruments, made by John Weiss, 62, Strand; with a Selection of Cases wherein they have been successfully employed, and Testimonials of their utility from eminent Surgeons. Illustrated by numerous Engravings. Second edition, much enlarged.

Cholera Morbus.—A short and faithful Account of the History, Progress, Causes, Symptoms, and Treatment, of the Indian and Russian Cholera, taken from various authentic sources; with Cases as related by Practitioners in India; and also, the interesting Case lately given to the Public by Dr. Hamilton Roe, of the Westminster Hospital. By John Austin, Surgeon.

A Popular Treatise on the Teeth and Gums, and Diseases attendant on them: designed for the use of Families. By John Winckworth.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

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OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 20, 1831.

PUBLIC LIBRARY 1859

SOME OBSERVATIONS SUGGESTED BY DR. PROUT'S LECTURES;

Addressed to the Editor of the London Medical Gazette.

By A. P. W. Philip, M.D. F.R.S. L. & E. &c.

Sir,

As the lectures of Dr. Prout were published in your journal, you will much oblige me by giving a place in that journal to the following observations. I may say with truth, that no man is more sensible to the merits of Dr. Prout than he who now addresses you. The ardour and success, indeed, with which he has applied himself to his favourite science, and the consequent authority which, in all subjects connected with it, his name possesses, are the causes of the present communication; for in proportion to these will be the effect of any error he may be betrayed into. If I am right in supposing him in error, it is error that arises from the love of a science which has enabled him to do so much.

To this partiality I ascribe the following observations in the eighth and ninth pages of his treatise:—

"Thus then, (to recapitulate briefly what has been said) we may consider chemistry to hold a sort of intermediate rank between anatomy, on the one hand, and metaphysics, or psychology, on the other; and by *gradually coalescing with both, to connect the whole, as it were, into one great system.* Of these extensive branches of knowledge, anatomy, from its obvious and mechanical nature, no less than from the great attention that has been bestowed upon it, is by far the best understood, and scarcely a nerve or fibre, perhaps, remains that has not been again and again demonstrated, so that comparatively little remains to be done in it. On the other hand, if we know little of the nature of living action, or psychology[*], it has not been for want of inclination and attempts to investigate it, but simply from the nature of the subject, which, for the most part, is beyond our comprehension; while, if we turn to the vast and intermediate field, where by industry and perseverance almost every thing is within our power, we find comparatively little done, and very few working. How is this? What is the reason that so important and interesting a branch of knowledge should be so unaccountably neglected, and that our knowledge, in fact, respecting it is little farther advanced than it was twenty years ago."

The questions which this passage suggests are, How far does chemistry fill the intermediate space between metaphysics, the science of mind, and anatomy, to which we owe a knowledge of the structure of the body? and, has in fact no advancement of any importance been made in this intermediate space during the last twenty years?

Chemistry, so far from filling this intermediate space, occupies, I conceive, a portion of it only analagous to that occupied by mechanics. That the portion occupied by the former is more extensive, will be readily allowed; but to explain the phenomena of the living animal body on chemical, is a task just as hopeless as the attempt to explain them on mechanical principles. Were there no other proof of this position, the influence of the mental on the bodily functions would be sufficient.

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[*] It is remarkable that Dr. Prout here uses the word psychology as synonymous with living action, while in the foregoing sentence he uses it as synonymous with metaphysics. Is then the term living action synonymous with metaphysics? Can the laws, for example, of the living muscular fibre be with any propriety regarded as a branch of metaphysics?

The functions of animal life form a separate and peculiar science, equally distinct from mechanics and chemistry, on the one hand, and metaphysics on the other. In endeavouring to trace its laws we derive much assistance from mechanics, and still more from chemistry; and the more successfully these sciences are cultivated, the greater, of course, will be the assistance they afford; but it is as possible for the mere optician to arrive at a knowledge of the laws of vision, as for the chemist to determine the properties of the living solid.

What analysis, for example, will teach him that the law of action in the sensorial organs is uniform excitement, always followed by proportional exhaustion; while that of the muscular fibre is interrupted excitement, which is only, when excessive, followed by any degree of a similar state? Chemistry may explain the nature of the changes produced in the air, and in the blood, by respiration; but could it ever tell us that, in all the more perfect animals, this function, and consequently life, immediately depend on a constantly renewed act of the sensorium?

To determine the laws of animal life, it is neither the composition of their organs, or of the agents which influence, or are influenced by them, but the vital functions themselves, that must be observed. There is no shorter road than in other sciences. On direct observation alone our knowledge must be founded; and pathological inferences on chemical premises alone, will generally be found to be erroneous.

Dr. Prout has, indeed, laid our profession and the public under no slight obligations to him, by his successful application of chemistry to the elucidation of urinary diseases. Why has his inquiry been here so successful? Because these diseases in a great degree depend on the properties of the urine, which forms no part of the living animal. It is the proper subject of chemical research; but it will appear, as far as I am capable of judging, from what I am about to say, that when he attempts to carry his inferences farther, without a due attention to the laws of animal life, his rules of practice become inapplicable.

Between the sciences of mechanics and chemistry, and the science of the living body, there is the same kind of chasm which exists between this science and that of the mind, and no labour will ever enable us to fill up either. Between the two first and the last, the whole science of the living animal body is interposed, which in the foregoing quotation from Dr. Prout's treatise is altogether overlooked; for it is impossible that any knowledge of chemistry can ever bring us acquainted with a single law of the vital functions.

The moment we forget that chemistry, in such researches, is subordinate to the science of these functions themselves, our inferences must necessarily be erroneous. It is in vain that we know the chemical properties of the subjects of our inquiry, if we are unacquainted with the laws which, in the animal economy, control these properties.

Dr. Prout in his observations on this part of the subject is by no means explicit; and here precision is essential before we can attempt the advancement of physiological knowledge on mere chemical principles. If what Dr. Prout calls the organic principle controls the chemical properties of bodies, the attempt to trace its laws by the study of these properties, must necessarily be fruitless. What he says of sugar, the product of an organized body, in page 3, is very intelligible:—"That the organic principle of the plant does not at this moment, nor has at any time, even at the moment of its formation, acted as the cause which keeps the elements of sugar in their present state of union; but that they first combined, and still remain in union in virtue of the natural and inherent affinities existing among the particles of which it is composed." But this is evading, not removing the difficulty. Granting what is here said, we are still at a loss. What brings the elements of the compound into such positions respecting each other, that the chemical result is different from what we ever observe in unorganized bodies? Let us see what Dr. Prout makes of this. Two pages lower he observes, "No where do we see the organic agent act upon elementary principles in mass, as we are obliged to do in our experiments, but by the medium of a complicated and minute apparatus, which enables it to operate, as it were, *on the ultimate particles of bodies, and by these means to exclude some and to bring others into contact, according to the design in view. With respect to the nature of the organic agent, this view of the subject leads us to the conclusion, that in different instances it is endowed with different degrees of power, but that in all cases it must be considered as an ultimate principle, endowed by the Creator with a faculty little short of intelligence, by means of which it is enabled to construct such a mechanism from natural elements and by the aid of natural agencies, as to render it capable of taking farther advantage of their properties, and of making them subservient to its use.*"

Is here any step taken towards a mere chemical investigation of the laws of organized bodies? Would it at all affect our inferences respecting the insufficiency of chemistry to elucidate the laws of the organic principle, that chemical properties are rather indirectly than directly controlled by it? It is not necessary to object to Dr. Prout's explanation. It leaves the subject of discussion where it found it. It is only an attempt to tell us why the chemistry of organized is not the chemistry of unorganized matter.

The only difference between the opinion of Dr. Prout and that of Hunter and others, who maintain that the chemical laws of the living animal body are not identical with those of inanimate nature, is, that Dr. Prout gives his explanation of the fact; a very scientific, and perhaps a just explanation, but one which, from the nature of things, cannot be brought to the test of experiment; and which, be it observed, on the supposition of its being correct, includes *a principle that operates only in the functions of organized bodies, and whose effects we can never even hope to see exemplified in unorganized nature.*

But if such are the difficulties in the application of chemistry to what may be termed the chemical department of physiology, what shall be said of the attempt to determine, by chemical investigations, the laws of the whole science? If we find such difficulties in explaining, on chemical principles, the formation of a secreted fluid, what shall we say of the proposal to explain on the same principles all the laws of the living solid? If this or any other proposal abstract the mind from the study of the properties of the living solid *itself, from* that moment our progress in physiological knowledge is arrested.

If, at the time, the phenomena of the vital functions had equally, with mere chemical phenomena, been present to the mind of Dr. Prout, he would not, I believe, have written the following sentences, which I quote from the first page of his treatise. "A few of the phenomena presented by living organized bodies are obviously of a mechanical nature; but do we reason justly in the great majority of instances when we attempt to explain the most complicated phenomena by the assumption of a little more or less blood, or other fluid, by the presence of enlarged or contracted vessels or apertures; by diminished, excessive, or deranged vascular power or action, and a variety of similar circumstances? Or are our notions of the operations of remedies expressed by such terms as evacuants, deobstruents, tonics, &c. all having reference to mere quantity, either in mass or power, a whit more satisfactory?"

Dr. Prout here forgets that an alteration in quantity, either of mass or power, is but one link of a chain, which often draws after it consequences, either good or evil, of the last importance in the animal economy. The exact extent of the mass, or the precise power employed in a chemical experiment, may frequently be of very little importance; but in the living animal, under certain circumstances, it makes the difference between health and disease, life and death.

If the vessels of any part of a living animal be preternaturally distended, their power is impaired, the blood moves with diminished velocity; the secretions of the part consequently fail. By the distention of the vessels, their nerves are put on the stretch. This irritation of nerve is communicated by the general sympathy, which forms the nervous system into a whole, to every part of the frame. In every part the secretions, all of which depend on the state of this system, are deranged; and if the morbid distention be in the vessels of a vital organ, the most formidable disease is established. We relieve the distended vessels from that part of the blood which causes their morbid distention, and the whole of this series of changes is reversed; the vessels regaining their usual capacity, and with it their usual activity, the secretions of the part are restored. The cause of nervous irritation in the part is removed; and in consequence of the same sympathy which caused the evil to spread to every part of the system, it is now in every part relieved, and the various secreting organs resuming their usual functions, health is restored.

In the whole of this process, no doubt, many and varied chemical changes are effected, some of which we can detect, and all of which we should be glad to know; but the most important knowledge here is, that the lessening the quantity of blood in the distended part will restore that part, and through it the whole system, to a state of health, and thus a process be terminated, which would otherwise have terminated in death. I therefore conceive, that here the laws by which the increased quantity of blood in the vessels, however occasioned, is capable of producing the effects just detailed, and on which the relief obtained by lessening it depends, are those which chiefly demand the attention of the physician.

When I said that Dr. Prout, in his attempts to apply chemical principles to the improvement of pathology has, as far as I can judge, already been led into error, I referred to his observations on diet. That there is an affection of the digestive organs in which a diet similar to that which he recommends, with the exception of the frying with oil or butter, is the best, I admit; but this is not the usual state of these organs in indigestion.

I have been at much pains, in the last edition of my treatise on that disease, to point out two speciеs of it which, notwithstanding the similarity of their symptoms, are diseases of a very different nature. It appears from many experiments, the accuracy of which have now been generally admitted, that the power of secreting surfaces so directly depends on the influence conveyed by the nerves, that it is immediately deranged if any considerable portion of this influence be withdrawn. Thus it is that failure of nervous power is not an unfrequent cause of indigestion; and a failure of the digestive functions is often among the most prominent and distressing consequence of the impaired powers of the nervous system; but the indigestion caused in this way is of a very different nature from the more common form of the disease, arising from causes of injury existing in the stomach itself or the organs immediately connected with it; although some attention, both to the symptoms and history of these cases, is required to enable us to distinguish them.

In both, various symptoms, indicating disorder in the functions of the nervous system, as well as in the digestive organs, attend; but in the one, the indigestion being only symptomatic, only to a limited extent influences the nervous symptoms; while in the other, being the original disease, the nervous symptoms wholly depend upon it, and abate in proportion as the due functions of the digestive organs are restored.

These cases, although often confounded, require, as may be inferred from the difference of their nature, very different plans of treatment; and I feel no hesitation in saying, that according to my experience of indigestion, it is to the former case alone that the rules of diet laid down by Dr. Prout are at all applicable. In the latter, by far the most frequent species of indigestion, I have found that food reduced to a state of pulp by such processes as those recommended in the 15th page of Dr. Prout's Treatise, is oppressive, and of difficult digestion*, while beef, and still more generally mutton, moderately boiled or roasted, which he regards as so indigestible, I have, according to the report of at least many hundred patients, almost always found to be the food of most easy digestion.

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* "And here," Dr. Prout observes, "it may not, perhaps, be amiss to make a few remarks on what is termed French cookery, in opposition to that generally employed in this country. In France, most substances are exposed, through the medium of oil or butter, to a temperature of at least 600°, by the operation of frying or some analogous process. They are then introduced into a macerating vessel with a little water, and kept for several hours at a temperature far below the boiling point, not perhaps higher than 180°; and by these united processes, properly conducted, the most refractory articles, whether of animal or vegetable origin, are reduced more or less to the state of pulp, and admirably adapted for the farther action of the stomach." Dr. Prout here seems to take for granted, that reducing food to a state of pulp renders it more digestible. With the exception mentioned in the text, my experience is directly in opposition to this maxim.

The cause of soft and pulpy food being often the most grateful when the indigestion proceeds, not from causes acting on the digestive organs, but a diseased state of their nerves, is, I believe, not that such food is most easy of digestion, but that the state of the nerves of the stomach in such cases rendering them morbidly irritable, the soothing effect of pulpy food is grateful to them, while they are unable to bear the more stimulating properties of the more simple food.

Were it not that it would lead into too long a discussion in such a communication as the present, I could illustrate and confirm these remarks by many observations made both on the dyspeptic and the healthy stomach, and particularly relating to the cases in which great anxiety, or long and severe study, had weakened the nerves, and brought those of the stomach into so irritable a state that it was difficult to find any species of aliment they could bear.

I have seen, in a case where much study and a sedentary life had injured the nervous system, the nerves of the stomach so irritable that they could bear only vegetable food, reduced to a soft and mucilaginous state; yet it is notorious, that vegetable is less easy of digestion than animal food; and with such difficulty was the food digested in the case I refer to, that after many trials the patient found it necessary to allow twenty-four hours for the digestion of each meal. He ate only at an early dinner hour.

Unless the distinction just pointed out be carefully kept in view, we shall constantly err in applying our rules of diet to individual cases. It is also to be observed that I speak here of extreme cases. In many cases we find the two species of the disease combined, causes deranging the digestive organs, and weakening the nervous system, having conspired to produce the disease; the cause of a great deal of the variety observed in indigestion, which tends at first view to give its symptoms so anomalous a character as almost to make us despair of reducing them to any principle of arrangement.

My experience is equally at variance with Dr. Prout's deductions from his experiments respecting sugar, oil, butter, &c.

With respect to sugar, it is remarkable that some dyspeptics cannot take the smallest quantity, whether crystallized or not, with impunity. They find it necessary to abstain even from the most moderate use of it in tea. Some have inferred from this that sugar is always prejudicial in indigestion: I have not found it so. Many dyspeptics can take it in the usual way, and some very freely, without injury. These differences seem to depend on peculiarity of constitution alone, for we constantly find that both articles of diet and medicine which agree well with one patient, disagree with another, even when the cases are of the same nature. But when sugar has been used, the most refined crystallized sugar has been found lighter than either molasses or soft sugar; and I have known a lump of refined sugar eaten alone act as a grateful stimulus to the stomach, and tend to remove a sense of oppression, so that it has been habitually used by the patient for this purpose, and that without the least injury; its stimulating effect, by promoting the action of the stomach, more than compensating for any additional power of digestion, it required. This is an effect inexplicable on mere chemical principles, but one of great consequence in estimating the dietetic qualities of the sugar.

Of the effects of oil, our opportunities of judging in this country are very limited. One of a highly dyspeptic family under my care, having passed some time in Italy, introduced into his family the use of olive oil instead of butter. All its members made the change, and they all told me they found the oil lighter than the butter. One of this family many years afterwards having come into a very different state of health, found the oil now disagree with him.

Such facts, it will be admitted, set at defiance all chemical analysis. Would not oil eaten with bread disagree with the stomach of most people in this country, were it for no other reason than the disgust it would occasion? Is not the food, *cet. par.* which pleases the palate better digested than that which disgusts it? can any chemical process explain to us why this is so? It may be useful on many accounts to determine the composition of the food which best agrees with the stomach, but we have no other way of discovering what that food is but by observing its effects on the stomach itself.

I come now to the last of the two questions I proposed to consider. Has, in fact, no advancement of physiological knowledge been made during the last twenty years? It was my intention, in the following part of this paper, to enumerate all the facts relating to the science of animal life which have been determined during that period; but I find, on reviewing the subject, that this would swell the present communication to too great a size. I shall therefore confine myself to those facts, the confirmation of which have come under my own observation. All the experiments by which they were ascertained were frequently repeated in the presence of men well qualified to judge of the results, and many of them in different parts of the continent as well as in this country; nor has there occurred an instance, since their first publication, of any of the results having been found to be erroneous.

IT appears from the experiments here referred to,

THAT the opinion of Haller, respecting the power of the muscular fibre, (that it is independent of the nervous system,) is correct; it having been proved experimentally that the influence of this system, in its effect on the muscular fibre, resembles the effect of all other agents capable of exciting it. It tends to exhaust, not to maintain, its excitability[*].

That the power both of the heart and vessels of circulation is independent both of the brain and spinal marrow[†].

That the nervous power is capable of acting as a stimulus both to the heart and vessels of circulation[‡].

That the nervous power is capable of acting as a sedative both to the heart and vessels of circulation, even to such a degree as instantly to destroy their power[§].

That the proof of the vessels of circulation possessing a principle of motion independent of their elasticity, which bears the same relation to the nervous system with the excitability of the heart, not only, we shall find, as far as respects the kind of influence which they derive from that system, and the way in which it is supplied to them, but also as far as respects the purposes for which it is bestowed on them, affords a strong argument for believing that this power is of the same nature with that of the heart[*].

That the laws of the excitability of the sensorial and muscular systems are very different; those of the former being uniform excitement always followed by proportional exhaustion, those of the latter interrupted excitement, only when excessive, followed by any degree of the same state[†].

That the sedative is not the effect of excess of the stimulant effect, but, like excitement itself, the direct operation of the agent[‡].

That there is no agent capable of affecting the living solid that does not in small quantity produce more or less of the stimulating effect, and none that in greater quantity does not produce the sedative effect; but the degree in which agents possess each of these qualities which in different agents bear no proportion to each other, admits of infinite variety[§].

That the power of the muscles of voluntary motion, like that of the heart and blood-vessels, is independent of the brain and spinal marrow, the nervous power influencing them in no other way than stimulants and sedatives do[‖].

THAT the manner in which the muscles of voluntary and involuntary motion are subjected to the influence of the nervous system is essentially different, the former being subject to the influence of no part of the brain or spinal marrow, except the small portions from which the nerves of the particular muscles arise, while each of the muscles of involuntary motion is subjected to the influence of every part of these organs[¶]. Hence it is that neither mechanical nor chemical agents acting on the brain influence the muscles of voluntary motion, unless they are applied near the base of this organ, and that these muscles may be excited by stimulants applied to very minute parts of it;

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[*] My Experimental Inquiry into the Laws of the Vital Functions, 3d Edit. Exper. 34, 35.

[†] Philosophical Transactions for 1815. Exp. Inq. Exper. 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12.

[‡] Phil. Trans. for 1815. Experimental Inquiry, Exper. 13, 14. 15, 16, 17, 18, 26, 29, 30, 31.

[§] Phil. Trans. for 1815. Experimental Inquiry, Exp. 15, 16, 17, 18, 19, 20, 23, 24, 25, 29, 30, 31, 32, 33.

[*] Experimental Inquiry, Part 2, Chap. 1 and 2; also Exp. 43, 48, 49, 58, 59, 60, 61. This inference is farther strengthened by facts stated in a paper I had the honour to present to the Royal Society about two months ago.

[†] Experimental Inquiry, Chap. 12.

[‡] Ib. Chap. 4 and Chap. 12

[§] Annals of Philosophy for 1818. Experimental Inquiry, Chap. 12.

[‖] Experimental Inquiry, Exp. 34, 35.

[¶] Ib. Exp. 36, 37, 38, 39, 40, 41, 42, 43, 44.

while both mechanical and chemical stimulants applied to any considerable part of the brain or cerebellum, influence the heart, which cannot be sensibly influenced by any stimulant applied to a minute part of either of them[*].

That the influence of agents applied to the brain on the heart and muscles of voluntary motion, also differ in the following respects: — Mechanical agents acting on the brain are better fitted to excite the muscles of voluntary motion, and chemical agents the heart[†]. The heart obeys a much less powerful agent applied to the brain than the muscles of voluntary motion do[‡]. While stimulants acting on the brain excite irregular action in the muscles of voluntary motion, they never have this effect on the heart, but simply increase the force and frequency of its contractions, nor is its action rendered irregular by sedatives acting on the brain, except they be of great power[§]. The excitement of the muscles of voluntary motion takes place chiefly at the moment at which the stimulant is applied to the brain, while that of the heart may generally be perceived as long as the stimulant is applied[||]. In the process of dying, after all stimulants applied to the brain fail to excite the muscles of voluntary motion, both mechanical and chemical stimulants so applied still excite the heart[¶]. All the foregoing differences in the effects of agents, applied to the brain, on the heart and muscles of voluntary motion, are referrible to the following law:—That the heart is excited by all stimulants, even the weakest, when applied to a sufficiently extensive part of the brain, while the muscles of voluntary motion are only excited by intense stimulants applied to certain small parts of it[**].

THAT the secreting, and other assimilating processes, continue after the removal of the sensorial power[††].

That the function of secretion is destroyed by cutting out a part of the nerves of the secreting organ, or by dividing them, and turning back the divided ends[*].

That it may be restored when thus destroyed by subjecting the secreting organ to the influence of the voltaic pile, by which, as far as we can see, it is enabled to perform its function as perfectly as when it received the influence of the brain and spinal marrow through the nerves[†].

That all the other functions which preserve the healthy structure in like manner depend on the influence of the brain and spinal marrow conveyed by the nerves, and are destroyed by interrupting this influence by the means just pointed out, the part rapidly becoming disorganized[‡].

That this effect is wholly obviated if the influence of the voltaic pile be substituted for that of the brain and spinal marrow as soon as the nerves are divided, the structure of the part remaining perfect[§]. The delay of even a very short time in the application of the galvanic influence, will cause more or less derangement of the structure of the part.

That secretion and the other assimilating functions of the part although deranged, are not destroyed by simply dividing the nerves, if the divided ends are not displaced, even although they may have retracted for above a quarter of an inch from each other, the nervous influence still passing from the one divided end to the other[||].

That destroying any considerable part either of the brain or spinal marrow, deranges the secreting and other assimilating powers. The destruction of one-half of the spinal marrow deranges the secreting power nearly as much as dividing and displacing the eighth pair of nerves[*].

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[*] Experimental Inquiry, Exp. 36, 37, 38, 39, 40, 41, 42, 43, 44.

[†] Ib. Exp. 36, 37, 38, 39, 40, 41, 42.

[‡] Ib. Exp. 38, 39, 40.

[§] Ib. Exp. 32, 43, and others.

[||] Ib. Exp. 44, &c.

[¶] Ib. Exper. 41.

[**] Ib. Chap. 4.

[††] Ib. Exp. 65, 66, 67, 68, 69, 70.

[*] Philosophical Transactions for 1822 and 1828. Experimental Inquiry, Exp. 48, 49; also p. 227 et seq. 3d Edit.

[†] The London Medical and Physical Journal for May 1820, vol. xliii. p. 385.

Philos. Trans. for 1822, 1828, and 1829.

De l'Influence du Systeme Nerveux sur la Digestion Stomacale, par MM. Breschet, D.M.P. chef de Travaux Anatomiques de la Faculté de Medecine de Paris, etc. H. Milne Edwards, D.M.P. et Vavasseur, D.M.P. (Memoire lu a la Societe Philomatic, la 2 Aout, 1823; Extract des Archives Generales de Medicine, Aout 1823.)

Experimental Inquiry, Exper. 75, 76, 77, 78.

[‡] Philosophical Transactions for 1827. Experimental Inquiry, Exp. 48, 49, &c.

[§] Philosophical Transactions for 1822 and 1828.

[||] Philosophical Transactions for 1822.

Experimental Inquiry, Exper. 73

De l'influence du systeme nerveux, &c. Par MM. Breschet, Milne Edwards, Vavasseur, &c.

That simply dividing the spinal marrow does not derange the secreting and other assimilating powers[†].

The facts referred to in the preceding paragraphs throw light on the symptoms of diseases of the spine, and have suggested considerable improvements in their treatment[‡]. The same may be said of certain diseases of the stomach and lungs[§].

THAT the peristaltic motions of the stomach and intestines are independent of the nervous system, though influenced through it[||].

That the food when received into the stomach remains at rest in the central part of this organ, and unmixed with the food which still remains from the last meal; and is changed as it approaches the surface of the stomach, in consequence of that previously there being moved on towards the pylorus[¶].

That the food is most mixed with the fluids of the stomach, and the greatest change is effected in it, in the large end of the stomach, the food being less moist, and of more uniform consistence, in the small end[**]. That the efforts to vomit, occasioned by the division of the eighth pair of nerves, arise from undigested food coming into contact with the surface of the stomach, no longer covered with its proper fluids[††].

That the muscular power of the stomach remains after the division of the eighth pair of nerves, and the displacement of their divided ends; by which all that part of the food which has undergone the action of the gastric juice, and is consequently fitted to excite the proper action of the stomach, is propelled into the duodenum, undigested food alone remaining in the stomach, if the animal has lived from sixteen to twenty hours after the operation[*].

That the stomach and lungs, like the sanguiferous system, are influenced by every part of the brain and spinal marrow[†].

THAT the animal is still capable of maintaining to a certain degree its temperature above the surrounding medium after the removal of the sensorial power[‡].

That the destruction of any considerable part of the spinal marrow lessens the temperature of the animal[§].

That the galvanic influence occasions a disengagement of caloric from arterial blood, if it be subjected to this influence as soon as it leaves the vessels, but none if the blood has remained out of the vessels, even for two or three minutes[||].

That there is no disengagement of gaseous fluid from arterial blood on its leaving the vessels[¶].

That the galvanic influence occasions no disengagement of caloric from venous blood—that is, blood which has already undergone the action of the nervous influence, although subjected to it as soon as it leaves the vessels[**].

THAT the brain and spinal marrow are the only sources of nervous power, the nerves, ganglions, and plexuses, being only channels of its transmission, and means of uniting the influence of different parts of these organs[††]; and that the laws of the two great classes of nerves, the ganglionic on the one hand, and the cerebral and spinal on the other, essentially differ; the latter being organs of sense and voluntary power; the former, in the strictest sense, vital organs[‡‡].

That the influence of every part of the brain and spinal marrow is, through the ganglionic system of nerves, bestowed on all parts directly or indirectly necessary to the function of secretion, and the other assimilating functions, the due performance of these functions requiring the united influence of all parts of both organs*. The position of the ganglions, and the distribution of their nerves, tend to confirm these results of experiment.

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[*] Experimental Inquiry, Exper. 58, 59, 60, 61.

[†] The paralysis of the lower part of the body, occasioned by its division, arises, not from any loss of power in the spinal marrow, but from the lower part of the body having its communication with the principal source of sensorial power destroyed. Experimental Inquiry, Exper. 62.

[‡] My Treatise on Indigestion.

[§] Philosophical Transactions for 1817 and 1827.

My Treatise on Indigestion, p. 382, et seq. 6th Edit.

[||] Philosophical Transactions for 1822 and 1828. Experimental Inquiry, Exper. 50, 51, and Chap. xii.

[¶] Eperimental Inquiry, Exper. 53, 54, 56.

[**] Ib. Exper. 55, 56, &c.

[††] Ib. Chap. 7.

[*] Philosophical Transactions, 1822 and 1828. Experimental Inquiry, Chap. 7.

[†] Ib. Exper. 48, 49, 58, 59, 60, 61.

[‡] Ib. Exper. 68, 69, 70.

[§] Experimental Inquiry, Exper. 58, 59, 60. It appears from the experiments of Mr. Brodie (Philosophical Transactions for 1812), that causes powerfully debilitating the functions of the brain greatly lessen the temperature.

[||] Experimental Inquiry, Exper. 80–81, 82, 83.

[¶] Ib. Exper. 86.

[**] Ib. Exper. 84, 85.

[††] Ib. Chap. 9.

[‡‡] Ib. and Chap. 12, and the experiments there referred to.

That the various functions of the living animal body may be divided into sensorial, nervous, and muscular†. The sensorial power, though chiefly, is not wholly confined to the brain, nor the nervous to the spinal marrow; both powers in a greater or less degree residing in both organs ‡.

That what we call death is the removal of the sensorial power alone, the nervous and muscular powers still continuing§.

That in the function of respiration, the sensorial, nervous, and muscular powers co-operate‖.

That it arises from the ceasing of respiration that the removal of the sensorial power is at length followed by the loss of the nervous and muscular powers¶.

That whatever be the cause of death, the functions cease in this order, unless the sensorial or nervous system be so impressed as instantly to destroy all the functions**.

Such are the immediate inferences from the experiments. By comparing them together we arrive at the following conclusions.

The circulation is supported equally by the power of the heart and blood vessels; and we have reason to believe that, in both, the power which supports it is a muscular power.

The power of the muscles, both of voluntary and involuntary motion, is independent of the nervous system, and depends on the organization of the muscular fibre itself. Both these sets of muscles are equally capable of being excited by the nervous power; but while this power is the sole stimulant to which the muscles of voluntary motion are subjected, it acts only occasionally on the muscles of involuntary motion, which are excited in all their usual functions by stimulants, over which the will has no influence; for which reason, and because they are incapable of effecting an object desired, their action is involuntary. If we had had no wish to handle, the fingers would never have become subject to the will.

When the muscles of involuntary motion are excited by the nervous power, it is not applied to them in the same way as to the muscles of voluntary motion. The consequence of which, it appears from what has been said, is, that the muscles of voluntary motion are only excited by powerful stimulants, applied to the small parts of the brain and spinal marrow from which their nerves originate; while the muscles of involuntary motion are excited by all stimulants applied to any considerable portion of these organs.

The excitement of the muscles of involuntary motion in all their usual functions occasions no exhaustion in them, the muscular excitability not obeying the same laws with that of the sensorial organs, whose excitement is always followed by proportional exhaustion. The excitement of the muscles of involuntary motion seems to be rendered independent of the nervous power, because their functions require a more uniform excitement than could have been derived from this source; and they appear to be subjected to the influence of the whole brain and spinal marrow, because they are directly or indirectly subservient to the functions of secretion and the other assimilating processes, which require for their due performance the united influence of every part of these organs; for the nervous power, we have seen, is not supplied by the brain alone, the spinal marrow supplying a necessary part of it, and that independently of any operation of the brain on this organ.

In the secreting and other assimilating functions the sanguiferous system appears only to supply the fluids to be operated upon by the nervous power; and the disengagement of caloric which supports animal temperature, is also effected by the action of this power on the blood. Thus it is that those vital functions which depend on the secreting and other assimilating powers are immediately subjected to the nervous system; while those which depend on the muscular power alone are only indirectly subjected to it, the nervous power influencing them through the functions of respiration and the secreting and other assimilating processes.

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* Ib. Exper. 48, 49, 58, 59, 61, 62.

† Philosophical Transactions for 1829. Experimental Inquiry, Chap. 9, and the experiments there referred to.

‡ Ib. Exper. 63.

§ Philosophical Transactions for 1829. Experimental Inquiry, Exper. 64, 65, 66, 67, 68, 69, 70, 71.

‖ Ib. Chap. 11, and the experiments there related and referred to.

¶ Ib.

** Ib.

SUCH is the relation which the nervous system bears to what may be called the circumference of the animal body, in contra-distinction to the sensorium, which may be justly regarded as its centre, to which that system bears a relation of equal importance; for it may be regarded as the means of connecting the organs of the sensorium with all other parts. In its power this system is independent of the sensorium, for we have seen it capable of all its functions after the sensorial power is withdrawn. It is still capable of exciting the muscles both of voluntary and involuntary motion, of causing an evolution of caloric from arterial blood, of forming from the blood the various secreted fluids, and maintaining all the other assimilating processes on which the healthy structure of every part depends. In all these functions, however, it is capable of being influenced by the sensorial power, constantly, in the excitement of the muscles of voluntary motion, occasionally in all the others. It therefore bears the same relation to the sensorial system which the muscles bear to it. As the muscular is independent of the nervous power, so is the nervous of the sensorial power. As the nervous influence all the muscular functions, those of the muscles of voluntary motion constantly, those of the muscles of involuntary motion occasionally; so the sensorial influence all the nervous functions, those of the cerebral and spinal nerves constantly, those of the ganglionic nerves occasionally. Thus all the functions of the nervous and muscular systems, by which we are connected with the world that surrounds us, are constantly subjected to the sensorial power; while the functions on which our life depends, with the exception of respiration, are only occasionally so, and under circumstances in which the will has no control. With this exception the latter are all functions of the nervous and muscular powers alone. To respiration the sensorial power also is necessary; and therefore the nervous and muscular powers never long survive the loss of the sensorial power.

The nervous power, which connects all the other powers of the living animal body, effects so many changes in it, and has so large a share in connecting it with the world around it, cannot, strictly speaking, be regarded as one of the vital powers of that body, but as an agent employed by them; because it has been proved by direct experiment that it is capable of existing independently of the mechanism of the part in which it resides, and therefore is not peculiar to that mechanism; and by the same means that all its functions may be performed by galvanism, made to operate in the same circumstances in which the nervous power operates.

When the whole of the phenomena here referred to are duly considered and compared, the inference appears to be unavoidable, that the nervous and galvanic powers are the same. I have made many attempts to influence the galvaniscope by the nervous power, but hitherto without success; which need not surprise us, as the electricity of electrical animals, although sufficiently powerful to benumb a horse, produces no effect on the most delicate electrometer.

The nervous and muscular functions, being the effects of inanimate agents acting on living parts, survive the sensorial functions, which are the effects of living parts acting on each other.

Hence we see why the two first set of functions bear so strong an analogy to the processes of inanimate nature, while in the sensorial functions we can trace no analogy of this kind.

Such are the results, with what appear to me the necessary inferences from them, of a very extensive set of experiments in which I was, as far as more active duties admitted, many years engaged. These results are capable of being illustrated by the labours of the chemist. He may point out the chemical changes which accompany and doubtless modify them, but it is impossible that he should ever add one to the number. The laws of the vital functions can only be determined by observing the phenomena of these functions themselves.

IT appears from what has been said that by the powers of the nervous system the animal body is formed into a whole. Through this system the sensorial power operates, and all the vital powers either immediately depend upon the nervous system, or come within the sphere of its influence. Hence results the sympathy which exists among the different parts of the living animal which more than any other principle of our frame influences the phenomena of disease. It is by this sympathy that an injury of any one part soon produces derangement of the whole, and that diseases of continuance are constantly changing their aspect, and becoming more complicated. One of the most important as well as difficult tasks of the physician is, to trace the effects of this sympathy, that he may be enabled to arrive at the origin of the evil; without which his plans of cure, however they may alleviate, can never be permanently successful; for the most prominent evil is not always the first in the chain, and if the original affection be overlooked, the same sympathy will sooner or later reproduce the same train of symptoms.

The brain and spinal marrow suffer from all affections in every part of the system, and as on their functions depend both the functions and the structure of every other part, long-continued irritation in any part, although not of a nature to produce serious consequences in the part itself, never fails more or less to affect the whole, and thus prepare the various organs to yield to causes that would have been resisted had they not, if I may use the expression, been habitually prepared for disease. Did the limits of this communication admit of it, I could illustrate what is here said by many cases in which the rapidity and fatal effects of the disease could only be accounted for on the principles I am here endeavouring to explain.

Nor is it necessary, in order to produce this general failure of the powers of the constitution, that the cause of irritation should be constant. A frequent recurrence of attacks, not in themselves of a formidable nature, are sufficient at length to produce the effect; and the friends of the patient are surprised to see a train of symptoms suddenly supervene which defy our art, in one whose frequent indispositions they had been accustomed to consider so trivial; and the surprise is the greater that in the intervals of the attacks there is often little to indicate the change that is gradually taking place. The fault rather consists in an increased tendency to disease, than disease itself. The patient's strength may be in some degree impaired, his flesh in some degree reduced, or he may appear fuller and paler, having, in consequence of a failure of power in the organs of waste, acquired what is called unhealthy fat, but still he goes about and eats his victuals, and generally makes no complaint; yet a physician of experience will perceive that this is a state that cannot continue without becoming worse, although he may not be sufficiently aware of its nature to point out the remedy.

The two circumstances which most influence the progress of disease by sympathy are, that from the constitution of our frame some organs sympathise more than others, and that the sympathy is always most apt to affect a part weakened by other causes. Thus, peculiarity of constitution generally influences the progress of disease. Where there is a constitutional weakness of any part, that part is most liable to be affected by the diseases of other parts. It is also a point of great consequence to be attended to, that compound diseases, if I may use the expression, in which, by the power of sympathy, more than one organ has become affected, are of a more formidable nature, the severity of the symptoms being the same, than disease confined to one organ; for the affection of each tends to aggravate that of the other, and all causes of injury to either affect both: thus the chance of mischief is increased, and the greater the number of organs involved in the disease, the greater, for very evident reasons, is its tendency to spread farther.

A principal object of my Treatise on Indigestion was to illustrate the effects of the sympathy of which I am now speaking; but the plan of that work was too confined to permit me to enter on the subject in its full extent: and I last year took a more extended view of it in a Treatise on the Prevention of Organic Diseases; pointing out the application of the foregoing results, to explain the steps by which organic disease is often established, and the principles of the treatment by which a tendency to it may be counteracted; and even a certain degree of it often removed, especially in the organs most under the influence of medicine.

I shall close this paper by the following observation on the only kind of experiments by which the laws of the vital functions can be determined. We have seen that death consists in the ceasing of the sensorial functions alone, and that they only indirectly contribute towards maintaining life through the medium of respiration. With the exception of respiration, then, all the vital functions continue for a certain time after what we call death, as we have seen ascertained by direct experiments; and their continuance may be much prolonged by inflating the lungs at intervals, so as to imitate respiration. Hence it is that most physiological experiments may be as well performed on the newly dead as on the living animal, and some with greater advantage. Of the experiments detailed in my Experimental Inquiry, about three-fourths were made on the newly dead animal. Thus a principal objection to such experiments is removed; and this is of the more consequence, because they generally require to be frequently repeated, to render the result certain.

PUERPERAL FEVER.

To the Editor of the London Medical Gazette.

SIR,

IN a former communication I attempted to point out the difference which exists between two diseases very generally confounded with each other, under the common term of puerperal fever. An idea is prevalent among medical practitioners that the only difference between the acute and subacute disease, or the typhoid and the inflammatory, is, that one occurs in women, whose constitutional powers are greater, whose circulation is more active and energetic, and who are of a more inflammatory diathesis than those who labour under the typhoid form; that they are not two diseases, but identically the same, modified only by the constitution of the patient, by her age, her previous modes of living, and all those causes which may render the circulation more or less vigorous and energetic. To this hypothesis there are, I think, insuperable objections; the acute form affects not unfrequently the weak and exhausted, and the strong and previously healthy constitution of the parturient female does not secure her from falling a victim to the typhoid variety of this fatal malady; nor in the latter case will the antiphlogistic plan of treatment, nor the firmest credence of its inflammatory nature, prove *a saving faith* with respect to our patients. The diagnosis is in most instances, though not easy, yet by no means impossible; the former or acute inflammation of the uterus and contiguous structures is generally sporadic; the latter (to which, perhaps, the term puerperal fever should be confined) generally occurs successively to a number of lying-in women, and is not traceable to cold, to exertion, or other causes, as metritis very frequently is. There are many symptoms common to both, and few peculiar to either, yet in most instances, and probably in all, a careful attention to the origin of the disease, and the succession of its symptoms, will prove a sufficiently accurate guide to direct us in the appropriate modes of treatment. The acute inflammatory puerperal fever, inflammation of the uterus, its peritoneal investment and contiguous structures, I have seen arise from cold, from over exertion early after delivery, and in some instances apparently from mental causes, while the low form of puerperal fever is probably in all cases the result of contagion, or the reception of some morbific agent, which evidently produces a disposition to putrescency, a dissolved state of the blood, and a general ramollissement of the uterus, the spleen, liver, and kidneys.

In the second number of your valuable journal, which succeeded the paper I submitted to your readers, there appeared a communication decidedly corroborative of the accuracy of those distinctions which I had endeavoured to point out.

M. Sanson, of the Hotel Dieu, there states that he had "treated several cases in the present year of uterine phlebitis, and that it proved fatal under every plan of treatment, and assigns as a cause of the disease the sudden changes of the weather as to temperature and humidity." With this opinion I cannot concur, having witnessed puerperal diseases in their worst and most fatal form in the warmest and dryest seasons, as well as in the wettest of our autumns and winters.

"M. Sanson states, that two cases of *metritis* were cured by the adoption of the antiphlogistic mode of treatment; but he says that bleeding, whether local or general, though employed at an early period, has proved unsuccessful, and the antiphlogistic plan has been found in numerous instances to hasten rather than retard the fatal result; nor did mercurial frictions, laxatives, nor vesications, prove in the least degree more beneficial." M. Sanson has employed emetic tartar in inflammations of the veins following surgical operations, and observes that future experiments must shew whether this, or any other remedy, is capable of neutralizing the effect of the contagion of puerperal fever."

It is a curious fact, that pregnant and parturient females are perhaps exclusively the only individuals susceptible of the disease excited by this contagion; but I have lately heard a case related by a friend which, if correct, would seem to shew that it is capable of being received into the system independent of pregnancy or parturition. An observant practitioner, with whom I was conversing on the subject, detailed the case of an elderly female, who had never been pregnant, and who was under his care for stricture of the rectum at a period when puerperal fever was extending its ravages. She resided under the same roof with a woman who at the time laboured under the low form of puerperal fever. She was seized with shiverings, pains in the hypogastric region; the abdomen became tumefied and tympanitic; her pulse small, frequent, and irregular, with the characteristic prostration of strength and disturbance of the sensorium, and he considers her death to have been occasioned by the contagion of puerperal fever.

It is difficult to explain the reason why women, whose uterine system is in a state of development, should be almost, if not entirely, the sole victims of this disease; nor does our present knowledge of the laws of contagion enable us to discover why this, or any other miasma, should affect one part of the body while all the others, although equally exposed to it, are exempt from its influence. Thus influenza, an epidemic which has been so prevalent lately, affects first the mucous lining of the larynx, pharynx, and nares; cynanche parotidæa, the glandular system; and the contagion of Egyptian ophthalmia excites inflammation only in the eyes.

The consideration that typhoid puerperal fever is influenced so little by any remedial measure at present known, is humiliating; and I cannot help believing that the mistaken pathology respecting its danger being commensurate with, and dependent on the degree of inflammation, has tended greatly to retard the discovery of other and more successful modes of treatment than have yet been devised.

I should not expect benefit from the use of tartarized antimony, as suggested by M. Sanson: it would excite vomiting; and the agitation and pressure of the abdominal muscles would probably increase the symptoms; for the like reason, the spiritus terebinthinæ may be considered a very doubtful remedy; it has failed in the practice of some of the ablest of the profession, and I have never heard from any of my medical friends a single case corroborative of the sanguine statements of Dr. Brenan. With respect to active depletion, by bleeding, whether local or general, I would suggest whether we are not by it counteracting the efforts of nature, and whether the febrile symptoms may not be a salutary means of enabling the system to throw off that which would inevitably prove destructive to life, and if this be at all correct, if we diminish power and exhaust the patient by copious bleeding, all that remains for us is to watch her certain progress to the grave.

The early and free employment of mercury appears to promise more than any other remedy hitherto suggested, and mercurial frictions to the thighs and abdomen, with the internal use of calomel in large doses, repeated at short intervals, should be had recourse to, so as to affect the mouth as speedily as possible.

I am fully aware that there is little novelty in the suggestions I have ventured to make, as to the nature and treatment of the varieties of puerperal fever, nor can I propose any remedy by which it may be successfully combated, with a degree of certainty; yet there are practitioners who are still so much influenced by great names, and who swear by Gordon, Hey, and Armstrong, that their creed is infallible, blind to the fallacies of their theory, when their patients die, after having been subjected to the antiphlogistic treatment in its fullest extent, they attribute the fatal result (like their learned predecessor, Dr. Sangrado,) to their not having lost a sufficient quantity of blood, nor swallowed enough warm water! An accurate and just theory of the nature of a disease is likely to lead to a successful plan of treatment, and as this is one which destroys human life at its most valuable and interesting period, I shall be quite quite content if this paper possess only the negative merit, *populum falsis dedocere uti vocibus.*

CHARLES SEVERN.

Jewin-Street, Aldersgate-Street.

TUBE FOR EXPLORING DEEP WOUNDS, &c.

To the Editor of the London Medical Gazette.

Gateshead, Aug. 6, 1831.

SIR,

HAVING frequently proved the utility of the instrument described below, I shall be obliged by the insertion of this communication in your journal.

I wish to call the attention of the profession to an easy method of inspecting deep-seated parts, which cannot otherwise be brought into view, *e. g.* vagina, os uteri, perhaps the rectum, and the bottom of deep wounds, as well as incisions made by the operator.

The instrument alluded to is simply a glass tube, open at both ends, inclining to a conical shape, the larger end spreading out, or bell-shaped, while the smaller one is rounded off very smooth, and approaches the semi-globular form. The opening at the smaller end must be as large as it can conveniently be made, and with perfectly smooth rounded edges—the whole tube of sufficient strength to obviate any hazard of breaking; and of different sizes, so as to be adapted to the various purposes to which they may be applied. The above description will enable any of the flint glass manufacturers to supply the tubes.

The rounded form of the smaller end renders its introduction perfectly easy; and if the patient is so placed that the light passes down the tube, whatever part presents at the opening may be inspected in a satisfactory manner, cleaning it if necessary with a piece of sponge fixed on the end of a probe. When used in examining the vagina, &c. or in making exploratio obstetrica, female delicacy may be spared by passing the tube through an opening in the linen.

I am, sir,
Yours respectfully,
T. K. FIFE.

MEDICAL EDUCATION.

To the Editor of the London Medical Gazette.

SIR,

I HOPE you will favour me with a place in your pages for the few following remarks. The subject of them is one of great and general interest, both to the public and the medical profession, and, I hope, may soon meet with that attention from all which its importance demands.

I am, sir,
Your obedient servant,
J. S. M.

August 9th, 1831.

To Sir Astley Cooper, Bart.

To you, sir, as at the head of the surgical department of our profession in England, I would beg to offer the few following observations, hoping they may meet with your attentive consideration and efficient support. My attention has been more particularly drawn to the subject by some remarks in Dr. Conolly's valedictory lecture on retiring from the London University, published in the Medical Gazette of May 7th—remarks which, I fear, are but too well founded. He there declares it to be "his fixed opinion, that the study of physic, as a preparation for practice, is most miserably neglected;" "that every successive year sees hundreds of young men entering upon all the responsibilities of practice, and undertaking the management of the most serious diseases, who are actually not yet sufficiently versed in the practical part of their profession, to engage in it with safety to their paitents;" and, "that for this extraordinary and lamentable fact, those at the head of the profession have in some measure to answer." The lapse of forty years, sir, you are well aware, has seen a great and decisive change in the practice of the healing art. The race of old English apothecaries is extinct, the midwives have nearly disappeared, while the physician has retired, or rather has been *ousted*, from a considerable part of his former sphere, and the mass of practice is falling into the hands of the *general practitioner*, a title of very wide and *general* signification; including within it the well-educated surgeon, with a medical degree in his pocket; and the druggist, and retailer of ginger beer and soda water. The regular physician, of course, cannot be admitted even into this comprehensive fraternity. However varied the professional merits of the *confrères* may be, the field of exertion is one and the same; every department is open to them, and every department is entered by them, while the only one among them who can pretend on public grounds to be properly qualified for the immensely extended sphere, is the *graduated surgeon*, the *surgeon-physician*, or *physician-surgeon*, whatever term he may be styled by, who has taken out a regular medical degree, as well as a surgical diploma. These, we think, will be found to form but a small proportion of practitioners in England; while the others are almost systematically excluded from paying proper attention to the acquisition of medical science; for while the College of Surgeons demands no knowledge of it in those who, under the name of surgeons, are to practise it, the necessary acquaintance with it demanded at the examination in Apothecaries' Hall, is not, we fear, sufficient to supply the deficiency, the apothecary's department being a grade more remote from that of the physician than the rank of surgeon is understood to be; and yet Dr. C. observes, *nine-tenths* of the practice of medicine, strictly so called, devolves upon those who have taken no degree in medicine, and who have barely contrived, in the course of one or two years, devoted to the study of all the branches of medical science, "to pay some little attention to *medicine*;" and should a young man, starting as a general practitioner, find himself well acquainted with it, while he has cause of self-congratulation that he has not to acquire it "during the first half-dozen years of practice, and at the expense of many lives," he will, I think, have to thank himself more than the legal requirements of corporate bodies.

I would not, sir, find particular fault with the English surgeon, as a surgeon, if he confined himself to that department of our profession for which alone his education has fitted him. He may reduce a dislocation, set a fracture, apply a bandage, with dexterous manipulation; while in the treatment of fever, dyspepsia, or even phthisis, I may doubt, and not without reason, both his knowledge and skill. Nor would I insist upon the mere degree of M. D. as the test of all excellence, but merely contend that the holder of such a *regular* degree, is better qualified for general practice than the surgeon, and would suggest that such parts of *his* education as give him this superiority be adopted into that of the surgeon. I have heard it suggested, and perhaps the idea is not a bad one, that different degrees might be conferred, according to the professional attainments of candidates for license to practise; perhaps, such as masters in surgery, bachelors in medicine, &c. which, after a proper course of study and examination, might confer such respectability upon the holders, as to become an object worth contending for by the student of medicine: for what young man is likely to put himself to the trouble and expense of the M. D. if he pretend to general practice, when he knows he must put *it* in his pocket, and *himself* on a level with any apothecary's boy, who settles as a surgeon next door to him? But who, it may be asked, are to confer these degrees? the Hall of Apothecaries, the Colleges of Physicians and Surgeons, and the Universities, will all be up in arms for the defence of their privileges. I would look, however, to public opinion, to the liberal spirit of his Majesty's government, and to the intellectual light of the age; and would hope, though, alas! it looks too like a "forlorn hope," that, as something *ought*, so something *may* be done. I would cast an eye upon the London University and King's College. If they fail in making any alteration and improvement, and other learned bodies shut themselves up within their chartered rights, the work of systematic mischief will, I fear, go on as it does now; for poor good-natured John Bull, if the subject be not brought prominently before him, is not likely to trouble his head much about the matter, but continue to swallow the assertions, as he does the drugs, of any ignorant pretender, who says he is wise without experience, or learned without study. Hoping, sir, that you will excuse the liberty I have taken in thus addressing you, and that these crude suggestions may not be altogether thrown away, I am

Your most obedient servant,
J. S. M.

ON DISINFECTION AND THE PRACTICE OF THE QUARANTINE;

Being an Examination of certain parts of a Pamphlet lately printed by Dr. Ure, on this subject.

THE disinfecting powers of chlorine have been so clearly demonstrated by the success of its application during a long course of years, that there is no occasion for us to make any remarks upon the evidence given by the Doctor; we purpose confining our observations to his new experiments, which we repeated, as follows, on their first announcement:—

1st EXPERIMENT.—Present: C. Lutwidge, Collector; — Rodmell, Comptroller of the Customs; G. Fielding; and J. B. Emmett. Some dry litmus paper was rolled up in a few folds of flannel, which was tied up in a bundle; a powerful stream of chlorine was brought in contact with every part of the surface of the flannel, and continued for more than a quarter of an hour. On unpacking the bundle, there was no change in the paper.

2d EXPERIMENT. — Present: as before. The same was repeated with moist litmus paper. No change.

3d EXPERIMENT.—Present: C. Lutwidge, G. Fielding, J. B. Emmett.

Two equal and similar miniature bales of hemp were provided, in the middle of each of which was placed moist litmus paper. They were considerably pressed, and wrapped in canvas. The dimensions were, fourteen inches long, six broad, and four thick. One was kept in a room to which chlorine had no access; the other was exposed, in a deep earthen jar, to a very dense stream of chlorine, for more than twenty minutes. The jar was then tightly covered. At the end of twenty-six hours the packs were examined; the latter smelled very powerfully of chlorine. The two sets of papers were precisely alike; both were whitened in a very inconsiderable degree, but to the same degree; and we have no doubt that the vapour of packed hemp whitens litmus, or that the hemp absorbs the colour mechanically, as charcoal does.

Now, since in twenty-six hours the chlorine was unable to penetrate through two inches of packed hemp, we beg leave to propose a query to Dr. Ure: in what time, we mean in how many years, chlorine will penetrate to the centre of the lowest tier of hemp bales, which have been packed by powerful screws, or even by the hydraulic press, and which are wedged as tightly as possible in the ship's hold? We must remember, that when porous matters are very tightly packed, they are not penetrable to any considerable depth by even water, whose powers of capillary attraction incalculably exceed those of any gas. This is proved by the leakage and wrecks of ships. Besides, the density of the chlorine, when it proceeds below the surface of the goods, must decrease in a geometrical progression, and may therefore be represented by the logarithmic curve. Hence it cannot penetrate far, in a state sufficient to produce disinfection.

Dr. Ure remarks, in another place, that in one experiment the outside of his bale was corroded by the chlorine. If this be requisite to the production of the effect, those who know how bales are packed together, will see that the corrosion of the outside must ruin the whole, and make it unmarketable. Besides, the doctor has never proved that chlorine, which will just bleach litmus paper, will disinfect effectively—a very doubtful point.

If goods be really infected, they must certainly be unpacked, spread thinly out, and exposed to the disinfecting gas.

With respect to the gravity of the gas causing it to descend and leave the upper part of the apartment without any, we can only say, that it is contrary to what Mr. Dalton has established respecting the uniform diffusion of gases through each other, and to experience. We only beg the doctor to open a bottle of chlorine in any part of a large room, and he will perceive the smell very powerfully, almost instantly, whether opened on the floor or in any other situation.

Geo. Fielding, Ch.
J. B. Emmett.
G. H. Fielding, Ch.*

Hull, 18th August, 1831.

SULPHATE OF QUINA IN THE STRUMOUS VARIETIES OF OPHTHALMIA.

To the Editor of the London Medical Gazette.

Sir,

In your number for July 30, page 515, Mr. Middlemore has surely fallen into an error, in ascribing to Mr. Mackenzie a predilection for bleeding and mercury in strumous cases of ophthalmia. In his paper "On the Utility of Sulphate of Quina in Strumous Ophthalmia," published in the Glasgow Medical Journal for November 1828, Mr. Mackenzie strongly recommends this medicine, both in phlyctenular ophthalmia, or the disease commonly called scrofulous inflammation of the eyes; and in corneitis, an ophthalmia with which a certain degree of iritis is generally conjoined. Extracts from this paper are given in the Medical Gazette, vol. iii. page 234; but it is necessary to have recourse to the original, to see how far Mr. Mackenzie's confidence in the remedy extended at that date.

I am, &c.
A. B.

MR. PATTISON'S "STATEMENT."

To the Editor of the London Medical Gazette.

Sir,

However painful it may be to refuse the feelings of sympathy to a person in Mr. Pattison's situation, he is not to be allowed to promulgate misrepresentations against respectable gentlemen and a large class of students, without some remark. It is evident that the ex-professor does not know the generous character of the minds of English students of medicine: he may stare when he hears it stated that there is no body of men more ready to espouse the cause of a teacher, and whenever there is the slightest disposition exhibited to treat him unfairly, to uphold him with devoted attachment against his enemies. This will be found true even when their professor is not endowed with much eloquence, or a very attractive lecturer. But this generous feeling arises from a definite cause, which it is easy to describe. The students must perceive that their preceptor is devoted heart and soul to their interests—that it is for their advantage that he works with all his powers—that they themselves are the objects of his unceasing anxiety—and not another—especially not himself. If they discover that he is idle, and that there is mingled with this a constant attempt to draw attention to himself, they justly enough abandon him, to make the best of this propensity. If he talks to them with an air of conceit and gasconade of performing the operation of hernia with a penknife—of tying the carotid artery as commonly as any of them have bled in the arm, and of doing many other wonderful feats, the students have common sense enough to know that this is not for their interest—that young anatomists are not to become surgeons by this display of the prowess of their professor. They see in the library the large and splendid work on hernia, by Sir Astley Cooper, full of the most finely-executed and expensive plates, a donation made by that eminent surgeon to the profession, at a great pecuniary sacrifice, as if it were to mark strongly his sense of the difficulties and the importance of the operations of hernia. They know very well that the tying of the carotid artery is not to be treated as an every-day occurrence. Although they may be yet but students, they have a quick-enough sense of what is the proper style of communicating information to them.

I am the farthest possible from pretending to justify altogether the riots and outrageous conduct of the pupils in Mr. Pattison's class-room. The want of discipline was in a great measure to be attributed to the students knowing that there was a Council and a body of Proprietors to whom Mr. Pattison was amenable, (a thing "unique" in the annals of Universities,) and more so to the Council, acting throughout the whole of these transactions with the greatest indecision both towards their Professor and towards his pupils. But when Mr. Pattison alleges publicly, and Dr. Birkbeck reiterates what he says, and the newspapers circulate over the country that his colleagues conspired against him, and that the students were malevolent, and that every one sought his ruin, it ought also to be represented what is the true character of this persecuted individual. We do not mean what is his private character: all that we have to do with is his method of addressing the pupils, and the truth of his allegations in his public statements. These are to be placed in the scale in weighing the merits of this most singular case of alleged conspiracy. Without them it cannot be understood how it was only in Mr. Pattison's class-room, and not in that of any of the other professors, and that it was for three distinct sessions, that this insubordination of the pupils was manifested. The inquiry ought to be fully carried through, as it is necessary for justifying the many against the one.

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* Being engaged on quarantine duty, did not witness the experiments, but was present at the opening of the bales and examination of the litmus paper.

I have neither time nor inclination to examine the numerous charges brought by Mr. Pattison, in his Statement of Facts, against all his enemies—among the students, professors, Select Committee, Warden, and Council of the London University; but I think the last-named body ought in justice to be acquitted of one charge, not the least inexcusable, if it were true. He asserts that he had as his competitor and rival candidate for the chair of anatomy Mr. Charles Bell—that the Council, "after mature deliberation," made their selection, and chose him, the author of this Statement of Facts, in preference to Mr. Bell. But the Council must be exonerated on this head: Mr. Charles Bell never was a candidate for the professorship of Anatomy.

Mr. Pattison has entirely forgotten, in his blind attempt to magnify himself, what the original prospectuses (which I have lying before me) contained. They announced that the anatomical department was to be taught by three professors, whose names are given, but without any appearance of his holding that elevated position which he boasts of; they are—Mr. Charles Bell, Mr. Meckel, Mr. Pattison.

Every one contemplated that the last-named professor, who was totally unknown, was to undertake the rudimental parts of anatomy, and to make his way in public opinion as every man must do who commences lecturing in London. Certificates from America could never support a man in the estimation of London students. The notion which made this announcement in the prospectus acceptable was, that he was to prepare the students for the lectures of the eminent men with whom he was associated.

It was the failure on the part of the Council to induce Mr. Meckel to join their school which made a change in the position of Mr. Pattison. It was not until a year after the first prospectus was published, as his own "Statement" announces, that the Council entertained the idea of adding the late Mr. Bennett to the anatomical department.

Mr. Pattison speaks of his having been elected after "mature deliberation." If there were such delay, we can surmise the reason. It could not be unknown to the Council that he had been engaged in disputes wherever he had lectured; and they must have had to determine, whether the frequent shifting of his scene of lecturing was occasioned, as his certificates declare, by a succession of honourable promotions, or were the consequence of these contests[*]. It is surely an instance of the friendly disposition of the Council towards him, that they interpreted his certificates in the most favourable way.

Being placed where he was, it was the personal interest of every individual professor to prop up their teacher of anatomy, and to maintain for him a respectable character. The interest of the whole school required it, more especially when it was assailed, as it was, at its commencement, by numerous enemies from without. Yet it would appear from the statement. that those who were most concerned in supporting him, were conspiring against him even before his lectures began.

I have the honour to be, sir,
Your obedient servant,
VINDEX.

17th August.

[We should not have inserted the preceding letter had not the name of the writer been sent with it. It has in no degree altered the opinions we had previously expressed in the leading article of the present number.—E. G.]

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[*] See, in reference to this, a letter addressed by him, about this time last year, to the Lancet.

THE LATE DR. BADELEY.

DIED, on Sunday morning, July 24, at his residence in Chelmsford, John Badeley, M.D. in the 84th year of his age. He was the youngest son of Samuel Badeley, Esq. of Walpole, in the county of Suffolk. He took his degree of Doctor in Medicine at Edinburgh, September 12, 1771, and had practised in this town for the long period of 59 years. It would be an injustice to his memory, if this record of dates were to be considered only as a copy of that register—

> " Where to be born and die,
> Of rich and poor makes all the history."

Of Dr. Badeley, who has been so long identified with this town, neighbourhood, and country, it may justly be said, that he has left a void not easily to be filled, whether we consider him as a man, physician, or friend. To his profession he brought an acute penetration, a solid judgment, a benevolent care, great suavity of address, and a most persevering anxiety for the comfort, relief, and cure of his patients, to whatever rank of life they might belong. In society he uniformly exhibited the urbanity and manners of a polished gentleman: among his friends he was hospitable, cheerful, easy, and as willing to be pleased as he was capable of making himself pleasing. If he has not added greatly to the stock of medical science by his writings, (for he had no leisure for such compositions) he displayed his abilities by a most extensive practice, and he preferred the gratification of having living witnesses, who owed their health to his judgment and skill, to the publication of theories, however ingenious, and to the commendation of professional critics. His life was prolonged to a period beyond the common limits of mortality, and in proportion to its length, were its value and utility demonstrated. He lived esteemed, beloved, and respected; he has died regretted, honoured, and lamented.

ANALYSES & NOTICES OF BOOKS.

> " L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The History of Medicine, Surgery, and Anatomy, from the Creation of the World to the commencement of the Nineteenth Century. By WILLIAM HAMILTON, M.B In 2 vols. Lond. Colburn & Bentley.

WHEN reviewing Mr. Moir's little volume, the Outlines of the Ancient History of Medicine*, we entered pretty fully into the consideration of the utility of such works; and being much pleased with his performance, we recommended that gentleman to proceed in his design, which we saw obscurely intimated in his last page, and to let us have in two volumes more, of the same size, such a continuation of the work as might bring his Outlines to the beginning of the present century. We did this with Dr. Hamilton's volumes before us, half persuaded that they did not supply the want in medical literature of which we complained, or, at all events, not seeing why Mr. Moir, who had begun so well, should be baulked in his intentions by the appearance of the Doctor's work, whether well or ill executed It has since, however, on a deliberate perusal, been impressed upon us, that the "History of Medicine, Surgery, and Anatomy," is a work of very inferior merit, and far from being suited to satisfy the expectations of those who feel interested in such subjects. And having ventured to pronounce so unfavourable an opinion thus *in limine*, it is of course our bounden duty to support it with specimens of what we conceive to be the unwarrantable defects, inaccuracies, and even glaring errors of the performance.

Much has been written, and well written too, on the *ancient* history of medicine; and if we comprehend under that epithet all that eventful period which reaches from not exactly *the creation of the world*, (for few historians of the art have pretended to trace back any authentic particulars of it to so extremely antediluvial an epoch,) but from about the time of Hippocrates, that is, about 3500 years later, to the beginning of the 16th century, we shall find the materials, though somewhat voluminous, almost ready arranged to our hand. Even if we had no other groundwork than Le Clerc and Freind, for illustrating the period in question, we could not have better;—Le Clerc, than whom, notwithstanding the flippant mention of him in the work before us, as "a French writer *of no mean abilities*, whose History of Medicine is a work of *considerable merit*," few have adorned the profession with more learning and powers of research; and Freind, a man of genius and profound erudition, whose virtues and talents have not obtained from his country one-tenth of that celebrity which they have deserved. But we have besides these, in the works of Schulzius, Casiri, Portal, and Sprengel, a fund of information illustrative of the earlier periods of medical history, abundant and copious as need be desired.

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* Med. Gaz. p. 368, present volume.

These ample sources, however, vanish almost into insignificance, when compared with the mass of materials which lie before us for the illustration of medical history during the last three centuries. They require, to be sure, a master-mind to elicit their spirit, and to reduce them to a shape fitted for the grasp of ordinary inquirers; but this is the province of him who sets himself up for an historian—it is a duty from which he cannot escape or be excused. It will not do for a writer to present us with a chronological series of medical sketches in lieu of this digest: he must not impose upon us such meagre and crude morsels for *history*: it is become, we know, the fashion of the day, but one is tired of hearing the appellation of history so shamefully misapplied, and it grows a thing no longer tolerable. We are sorry to be obliged to rank Dr. Hamilton among historians of this class; but we cannot help it: he has himself provoked our censure by the pompous title he has bestowed upon his book.

Let us enter somewhat more into particulars. The first volume, which is far the more voluminous of the two, brings us up to the beginning of the sixteenth century, *from the creation of the world*, though "of the years *before the flood*," says our author, "we can catch but those rare and partial glances which Omnipotence has been pleased to reveal in the writings of Moses—glimpses which are *hardly sufficient for the construction of a plausible hypothesis*, or the foundation of a probable conjecture. Like travellers enveloped in an Alpine mist, we wander in darkness and uncertainty without compass to direct or planet to enlighten us!" The more merit then, of course, has Dr. H. in extracting a history out of those *glimpses in darkness*—though certainly we do not find that he has made much of them. Upon turning to that part of the book where we expected to find this period treated, we perceive the matter thus briefly disposed of:—

"Of the state of medicine before the flood, we are destitute of the slightest *authentic* records on which we might form a *conjecture*: we are justified, however, by many circumstances recorded in the Holy Scriptures in *believing* that medical, as well as many other branches of useful knowledge, *had arrived at a very considerable degree of perfection in the antediluvian ages*." Three pages are then occupied with conjectures of various sorts, such as, that we are "justified in a *suspicion* of the identity of *the fabulous Taaut* with *the true Tubal*;" and, generally, that "numberless personages whom we find spoken of as having lived at periods considerably posterior to the great catastrophe of the deluge, *are to be sought for*, if we would find their real prototypes, *in the ages which preceded that memorable event*, and in the very infancy of the human race." Our author quotes Schulze for "the strong probability that Adam, yielding to the all-authoritative voice of necessity, discharged the office of accoucheur to his wife, and thus performed the first operation in surgery."

We recollect a better conjecture than this, concerning the first exercise of the art of surgery—we mean that of Sir Thomas Brown, who, in one of his most learned treatises, is pretty positive that God himself performed the first surgical operation when he removed one of Adam's ribs for a certain purpose—and healed up the wound, we suppose, by the first intention!

We give Dr. Hamilton's summing up of the antediluvian period: it will excite a smile from the reader who shall attend to its elaborateness, while it cannot but surprise him by the inconsistency which it manifests with a passage which we have just now extracted:—

"In referring, with Brambilla, the

origin of surgical instruments to a period so remote as the second generation from Adam, it must not be supposed that men had, in those early days, made *any very great* proficiency in the art of surgery, or that the instruments which they employed were of other than the rudest and most inartificial construction. *As the medical knowledge of the Antediluvians may be reasonably presumed to have been confined to the exhibition of a few vegetable simples of the mildest character and most innocuous activity, so we may justly conclude that their surgical skill was equally restricted* to the treatment of the most ordinary wounds and the application of the least complicated bandages; to the reduction of a dislocated joint, or the setting of a broken limb. The amputation of a sphacelated leg, or the trepanning of a fractured skull, would have been a flight of skill far transcending *the narrow limits of Antediluvian knowledge*—a deed of daring far beyond the cautious timidity of *an age of inexperience*; and the practitioner who should have had the hardihood to attempt the operation of lithotomy would have been viewed as little, if at all, better than a murderer."

But escaping as rapidly as possible from the regions of fable and romance, we pass on to the account of Hippocrates, given in the next chapter, expecting to find our author's talent for conjecture turned to some account in elucidating the private history of the father of medicine. Will it be believed, however, that Dr. Hamilton does not even favour us with what few facts have been, almost by universal consent, attributed to the Coan sage? That he was the son of Heraclides and Praxithea—that he was the seventeenth in descent from Esculapius on the father's side, and the twentieth from Hercules on the mother's—and that he travelled even amongst the Scythians in quest of knowledge, might be passed over by our author as too apocryphal; but he who has displayed so much ingenuity in his antediluvian conjectures, might not have disdained to give us his opinion of the probability of Hippocrates having practised in Athens during the pestilence, and having been rewarded with a golden crown there for his services, notwithstanding the silence of Thucydides on the matter; or whether the father of physic had Herodicus and Gorgias for his early preceptors; or whether he had the pleasure of being able to pronounce his old master in philosophy, Democritus, who was supposed to be the maddest, to be really the wisest man in his city. All this would have been both amusing and instructive in Dr. Hamilton's hands, but he has done nothing with it. The only facts he touches upon in the biography of Hippocrates, are contained in these two sentences:—

"This celebrated physician was born in the first year of the 80th Olympiad, *or between* 400 and 500 years before the commencement of the christian æra, in the island of Cos or Coos, situated in the Ægean sea, at no great distance from Rhodes: an island singularly distinguished by having given birth to two of the most eminent men in their respective professions among the ancients—Apelles, the first painter, and Hippocrates, the first physician of his age."

"His valuable life was prolonged considerably beyond the ordinary duration: he died at Larissa, a town of Thessaly, about 361 years before the birth of Christ, at the advanced age of 101 years."

It is rather curious, we may observe *en passant*, that Dr. Hamilton, so classical a man, should mention the first year of the 80th Olympiad as some year between the 400th and 500th before Christ; and that he should afterwards state Hippocrates to have died in the year 361 B.C. æt. 101. If Hippocrates was born in 80th Olymp. 1, and lived to be 101 years of age, Dr. H. should know that the father of physic certainly died in 359 B.C. These, however, to be sure, are trifles; and if our historian has not given us the private life of Hippocrates, he has given us plenty of his theories, it may be said, in lieu. To tell Dr. H. the truth, we are heartily tired of this eternal *decies* or *millies repetita* reiteration of the theories of Hippocrates; we are surfeited with the *toujours perdrix* of the copyists of Le Clerc, especially when they proceed with their diatribes as if they were original investigators of the writings of the Coan. As for Dr. H. he seldom, it is true, favours us with references; but in this instance, with the exception of one passage acknowledged to be from Cabanis, he thinks proper to ascribe, at least we may judge so by his silence as to authorities, his whole account of Hippocrates's *doctrines* exclusively to Hippocrates and himself.

Galen is treated almost as cavalierly. We extract what is said of him :—

"The illustrious Galen, who deservedly occupies so conspicuous a station among the fathers of medicine, was the son of Nicon the geometrician, and born at Pergamus, *a city of Asia, celebrated as the place where parchment was first manufactured.* [What an interesting piece of information by way of parenthesis !] The date of his birth is not known with certainty, but he is generally believed to have flourished under the reigns of Trajan and the three succeeding emperors, attaining, it has been said, by extreme care, notwithstanding the natural weakness of his constitution, an age of *no less than* 140 *years.* Having made himself master in his youth of all the logic and philosophy which were usually taught in those days, he applied to the study of medicine under Satyro and Pelops, whom he soon outstripped by the rapidity of his attainments. He appears to have first distinguished himself as a medical writer under the reign of Adrian, or about the year 130, at which period he attracted considerable notice both as a teacher and practitioner."

Now the fact is that Galen, according to the best authorities, was born in A D. 130, and died in 201, æt 70. We wonder what Dr. Hamilton could have been thinking about when he adopted such inaccurate statements.

But we come now to a period when the original research of our author is put to the test. Le Clerc parts company from him with Galen, when Freind is said to begin his guidance. This, however, is not quite correct. Freind's history, though said in the title-page to commence from Galen, actually commences with Oribasius, who flourished *full two centuries* after *the* Physician of Pergamus. *Not a word does Dr. Hamilton say illustrative of the interval.* After finishing off Galen, and devoting a few lines to a little talk about "a night of Cimmerian darkness" which followed thereupon, "relieved only, at times, by the transient splendour of a few passing meteors," &c. he comes at once to Oribasius.

"Pergamus, which deservedly boasts of having been the birth-place of the illustrious Galen, [*and of parchment*, he might have added] also claims the merit of having given birth to Oribasius, who appears to have been born about the year 351, in the time of Julian the Apostate, and to have received the first rudiments of his education along with Magnus and Ionicus, at the school of Zeno, the Cyprian, at Sardis, from which circumstance Oribasius has been erroneously supposed to have been a native of that town," &c.

And so he goes on, wonderfully full about Oribasius, with Dr. Freind's assistance; but not a word about Serenus Sammonicus father and son, Alexander Aphrodisæus, Elian of Prœneste, or any other eminent physician who flourished in the interval. We are sorry to see this, it betokens so blind an adherence to the tracks of one or two of the most obvious authorities; but we must proceed: pass we, without delay, into the midst of the Arabian physicians. And here in the outset we must observe, as a remarkable circumstance—not a word from Dr. Hamilton respecting his immense obligations to Dr. Freind. We come to Averrhoes; and we shall see into what a quandary our author plunges himself by his disdaining references. After telling us that Averrhoes became an unbeliever, and was in consequence charged with heresy and thrown into prison, but released upon a *comparative* view of his errors, the historian of medicine coolly thus proceeds :—

"*Bayley* has collected many anecdotes respecting Averrhoes, from a variety of authors, by whom he has been led into a multitude of errors: as, for example, when he speaks of his enmity with Avicenna, which he assigns as a reason for his never making mention of him: a statement which only proves that *Bayley* blindly followed the authority of the writers he consulted, without once taking the trouble to refer to the writings of Averrhoes himself, in which he would have found the inaccuracy of this statement. *Bayley* likewise labours with no small assiduity to fix the charge of impiety upon the memory of Averrhoes, whom he represents as denying the immortality of the soul, and the existence of future rewards and punishments; as though it necessarily followed, from his questioning the absurdities of the Mahometan faith, that he denied all the great and fundamental truths of religion: or, as if the fact of his detecting the adulterations produced by the alloy argued his inability to distinguish between them and the precious metal which they debased. Had *Bayley* sought for a confirmation of this charge also in the works of Averrhoes himself, he would have discovered it to be utterly destitute of foundation, and to have originated solely in the malignancy of his traducers; for Averrhoes has, in one treatise, expressly maintained the *immateriality*, and in another, the *immortality* of the soul: facts which fully disprove this idle and malicious charge."

Now who is this *Bayley*, with whose critical delinquencies our author is so familiar—whom he undertakes to censure for his blindness in not examining authorities—for his labouring to fix the charge of impiety on Averrhoes—and for not having looked into the works of the Spanish-Arabian in their original purity?

We pray our reader's attention for a moment, while we answer the question, and at the same time expose as ridiculous a piece of pretension as we ever remember to have met with.

The *Bayley*, then, of the above extract is no less a personage than the great critic, *Peter Bayle*, whom the historian of medicine has thus *traduced*, and of whom he does not seem to have ever possessed the slightest knowledge. We were a little puzzled ourselves, we must confess, at first; but recollecting that Dr. H.'s sources were rather limited, and that most probably the critical notice above cited was shadowed out from Freind in some manner, we turned to that author and found the original, unadulterated, unperverted passage, without any difficulty. We observed that the words of Dr. Freind were most curiously garbled—not to say totally obscured—as they were given by our historian; and it was not until we still further recollected, that Dr. Hamilton used for his compilation the Latin translation of Freind's history, that we were in some measure relieved from our astonishment. The first sentence or two of the Latin version will suffice to inform the reader concerning Dr. H.'s process of history writing:—

"Multas res ex variis auctoribus ad Averrhoem pertinentes collegit *Baylus:* sed cum hic illius scripta, uti videtur, nunquam aspexerit, auctores istos fidenter sequens, ab iis sæpe in errorem ducitur. Uti cum," &c.

And the corresponding passage in the original English will shew how much our author deviated from the right path, by taking his information from second-hand authority:—

"Mr. *Bayle* has collected a great many passages out of authors, relating to Averrhoes; and as he seems never to have been acquainted with the original, he follows these authors implicitly, who often mislead him. As when he tells us from Champerius, that he was a bitter enemy of Avicenna, and that for this reason he avoids ever naming him, which he does in this very book often," &c.

Having our curiosity rather whetted by what we had thus discovered regarding Dr. Hamilton's notions of a *Syngraphein*, we were induced to compare other passages in his book with those corresponding (?) in the author from whom he took them; and our clue often led us unexpectedly to some surprising inaccuracies, which weighed the more with us, as our author evidently entertains much conceit of himself as a classical man. For example: in his account of Avicenna (which, by the way, is even *abridged* from Freind, in whose history the brevity of the notice of the illustrious Persian is perhaps its greatest fault), he tells us that Avicenna "resided chiefly in the city of Ispahan, and was accused by the Arabs of an immoderate indulgence in pleasure, which subjected him to a variety of complaints, whence *it became proverbial among them* to say, that *neither the study of philosophy contributed to virtue, nor that of medicine to the preservation of health.*" Now this last clause is vilely translated from the Latin translation, which runs thus:—"*deque illo* iis temporibus vulgare dictum erat, neque philosophiæ studia ad virtutem, neque medicinæ ad tuendam valetudinem quicquam *ei* contulisse:" and thus it is that Dr. Hamilton treats his friend Dr. Wigan, than whom, we are bound to say, he could not have had a more faithful guide, had he but availed himself steadily of his services. The passage in the original, we may add, is this:—"and it was a saying, they tell us, in those times, that all his philosophy could not make him moral, nor all his physic teach him how to preserve his health;" upon so slight a foundation has Dr. Hamilton reared his superstructure of a golden proverb current among the Arabians.

We find that we now have, in our cursory remarks on the "History of Medicine," filled a larger space of our critical department than we had intended to devote to the work; and we have not touched upon the second volume yet. Perhaps, then, it may be as well to stop here for the present, and to reserve our observations on the remainder of the work for another number. We confess we feel a very high degree of interest in the subject itself—independently of the merits of the particular production before us—nor can we assign any other reason for having dwelt upon a portion of it so long.

MEDICAL GAZETTE.

Saturday, August 20, 1831.

" Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

MR. PATTISON'S CASE.

THE ex-professor of anatomy and surgery in the University of London has at least gained one point by his unceremonious dismissal: the embargo of silence is taken off him, and his story is now out before the public. And such a story as it is!—a catalogue of some of the most damning facts we ever saw in a public document. We defy any man to read it through and to say that the individual whose grievances it professes to record, is not an exceedingly injured person—that the scene from which he has been driven is not the most disturbed and undisciplined in the British Empire, and that the persons calling themselves the Council are not, as a body, the most grossly incompetent set of men that ever dreamt of setting themselves up to preside over an University. From first to last—from the very appointment of Mr. Bennett to the singular station allotted to him, down to this dismissal of Mr. Pattison, accompanied by the sign manual of their own stultification, there has been nothing but one continued tissue of blundering and mischief-making on the part of the Council. Nothing can be stronger than the proofs of this which Mr. Pattison's pamphlet affords: we shall select some of them; and though we may not pledge ourselves for the accuracy of all his assertions, we believe, upon the whole, that the ex-professor is, as Dr. Birkbeck has said of him, " remarkably free from exaggeration."

We might even go anterior to Mr Bennett's appointment in search of a grievance inflicted by the ill-management and unfairness (not to give it a worse name) of the Council. Mr. Pattison was *assured* that a certain salary should be paid him, reckoning from the day of his engagement with the University: but after travelling into Germany in the service of the establishment, and having spent more money than was allowed him, upon applying at the end of the year for his salary, he was informed that as no minute of it had been entered he had no salary to receive, nor did the Council feel themselves bound to pay him any thing for his services, nor for the very heavy expenses incurred by his remaining in London. This was at a time too, when, as Mr. Pattison says, he had sacrificed at least five times the expected salary by not returning to America. But Mr. Bennett's appointment is decidedly the æra from which the Professor's difficulties must be dated. The wise men in the management never once saw that they were perpetrating a conjunction unheard of in any other medical institution in Europe—namely, the creating an officer whose duties closely connected him with the pupils, and whose interest it would be to elevate himself by bringing the talents and reputation of his Professor into disrepute. They were warned abundantly of the consequences of this measure, yet, as if labouring under some infatuation, they took not the slightest notice, but pursued their own headlong course. In a few months they were pestered with anonymous complaints against their professor, and were weak enough to order them to be investigated. Mr. Pattison was put upon his defence, and Mr. Hume was constituted his judge. Mr. Hume, we know, is an amateur in these matters, and he executed the task assigned him, says Mr. P., with impartiality and fairness. The return was, that there was no legitimate ground of complaint.

We need not dwell on the well-known story of Mr. Bennett's indecorous behaviour to Mr. Pattison in public—we mean at the first distribution of medical prizes at the University. The circumstance was well bruited abroad, and certainly did not tend much to advance the interests of the establishment. Between the parties themselves it occasioned, naturally enough, an open rupture, which, we believe, was never even pretended to be closed again.

The plot now thickens: a new character comes upon the stage, and this in the person of the late warden of the University, Mr. Leonard Horner. He performs a part of the most extraordinary officiousness that, we think, we have ever witnessed. The scene is in Scotland, where the worthy warden happens to be spending the vacation. He hears that Mr. Charles Bell is in the neighbourhood; and without more ado, acting upon his own authority, calls on Mr. Bell, begs of him to accept Mr. Pattison's chair, and supposing that his solicitations had been successful, writes to the Council to confirm "this act of disinterested liberality." The Council for once shewed their sense, and rejected the impudent application; but this seems to have been only the first overt act of a system of hateful interference afterwards practised amply by the warden towards Mr. Pattison.

In the second session, new charges of a more directly tangible nature were preferred against the professor, by students whose names, according to stipulation, were to be kept secret. This led to two investigations; one by a select committee of the Council, the principal members of which, Messrs. Brougham and Denman, expressed themselves fully satisfied of the groundlessness of the charges; and the other by four of the faculty, Drs. Conolly, Todd Thomson, Turner, and D. Davis, who not only procured positive evidence from the students themselves, that the charges were vexatious and false, but deemed it their duty to warn the Council against the danger of giving ear to attacks on character like these, that must at once disturb and disgrace the institution.

All this did not repress the eagerness of the Council, or perhaps we should rather say, of their principal organ, to obtain further grounds of accusation against the professor. An individual pupil at length came forward, and stated openly that, in *his opinion*, Mr. Pattison was incompetent to teach anatomy, and ought to be dismissed! What was the conduct of the Council upon this most disgusting breach of discipline, occurring under their very eyes? Their officer was directed to write to the ungenerous recreant, "that an examination (another!) into the conduct of a professor could not be instituted upon the representation of *one* pupil." The hint was sufficient; the *one* recruited for *more*, and succeeded eventually in procuring *fifteen* signatures of fellow-pupils to a memorial on Mr. Pattison's ignorance. The professor was *again* put upon his trial, and again were the Council obliged to come to their former decision, that these, like the former charges, were *utterly groundless*.

But it will be impossible to appreciate the boldness of the Council in venturing to give this decision, unless we take into consideration the formidable petitioners, or memorialists, whom they had to deal with. *The following* is a specimen of a *P.S. appended* to one of the documents sent in during the investigation:—

> "Should you, however, neglect our prayer, we warn you that we shall publish this very appeal—shall lay bare the deep and unanswered malignity of the mis-statements attested by Dr. Birkbeck—the mean, party-spirited, wilful duplicity, by which he, as the supporter of Mr. Pattison, tried to make the proprietors believe that one student only charged that gentleman, and that an ignorant student. Could Dr. Birkbeck, a member of the Council, a canvasser for Mr. Pattison, the head of the party against our Warden, and the cultivator of these cabals—could he say that he was ignorant of the seventeen pupils, including the great majority of the medalists, who in a phalanx charge Mr. Pattison with a palpable dereliction of duty, and urge his dismissal? If so, why is he retained in the Council? Did he know of these facts, why did he hide them? Because he had an end to serve. Why, then, we boldly ask, is he retained a proprietor of the Institution? Why have his mis-statements regarding Eisdell's ignorance, an insult which we all consider personal to ourselves, not been officially contradicted? &c. &c."

And this is language addressed by the students of an *University* to the governing body, one of whom is even singled out for especial censure, and all but commanded to be expelled! Were the authors of this audacity made an example of? Was a sweeping sentence of expulsion pronounced upon them? By no means: the spirited lads were borne with—were encouraged.

Soon after this last investigation, Mr. Pattison was induced to share his professorship with Mr. Bennett, and obtained from the Council the vacant chair of surgery, by way of compensation, and, it is to be presumed, in token of the unscathed character with which he emerged from his recent ordeal. It would be tedious, however, to trace all the petty annoyances to which he was still exposed, and for which he deems himself principally indebted to his good friend, the Warden. It is certain, says the ex-professor, that when similar proceedings were again going forward, "the Warden *wrote, in the council-room of the University, a letter for these insubordinate pupils to lay before the Council, for the purpose of having another inquiry instituted against me.* If Mr. Horner will deny the fact, I have the most unquestionable evidence to prove it. But Mr H. will not do so: he knows too well, *although he took the precaution to have it copied, by one of the students who was present at the time*, so that his own handwriting might not rise up in judgment against him—that the pupil he entrusted with his confidence did not keep his secret." We have no words strong enough to express our opinion of the conduct here exposed; and haply, neither have we space to devote to a more particular detail of the cabals which were still most disgracefully organized in the Professor's class-room. One or two circumstances, however, relating to this period, are too remarkable to be omitted. After relating some facts of a vile description with respect to certain acts of insubordination practised upon him in his theatre, he says, "the same day after I had finished the lecture, and retired to my private room, I was followed by a Mr. Peart, who had been one of the ringleaders in exciting the pupils to acts of insubordination, and had officiated as chairman at one of their meetings, a student who had rarely entered my class room, and to whom I had refused, on account of the irregularity of his attendance, to grant a certificate. On his entrance he demanded his certificate. I told him mildly, that for the reasons I had already stated to him, I could not grant it. A student who was with him, said insolently, "Mr. Peart has attended you as regularly as any body else." I did not reply to this, and Mr. Peart came up to me and said, "you are no gentleman." Having offered me this insult, he returned to the class-room, where a considerable number of the pupils still remained, and on being asked what he had done, he told them that he "had told Pattison he was no gentleman, and that he did not resent it!" Immediately afterwards he sent his attorney to serve a writ on me, to force me to grant him a certificate." Gross as this conduct was on the part of Mr. Peart, it should be added, that he found a palliator in no less a personage at the Council Board than Mr. Warburton: that gentleman was for excusing the personal insult, on the ground that it was offered in the Professor's *private room*—not his public theatre! But the Council put a finishing hand to the business, by letting the delinquent off, upon his making a sort of apology *to them;* after which they not only re-admitted him to Mr. Pattison's lectures, but, says that gentleman, "the intimation which conveyed that information to me, contained a direction *to grant Mr. Peart his certificate*, to which his attendance did not entitle him." We pause for a moment in astonishment at this singular proceeding in the heads of a university (!); and we only ask whether it does not imply, in addition to the grossest injustice to their Professor, the meanest truckling to the will and pleasure of the pupils; together with (what seems very like) the vilest dishonesty towards the public?

After this, what could be expected? Scenes were performed in the lecture-room that defy description; and *a standing committee* of the rioters issued their orders daily, whether Mr. Pattison should be heard in his own lecture-room or not. But were the Council aware of the existence of such a committee? Hear Mr. Pattison: "I answer, incredible as it may appear, they were not only informed of the fact by me, but they actually *communicated officially* with the chairman of that body!"

The climax of ridiculous, yet ruinous absurdity, was apparently now attained; but the Council had still some *duties* to execute. New charges were gathered against the ill-starred professor, and another investigation as to his competency or incompetency was determined on. Mr. Pattison proposed, that the *ipsissima verba* of his lectures for the remainder of the session should be taken down by a professional reporter—even by *our own* accurate stenographist; but this proposal was declined, and a method worthy of the Council adopted instead. This, as we reckon, was *the seventh* and last trial of the professor's ability. "Lord King, Mr. W. Marshall, and Mr. Merrivale, not one of whom knew a nerve from an artery, constituted themselves the judges of my anatomical pretensions! The proceedings of this committee became too ridiculous for even the students to stand it. The anatomical engravings belonging to the medical library were carried into the Council room; and with these before them, and with the assistance of anatomical dictionaries to explain technical terms, these gentlemen gravely deliberated on the amount and correctness of the anatomical knowledge possessed by the professor of anatomy!"

These methods, however, it now became manifest, were too slow for the purposes of those who would have the Professor out by all means. A Mr. Thomas Wilson gave notice that he would, at the next general meeting of the proprietors, move, "that Professor Pattison be recommended to retire from his professorships of Anatomy and Surgery." This announcement naturally led to much correspondence between Mr. Pattison and Horner, who was still in office, but called forth a feeling, if not an indignant remonstrance, from six of the most distinguished general professors belonging to the institution. Nothing can be more flattering than the testimony borne in this document to the talents of the individual in question, or more strongly expressive of disgust at the unparalleled annoyances to which he was exposed. We have not room or we should certainly give this document insertion, as well as, perhaps, the separate, and, if possible, still more decisive letters of ex-professor Conolly and Dr. Lardner. But we must close our present paper, which has become longer than we had intended. On the 23d of last month the finishing stroke was put to the business by Professor Pattison receiving a letter from the clerk of the Council, containing the following resolutions;—

> "Resolved—That Professor Pattison be, and he is hereby removed, from his situation of Professor of Anatomy and Surgery in this University.
>
> "Resolved—That in taking this step, the Council feel it due to Professor Pattison to state, that nothing which has come to their knowledge with respect to his conduct has in any way tended to impeach either his general character or professional skill and knowledge."

And the Statement ends with a brief appeal to the proprietors on the character and prospects of the University, which has been the scene of these strange transactions. "Lastly," says Mr. Pattison, "I would call the attention of the proprietors to the ruinous operation which the mismanagement of the Council has exerted on the interests and prosperity of the University. * * *In the University of London, the system pursued has been to degrade the character and authority of the professors, and to court, by the most contemptible compliances, the favour of the students.*"

We are bound to add, in conclusion, that so far as we are acquainted with the transactions treated of in Mr. Pattison's *exposé* (and, as our readers know, we have not been inattentive observers of the various movements which have taken place in the London University, from its very commencement), we must say, as we said in the beginning, that it is, as a statement, remarkably free from those exaggerations and mis-statements which would be very apt to slip into the narrative of grievances of an ardent and much-injured man. At the same time we must beg leave just to notice, at parting, two little inaccuracies which the writer has fallen into, and which he would do well to set right as soon as he conveniently can. In page 1, he states that Mr. Charles Bell and Mr. Mayo, among others, were his competitors for the chair of anatomy. This is not correct. Mr. Bell might have had that chair at the time, without competition, if he had wished to have it; and as for Mr. Mayo, though at a subsequent period he was, we believe, a candidate for the chair of surgery, we do not conceive it possible that he could have sought the professorship of anatomy at the opening of the London University, having just purchased the school in Great Windmil-Street from Mr. Bell. Again, in page 41, he quotes a passage which he says Mr. Bell delivered in his "valedictory lecture." It should be, and we believe it is pretty well known, that Mr. Charles Bell never delivered any such lecture.

ALARM AT HULL.

In consequence of information received by the Board of Health, Dr. Daun, who had but recently returned from his visit to Port Glasgow, was dispatched to Hull a few days ago. The cause of alarm in this quarter was apparently better founded than in the preceding instance:—a soldier was seized at three o'clock in the morning with vomiting and purging, accompanied by cramps, coldness of surface, prostration of strength, and other urgent symptoms, under which he expired at eight P. M. of the same day. The gates of the barrack were shut, the troops placed under quarantine, and, as may be supposed, the greatest dismay prevailed in the town. On investigating the circumstance it was ascertained, that, though in general a person of temperate habits, the unfortunate patient had been indulging to excess in the festivities, which, we understand, take place at this season among the lower classes of the people at Hull. He had eaten an immense quantity of fruit, and been in a state of intoxication for nearly three days successively, and the night before his attack had been dancing, or attempting to dance, in a room rendered excessively hot by the state of the weather, as well as by the crowd which was present. These circumstances sufficiently account for the fatal result of this case, and are of great importance, as shewing the effect of intemperance in increasing the severity of the cholera of this country. A notice on the subject has been published by the Mayor of Hull; and as no other case of the same kind has occurred, the communication with the barrack has been opened, and confidence restored.

ABOLITION OF QUARANTINE IN RUSSIA.

THE Russian government has done away with all quarantine. The reason assigned for this is, that the whole empire being infected, nothing remains to be gained by the restrictions, which were in other respects inconvenient and imperfectly fulfilled. It is stated in the official order, that travellers will be delayed bu a very short time; any who may be ill of cholera on board vessels, or boats, will be sent to the hospitals, and the conveyances suffered to proceed after having been fumigated. It is curious that, although this announcement, which implies quarantine to be useless or unnecessary, is contained in a decree of the Emperor, a double sanitary cordon is nevertheless kept up round the court at Peterhoff.

DR. BARRY'S ACCOUNT OF CHOLERA.

WE subjoin an extract of a letter from Dr. Barry, which contains the most striking and graphic description of cholera which has yet fallen under our notice:—

St. Petersburgh, July 20, 1831.

The disease is certainly somewhat mitigated, both as to the number and the fatality of attacks, though the weather has continued unchanged. Thermometer in our apartment steadily above 70° of Fahrenheit; very little wind, and what there is, constantly from the east, with the exception of about twenty-four hours last week.

Names for diseases or medicines, so contrived as to constitute little definitions, are bad things. I came here with an impression strongly fixed upon my mind, that the essential and dangerous features of cholera morbus were immoderate and ungovernable vomiting and purging of a serous fluid, violent spasms, and the exhaustion and collapse necessarily attendant on such a state; consequently, that the first indication would be, to restrain these depressing evacuations. The fact is, however, that vomiting and purging are amongst the least important symptoms of the present epidemic, though the appearance of the fluid evacuated is highly characteristic. Rice water strained and allowed to settle down, is, when shaken up, the best type. The evacuations, both upwards and downwards, either soon cease, or are easily repressed; while in many cases, and these the very worst, there are either none, or they are very trifling. It is the sudden paralysis and rapidly diminishing action of the heart, of the arteries and of the organs of respiration, with the stasis and thickening of the blood, the loss of the power to generate heat, that constitute the real danger of the first—the most fatal stage of this disease. Blue, black, flat lines, mark the course of the larger veins; a deadly livor colours the skin; even the tongue is icy cold; the respiration is short, quick, and imperfect; the scrobiculus cordis and diaphragm drawn violently upwards and inwards; the pulse and voice extinct; the limbs and belly torn with spasms; the hands and feet shrivelled, corrugated, and much diminished in volume; the reason unimpaired. It would seem as if all the colourless cells and vessels, upon which the turgor or plumpness of the integuments so much depends, were squeezed to emptiness, and nothing left but the thickened colouring matter of the blood. If this state cannot be overcome in a very few hours, the sufferer must die. *Mordechi*, or *mort de chien*, or *mort noir*, would, either of them, be a much more appropriate name for this inexplicable malady, than that by which it is at present designated. I am now quite convinced that neither Celsus nor Sydenham ever saw this disease, else they surely would not have omitted *all the symptoms* that I have just enumerated.

It is in the above state, particularly if there be violent spasm, that the magisterium bismuthi has been found so serviceable, assisted by cordials, synapisms covering the whole belly, and frictions. Neither warm baths nor vapour baths will do. The body is warmed by them, as a dead animal would be, but the faculty of generating heat not being restored, the patient cools down rapidly again, and with increased debility. Opium appears really to be contra-indicated, unless to allay vomiting and spasm, which the bismuth effects much better; and calomel they have not ventured to give in large doses.

Two physicians, (Germans,) Ysenbeck and Brailow, stated publicly and firmly yesterday, in my presence, at the Medical Council, that during the preceding eleven days they had treated, at the Customhouse Hospital, thirty cholera patients, *of whom they had not lost one.* They give two table spoonfuls of common table salt in six ounces of hot water, at once; and one table spoonful of a similar mixture, cold, every hour afterwards. They always begin by bleeding.

But in the ordinary way of treating the disease: suppose the first stage safely past; very rarely indeed—not five times in the hundred—does the patient return to health without passing through a dangerous fever, which not unfrequently assumes a typhoid character, with reddish-brown dry tongue; stupor; suffused eye; constipated and tender belly; dark sordes about the lips and teeth. The pulse, however, is generally quicker, and the skin hotter, than in primitive typhus. In this state many die from the fourth to the seventh day, and even later. In other cases the fever is benignant, and goes off within the fourth day by copious perspiration.

My object in entering into this detail is to warn you that many, and fatal cases of the present epidemic, may occur with little or no vomiting or purging. The shrivelling of the fingers and toes, the colour of the skin, the shrinking of the features, the coldness of the tongue, the feebleness or extinction of the pulse and voice, the rice-water evacuations when there are any, are the true marks of the disease, not to be mistaken.

A SHORT TREATISE ON CHOLERA.

We last week gave a few specimens of the mode in which cholera is contemplated by certain French writers. The following seems not unworthy of being preserved as a curiosity of the same kind: it is the most summary document on the subject we have seen—quite French—*à la bulletin*:—

(*From the* Messager des Chambres *of August* 14.)

CHOLERA MORBUS.

The cholera of the ancients, the Chinese *holouan*, the Indian *maudechin*, the Persian *outbb*, the Arabian *houwa*, the *troussegalant* of France, and the cholera morbus which spreads its ravages over the north of Europe, are all one and the same disease.

Modified by the difference of climate and change of seasons, the cholera loses much of its violence in winter, and increases in summer. It may also exist in conjunction with other epidemic diseases, and occasionally render them so complicated, that the real character of the affection cannot be discovered, as has occurred at Warsaw, where the cholera commits less ravage than the typhus in our army. We have grounds for hoping that the severe discipline of our army, the exertions of the police in our large cities, especially the excellent practice in our hospitals, both civil and military, will preserve us from this dreadful scourge.

The cholera morbus is not contagious, but so rapid in its effects that the slightest delay in checking its progress may lead to the most fatal consequences.

LABAT, D.M.P.

CAJEPUT OIL IN CHOLERA.

To the Editor of the London Medical Gazette.

46, Dover-Street, 17th August, 1831.

SIR,

MY engagements will prevent me from forwarding to you this week, for insertion in your valuable journal, the details of the severest case of cholera (with collapse and spasms) which I ever witnessed, and to which I was summoned at a little before two o'clock A.M. on Thursday, the 11th instant. In its progress, one hundred and fifty-five drops of Cajeput Oil were given, within seven hours, in doses of twenty-five, fifty, forty, and again forty drops, in a little warm water, with the most satisfactory result: other medicines were employed, without advantage The details of this interesting case I hope to be able to send you next week.

I remain, Sir,
Your very humble servant,
M. J. TIERNEY.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

MEATH HOSPITAL AND COUNTY OF DUBLIN INFIRMARY.

Cases treated by Mr. Hamilton, *under the superintendence of Dr. Stokes; with Mr. Hamilton's remarks.*

Diarrhœa after Ague.

MICHAEL FINNIGAN, aged 19, a labourer, somewhat emaciated, admitted into the hospital February 9, 1831, with diarrhœa, under which he has laboured three weeks.

His history is as follows.—During the months of August and September he worked in the fens, in Lincolnshire, and returned to Ireland in perfect health. About three weeks after his arrival he was seized with ague; the cold stage commencing in the morning, at ten o'clock, followed by hot and sweating stages. The fits recurred each day at the same hour. After remaining quotidian for two or three weeks he got some medicine, which greatly diminished the severity of the ague, rendering it irregularly tertian and quartan, and each succeeding fit much less violent than the preceding one; influenced, however, a good deal in these respects by the weather. In spite of this amendment he continued very weak, and about three weeks since was attacked with the diarrhœa under which he now labours. It commenced with pains in the abdomen, followed by watery stools; four or five of which were passed in the day. At times the stools were bloody and mixed with mucus, particularly after irregularity in diet, or the use of ardent spirits or porter, the drinking of which last occasioned a feeling of soreness in the stomach and bowels.

Since the first appearance of the diarrhœa he has constant pain in the head, often severe griping pains in the abdomen, and occasional vomiting.

The fits of the ague occasionally recur; one of these he had last night. He has also a slight dry cough, worse at morning and evening, and sweats during sleep. On examining the abdomen, the liver can be felt in the upper parts of the right hypochondrium and epigastrium, over which the sound, on percussion, is dull, the dulness extending upwards, to within a short distance of the right nipple. The spleen also can be felt in the left hypochondrium, a short distance beneath the ribs. The rest of the abdomen, which feels soft, is painful on pressure, particularly in the umbilical region. In the chest, the sound over the left clavicle, and some way beneath, is dull, and respiration feeble. Pulse full, and slow; tongue covered with white fur, red at tip.

Ordered mucilaginous drinks. Hirud xii. abdomini. Enemata Opiata duo.

10th.—Rep. Hirudines. Enema Opiatum unum. Pot. Muc.

12th.—Feels better; countenance much improved, having lost the dingy yellow hue it had on admission; belly less tender; purging stopped.

Rep. Enema et Potus, Vesicat. Abdomini et

℞ Pulv. Dov.
Hyd c. Cretâ. aa. gr. iii.
Omni tertiâ horâ.

18th.—No pain any where, but some slight tenderness in the epigastrium. No stool yesterday. Pulse regular; tongue loaded.

Rep. Med.

21st.—Got up yesterday, and felt pretty strong. No headache; no pain or tenderness in abdomen, in which the spleen cannot be felt; appetite and sleep good; no sweating; pulse slow and soft; tongue loaded. No medicine.

24th.—Experienced a feeling of cold at the stomach, with a dull pain in the loins and an inclination to stretch—symptoms he states always to have preceded a fit of the ague.

R Pil. Hyd. gr. ii.
Sulph. Quin. gr. i. M. bis in die.

A few days after, he was dismissed, perfectly cured.

Remarks.—The length of the latent period of the ague in this case is very curious, a month having nearly elapsed after exposure to the marsh-miasm before the disease shewed itself. Dr. Marsh, in his paper on the origin of fever, remarks that he has known the latent period even longer than this; Irishmen, as in the present instance, labouring in the fenny districts in England, and exposed to the malaria, not shewing the disease till months after their arrival in this country. The difference in the length of the latent period he considers to be greatly influenced by the presence or absence of exciting causes.

The ague, in this case, came on in the latter end of autumn, and appears, from the patient's own account, to have been of the quotidian type—an unusual one for such a time of the year. The treatment, some time after its first appearance, altered the type, and lessened the severity of each paroxysm, alleviating but not removing the disease. In this state of irregular intermittent, exposed, as this patient must have been, to the inclemencies of the weather and irregularities of diet, it is not too much to suppose that the constantly recurring periodical congestions, which pathology shews to be a character of this disease, at length produced a permanent congestion of the abdominal viscera. In such a state, and in such a subject, an exciting cause would not be long wanting to throw the stomach and bowels into a state of inflammation, producing the symptoms laid down in the case; the diarrhœa, the passage of mucus and blood, and the seat of the tenderness, rendering it probable that the lower intestines were chiefly engaged. Examination shewed the liver and spleen to be in a state of congestion. The headache was most likely only sympathetic.

Considering the diarrhœa in its true light, of only a symptom, and setting aside entirely the empirical use of astringents, Dr. Stokes applied his remedies to the gastro-enteritis, and the case shows how successfully; the diarrhœa not only being subdued in a very short time, but the congestion removed from the stomach and spleen. After the subsidence of the inflammation of the bowels, some symptoms of a recurrence of the ague were speedily stopped by small doses of quinine.

The effects of the stimulating articles of diet, as mentioned in the case, strongly point out the necessity of carefully withholding them in this disease.

Dysentery.—Exhibition of Strychnine.

Bernard Neil, ætat. 60, a woollen spinner, florid complexion, of temperate habits, but a great tea-drinker, was admitted on the 1st of March, 1831. Has been subject to periodical attacks of looseness in the bowels for the last thirty or forty years, generally occurring in the spring; also to attacks of asthma. Ten days since, after walking several miles, exposed to a cold wind, felt unusually fatigued, with pains and stiffness in the limbs, and next day was seized with pain and distention of the stomach. The pain gradually extended to the bowels, having the character of severe griping, worse round the navel. The abdomen was not swollen, but very tender to the touch. On going to stool, he passed nothing but mucus and blood, with tenesmus; afterwards, the stools became watery, occasionally, however, mucous, tinged with blood. He lost his appetite, and had foul taste in the mouth. Two days after the illness commenced, he took castor oil with some relief, and afterwards infusion of bark, which restored his appetite; but the looseness continued much the same. On Saturday last the tenderness of abdomen, which had been considerable, left him.

At present he passes eight or ten stools in twenty-four hours, each preceded by a griping pain in the lower bowels. The abdomen feels soft and flaccid, and is not in the least tender. No enlargement of the liver or spleen.

Some of the periodical attacks to which he is subject have been more severe than the present. He is also liable to a bowel complaint on eating fresh meat, as mutton, beef, &c. and therefore generally confines himself to the use of bacon, salt pork, &c. Pulse hard and full; tongue white; urine scanty.

After the application of leeches to the abdomen, followed by a blister and opiate enemata, had been tried with some profit, Dr. Stokes considered it a fair case for the trial of strychnine. He was accordingly put on the one-sixth of a grain in the course of the day, increased in the course of a week to half of a grain, with considerable advantage, the number of stools diminishing, and his constitution improving. He left the hospital, however, prematurely, for fear of fever.

APOTHECARIES' HALL.

Names of Gentlemen to whom the Court of Examiners granted Certificates of Qualification on Thursday, August 11, 1831:—

Isaac Brooke.
John Mavor Brown.
Joseph Chattaway.
Peter Hood.
William John Little.
Wm. Ralph Milner.
Joseph Neville.

W. Wilson, Printer, 57, Skinner-Street, London.

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Medicine and the Collateral Sciences.

SATURDAY, AUGUST 27, 1831.

PUBLIC LIBRARY

CAJEPUT OIL IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

I AM induced to forward you the two following cases, as I think they fully illustrate the efficacy of the cajeput oil in the present epidemic (cholera), and may prove an interesting postscript to the promised communication of Sir M. Tierney.

(Saturday), August 20, 1831.—Hannah ——, aged 50, housemaid, is subject to dyspepsia, but has been well until between seven and eight this evening, when she was seized suddenly with considerable abdominal pain, diarrhœa, and vomiting. I was called to her about ten, and found the vomiting severe and incessant, of a clear transparent mucus. Bowels purged five times; motions dark, fluid, and offensive; pain of abdomen intense, with much tenderness, and the muscles apparently in a state of cramp, or spasm; extreme anxiety; cold clammy perspiration; pulse very weak, about eighty. I prescribed as follows:

℞ Olei Cajeputi guttas, xl.
Magnesiæ Subcarbonatis, ℈j.
Aquæ Pur. ℥iss. M. f. haustus stat. sum.

11 o'clock.—The relief from the medicine has been decided; to use my patient's words, "to perfection." The vomiting and pain in the bowels have subsided, and she complains only of being very weak and cold.

℞ Ol. Cajeputi guttas, xl.
Magnesiæ Subcarb. ʒss.
Aquæ Puræ, ℥vj. M. f. mistura sumat 4tam partem 2dis horis.

August 21st.—This morning I find my patient free from complaint; she will continue the medicine at longer intervals. She has had an alvine evacuation during the day, of a natural colour.

Sunday, Aug. 21st.—About ten o'clock this evening I was sent for to Mrs. H. aged 35, about five months advanced in pregnancy. I found her complaining of excruciating abdominal pain, with vomiting of clear transparent mucus, and frequent purging; her pulse very weak, 75. She had taken about two table-spoonsful of brandy about half an hour previous to my visit, but this was instantly rejected by the stomach. The tenderness on pressure over the abdomen was so exquisite, and accompanied by pregnancy, that, had it not been for the experience of last night, I certainly would have bled largely. I determined, however, on giving the ol. cajeputi, in the dose of forty-five drops, combined with twenty grains of magnesia, in water. I saw my patient again at eleven. The effect of the medicine has been very beneficial; the pain had much subsided, and the stomach and bowels were quiet. I have directed fifteen drops to be taken every two hours during the night.

August 22d.—This morning I find her full of spirits, having taken three doses of the medicine.

The cajeput oil may be most gratefully administered combined with magnesia, which has the property of diffusing or causing essential oils to dissolve in water, and which, in the opinion of Annesley, has a certain degree of power itself over the stomach, in cases of cholera. I am disposed to think that the cajeput has some influence over the symptoms of the present epidemic; but that it has more than many other volatile oils (more especially peppermint), remains, I think, with Dr. Macleod, in your number of the 13th of this month, to be proved. I remember reading in Johnson's Medico-Chirurgical Review, some years ago, accounts of its very great efficacy in the treatment of the Indian cholera; but, from want of an Index to that truly valuable periodical, I cannot place my hand on the number, but so far as I can charge my memory the doses were large—about one hundred drops. So much has been said, and from such high authority (Sir M. Tierney), on the powers of the cajeput over cholera, that the drug monopolizers are at work, and its price has fully doubled within the last fortnight.

THOMAS BUSHELL.

117, Crawford-Street,
Portman-Square.

REMARKS

ON THE

GOOD EFFECTS OF CAMPHOR AS A LOCAL APPLICATION IN SOME DISEASES.

> "The effects of remedies on a disease, if accurately observed, form the most important part of its history; they are like chemical tests, frequently detecting important differences in objects which previously appeared exactly similar, &c. It appears to me that symptoms and dissections cannot settle the question, &c."—*Gooch on Peritoneal Fevers.*

To the Editor of the London Medical Gazette.

SIR,

I HAVE been accustomed for some few years back, in cases of croup, to apply externally to the throat, on flannel, a strong solution of Camphor in spirits of wine.

Sp. Vin. Rec. ℥j. Gum Camph. ℥ss.

The effect of the application has been to allay almost instantaneously the spasmodic action, and to afford convenient time for the exhibition of constitutional remedies. I saw the application give immediate relief in the case of a child, between four and five years of age, where the attacks of spasm were frequent, and so violent as to force the little sufferer, in his struggle for existence, to leap with almost supernatural strength from his bed to the ground. I have used the same application in irritation of the trachea, with decided benefit; and in one instance, by its immediate application, have rescued a patient from attacks, which had often previously threatened her with consumption in their progress. I have lately applied it over the whole abdomen with decided benefit in two cases in which, from the continued and improper use of mercurial, and purgative medicines, the gastric nerves had acquired such exquisite morbid sensibility as produced, in certain stages of the digestive process, a degree of constitutional irritation that temporarily deranged every function of the body, and led in one instance to a slight apoplectic seizure. The marked and immediate benefit its application produced in a case of puerperal fever, leads me to offer you its history somewhat in detail.

A woman, about thirty years of age, in the eighth month of her pregnancy, received a severe kick from a horse in the abdomen. A great deal of indisposition followed, but labour did not commence until the natural period: the patient was under the care of the parish midwife from the commencement of her labour until the fourth day. The pains had been constant and severe, but ineffectual; the waters were discharged, whether by accident or design I never could discover.

On my first visit, I found the os uteri dilated, so as to admit two fingers, the child's head presenting, resting on the brim of the pelvis. I advised her to remain quiet; to discontinue stimulants and exertion, both of which had been used without mercy. In twenty-four hours the child's head had made some little progress through the pelvis. As her powers did not appear to be greatly diminished, I still declined interfering. From the time I left her, her sufferings became tremendous; dreadful cramps seized her limbs on the accession of each uterine pain, checking its power and duration. Such had been the state of things for some hours. On my next visit, I found the head of the child was advanced to about the centre of the pelvis; the woman was much exhausted; pulse 120. The ergot of rye was now administered with very good effect, as, on my next visit, (the interval about three hours) I found the head of the child had so far advanced as to leave a space that would easily admit my finger between it and the perineum. In a short time the pains again decreased in strength and frequency. The exhausted and suffering state of my patient led me to determine on the immediate application of the forceps, and I extracted a child, which I had every reason to suppose had been dead about a fortnight. Nothing unusual occurred until the night of the second day subsequent to her delivery, when she had frequent vomitings, with intense pain in the abdomen, which gradually became tumid and exquisitely tender. I was not made acquainted with this change until the morning. I found her with the abdomen as distended as before delivery; great prostration of the vital and voluntary powers; pallid countenance, marked by inexpressible anxiety and suffering; pulse ranging between 130 and 140, not easily compressible; apparently choked with flatulence, which appeared occasionally to induce vomiting, that aggravated her sufferings dreadfully; no rigor had ushered in the attack. The following medicines were ordered her, and a piece of flannel, which covered the whole abdomen, loins, and part of the back, was soaked with the embrocation.

℞ Pulv. Rhei. ℈j. Aquæ. Puræ, q.s. ft. pilul. ij. stat. sumend—4ta. quaque hor rep.

℞ Potass, Carbon, ʒss. Sp. Ether Nit. ʒj. Tr. Hyos. ʒss. Tr. Amm. c. gtt. xxv. Mist. Camph. ℥j. M. ft. haust 4ta. quaque hora sumend. c. suc. limon.

℞ Camph. Gum, ℥ij. Sp. Vin. Rect. ℥iv. Tr. Opii, ℥ij. M. ft. Embroc.

In about two hours from the application of the remedies, she fell into that frightful state of nervous disorder, of which Dr. Armstrong has given us such a vivid picture in his treatise on the Puerperal Fever: he says, "the light wanderings of the mind were succeeded by a low muttering delirium, speedily followed by a stupor, in which the patients lay with their eyes half closed, and could not be roused from it but by loud speaking, upon which they started as from a disturbed sleep, uttered some vague and hasty expressions, and then sank into the same condition as before." The attendants, alarmed at this alteration, sent for me. A gentleman residing with me visited her in my absence, and abstracted about twelve ounces of blood, which was strongly buffed and cupped. She gradually recovered from this state of stupor, and he left her with the abdomen dreadfully tender, and as prominent as before delivery; the pulse ranging at 130.

I again visited her in about three hours: during this interval the embrocation had been reapplied. I found the swelling of the abdomen perfectly subsided, though it still remained exceedingly sensible to pressure; the pulse 120. She had again sunk into a state of stupor; her appearance was that of a corpse; and in addition to a variety of symptoms which Dr. Armstrong has so accurately described, her respiration was scarcely observable: no evacuation had been procured from the bowels. I directed the medicine to be continued, and ordered a purgative enema to be injected. In the course of the night the bowels were copiously relieved, the state of stupor passed away, and she slept quietly at intervals. In the morning there was no tension of the abdomen; pulse 100. The countenance had lost its anxious expression. I directed five drops of the Liq. Opii Sed. with ʒj. of Sp. Ether Nit. to be given every six hours, and continued the usual nourishment (gruel, with spice and brandy.) From this time until the present moment (a period of six days) she has continued slowly to recover

It is not my intention to lengthen this history, by detailing the progress of convalescence; it is sufficient to say, that difficulties were encountered and overcome by a mode of treatment, the object of which was to allay irritation, and to support the powers of the system; my only wish is to convey to you a faithful picture of the severity of the disease, and to point out the rapid subsidence of the abdominal swelling on the application of the camphor, and the consequent mitigation of every symptom which appeared to threaten her existence.—I am, sir,

Your obedient servant,
HENRY GEORGE.

Phillemore Place, Kensington,
August 16, 1831.

PATHOLOGY OF CONTAGIOUS DISEASES.

To the Editor of the London Medical Gazette.

Sir,

Impressed with the importance and necessity of acquiring ideas as accurate as possible in pathology, and convinced that in order to accomplish this we have much to unlearn, I am induced to submit the following suggestions to the notice of your readers.

Medical practitioners have probably always been more influenced by opinions than by facts, and adhere to the notions of talented individuals, with all the partiality of cherished prejudice. We are prone to receive the dicta of those authorities to whom we look with confidence for accurate views and appropriate modes of treatment; and if these individuals have formed and promulgated imaginative theories, destitute of the solid basis of observation, the influence they may exert on our minds, and the bias they may give to our practice may be incalculably dangerous. One professor, to whom I have often listened with delight, and whose powerful eloquence would equally have fitted him for the bar or the senate, traced all diseases to the liver, and believed all his patients to labour under hepatalgia, hepatagra, or hepatitis. His catholicon was calomel, by which he sought to remove complaints which often existed not in the body of the patient, but in the fertile imagination of the physician; and although the diagnosis he had formed was not unfrequently falsified and contradicted by postmortem examination, he adhered to his opinions to the last. Another eminent and eccentric individual, by whose death the profession has been deprived of one of its most successful cultivators, and whose talents have much contributed to elevate a neighbouring school to its present distinguished rank, considered all diseases to be occasioned by derangement of the stomach and chylopoietic viscera, an opinion which led him to adopt a most compendious method of prescribing, and an easy system of therapeutics. Without the investigation of symptoms, external or internal, the same prescription, diet, regimen, and directions, were thus deemed equally applicable to one patient or one hundred, and capable of alleviating any class or species of disease in Cullen's nosology. It cannot be disputed that the regulation of the diet, and of the alvine excretions, are essentially requisite to the successful treatment of any disease, and that solids and fluids, received into the stomach, which we measure by pounds and ounces, will be as likely to influence the health as those divided by grains or scruples; and Mr. Abernethy has, I conceive, deserved well of the profession for having more particularly called its attention to circumstances, from the neglect of which the most judicious medical treatment of internal, and probably of many external diseases also, must prove unavailing.

Ancient writers, to whom the important physiological truths, developed by experiment, were unknown, were most acutely observant of the external phenomena of disease, and possibly in some instances correct in their opinions as to its nature and origin. The utter neglect of their writings by modern practitioners is by no means a proof that they are valueless, for there may exist in the human mind a prejudice in favour of novelty, which is as likely to mislead the judgment as the blindest and most uninquiring adherence to obsolete authorities; and that pathology has not been so successfully cultivated as other branches of natural science far less important, is incontrovertible. The properties and constituent parts of the blood, and the changes which it undergoes from respiration, have been investigated and explained by our countrymen with a degree of exactness and perspicuity which entitles them to the gratitude of all who are interested in the extension of physiological discovery, and to trophies affording more permanent and real glory than those of laurelled conquerors, the spoilers and scourges of the human species, who have wasted oceans of the vital fluid.

Our acquaintance with the properties and chemical changes of the blood is not so perfect but that there remains much for future experimentalists and inquirers to discover.

I think it indisputable that it undergoes other changes equally important with those already explained, though not so obvious to our research, and not appreciable by any mode of investigation hitherto proposed. After bleeding several patients, we shall find well-marked differences in the appearance of the blood drawn; its colour, density, and specific gravity, will not be precisely similar in any two specimens and if we wait until coagulation has taken place, the difference will be still more decidedly apparent. Some of these varieties doubtless originate from the more or less perfect arterialization which it has undergone, or on the presence of inflammatory action; but these examples are only some of a multitude of changes of which it may be susceptible from having absorbed morbific agents of too subtle a nature to be discovered otherwise than by their subsequent effects on the constitution and the diseases produced by their influence; nor can we imagine that this fluid should undergo any without occasioning the most important consequences to the general system. The coagulable lymph separated when inflammation exists, is a phenomenon by which, in conjunction with other circumstances, we judge of its presence. This additional supply is probably one means by which nature arrests increased action, and preserves the constitution from the disease becoming generally diffused. During pregnancy the same circumstance obtains, and coagulable lymph is separated in greater proportion than at other times, to provide for the growth of the ovum, to secure its attachment to the uterus; and subsequent to parturition, this peculiarity must render the blood more speedily coagulable, and thus tend with greater certainty and effect to close the uterine vessels, and in conjunction with the contracted state of the uterus, to prevent the danger of hæmorrhage after the expulsion of the ovum. Besides these alterations in the blood arising from circumstances within the body, it is highly probable that it may undergo changes produced by the absorption of injurious matters from without; hence all those formidable and fatal diseases excited by contagion, so little understood, and influenced so little by medical treatment, dictated by a system of pathology, which confounds the effect with the cause, and possibly mistaking the one for the other, is often calculated not to cure the disease, but to destroy the efforts made to remove it from the system; hence also the necessity of extreme caution in the use of measures which diminish the powers by which nature seeks to secure the patient's safety; and increased action, whether general or local, may be in these instances a necessary process or an effort of nature to relieve herself of local congestion, to establish and restore discharges which may have been checked or suppressed, or a salutary and essential effort of the vis medicatrix naturæ to rid herself of something possessed of irritating and injurious qualities. Judging from the symptoms developed early in many contagious diseases, we cannot entertain a doubt that the miasma may be received into the system in a state of such extreme activity that its powers are at once depressed, that this curative re action cannot be accomplished, by which, in more sthenic diseases arising from miasmata, the morbific agent is eliminated and removed. In small-pox, scarlatina, measles, and some other forms of disease, the circumstances to which I refer are obvious to our senses, in consequence of the contagious matter being thrown out by the skin, and for its removal a certain degree of increased action is in all cases a requisite condition. In other diseases of miasmatic origin, although we are not in every instance able to discover the outlet by which the morbific agent is expelled, but which is probably the surface of the body, we have sufficient reason from analogy to believe that the same febrile excitement must necessarily take place, and that their most fatal and intractable form does not result from a high degree of inflammatory action, but that the patient may be in some instances lost for want of it. In such cases, the local abstraction of blood may possibly be in a few cases admissible, where congestion exists in important organs; but if this, or any other means of active depletion be indiscriminately employed, the event will prove that the measures adopted have effectually combated, not the patient's disease, but the actions by which nature has sought in vain to eradicate it, and to secure his recovery.

CHARLES SEVERN.

Jewin-Street, Aldersgate-Street.

ON THE

PHYSALIS PELAGICA (HOLOTHURIA PHYSALIS, LIN.)

With Observations on its Stinging Property.

BY GEORGE BENNETT, Esq.

(With an Engraving.)

THIS beautiful mollusca inhabits the seas of the tropical regions, and when first removed from the water excites the admiration of the spectators by the elegant and vivid colours with which it is adorned. These tints, however, are as evanescent as they are brilliant; and soon after this animal is captured from the sea, the crest sinks; the bright crimson and purple tints lose their brilliancy, and the beauty which had previously excited so much admiration, is lost.

The genus physalia (physalie) is thus defined by Lamarck (sur les Animaux sans Vertèbres, tom. 2d, p. 478). Corpus liberum, gelatinosum, membranosum, irregulare, ovatum, ad latera subcompressum, intus vesiculosum: dorso subcristato; ventre tentaculis variis instructo.

Tentaculi numerosi, varii, inæquales; alii filiformes interdum longissimi; alii breviores et crassiores.

Os inferum subcentrale.

Body flexible, gelatinous, membranous, irregular, ovate, a little compressed on the sides, vesicular interiorly, with a crest on the back; and numerous tentaculæ situated at the base. The tentaculæ are numerous, of different kinds, some being filiform, varying in length, others shorter and thicker. Mouth situated inferiorly and subcentral. The specie are the physalis pelagica, P tuberculosa, P. megalista, P. elongata, and are thus described by Lamarck:—

1.—*Physalis pelagica* (physalie rougeâtre.)

Phy. ovata, subtrigona, cristâ dorsali prominente subrubellâ venosâ. Inhabiting the Atlantic Ocean, Gulf of Mexico, &c.

2.—*Physalis tuberculosa* (physalie tuberculeuse.)

Phy. irregularis, ovata, obsolete cristata; extremitate anteriore tuberculis, cœruleis, seriatis, confertis. Inhabiting the Atlantic Ocean; and Lamarck observes—" elle a une rangée de tubercules d'un beau bleu à son extrémité anterieure, et sur son dos une crête aique, mais médiocre."

3.—*Physalis Megalista* (physalie bleu.)

Phy. ovata; extremitate anteriore longiore rectâ rostriformi; cristâ prominulâ plicatâ. This animal is the physalis megalista of Peron and Le Seur, (Voyage 1, plate 29, f. 1.) Inhabits the Southern Atlantic Ocean.

4.—*Physalis Elongata* (physalie allongée.)

Phy. oblonga, utrinque acuta, subhorisontalis. Inhabiting the gulf of Guinea.

These molluscæ are known among our seamen by the name of the "Portuguese men of war;" and galére, or frégate among the French, from its appearing like a small vessel, resting tranquilly on the surface of the water during a calm, at which time it is more readily discerned than during strong breezes. The species now more particularly under observation is the holothuria physalis of Linnæus, the physalis pelagica of Lamarck. The figure of the animal is somewhat ovate, and the upper portion resembles an inflated bladder, rounded at one extremity, and with a beak-like projection at the other; on the summit or back is a crest or ridge, slightly elevated, and sulcated and fringed at the edges; the whole of this part of the animal is of a light blue colour, tinged with brilliant crimson. I found the bladder to be filled solely with air. I have frequently heard it asserted that the animal has a voluntary power of collapsing the bladder on the approach of tempestuous weather; or, to use a nautical expression, "furls all sail," and then sinks to some depth. I am not aware on what authority this assertion rests; it is more probably a "seaman's tale" than the result of a naturalist's observation. On examination, no apparatus is found by which such an effect could be produced, and if it actually has such a power, why is it not exercised in every moment of peril? Why does it not, like the argonauta, collapse and sink on the approach of danger? When we approach the animal to capture it, or when it has been taken from the water, no such change takes place; the bladder still remains inflated, and can also be preserved in that state; and during a strong breeze, I have seen them floating on the waves, but from the ship passing at that time rapidly through the water, they are then more rarely observed. I have also seen them thrown in tempestuous weather on the beach at New South Wales, the bladders remaining always inflated. From these, and other reasons which might be adduced, the assertion cannot be considered as the result of actual observation. The inflated membrane is probably intended to keep the animal buoyant on the water, by which it is readily enabled to extend its long tentaculæ in search of prey, or it may be designed as a locomotive agent, aid-ing the animal in its progress over the "vast bosom of the ocean;" thus serving the purpose of a sail. It is said that the appearance of the physalia near the sea-coast is the indication of an approaching tempest.

Respecting this genus, Lamarck observes, that "la forme irreguliere, la crête dorsale, et les tentacules très long et pendans qu'elle a sous le ventre, la distinguent éminemment des velleles. Par cette même crête et par son interieur vesiculeux elle diffère de toutes les medusaires connues*." Situated at the under part of the animal is a mass of tentaculæ, some long and filiform, others short and thick, each apparently consisting of a chain of globules, filled with an extremely acrid fluid; in colour they are of a beautiful purple, with an admixture of crimson, and they are covered by a glutinous substance, having a peculiar odour. Lamarck observes, that "la bouche des physalies est inferiure, sans être tout-à-fait centrale. Les tentacules qui l'avoisinent ou l'environnent, et qui consequemment sont situés et pendans sous le ventre de l'animal sont nombreux, tres inégaux, et de diverses sortes. Les uns sont plus courts, plus epais, et paraissent terminés en suçoirs; les autres sont fort longs, filiformes, commes ponctués par la diversité de leurs couleurs locales; car ils sont vivement colorés de differentes manières, et il y en a de rouges, de violets, et d'un très beau bleu. Leur crête dorsale est aussi très vivement et agréablement variée dans ses couleurs."

I caught a fine specimen of this mollusca in latitude 9° 0' South, and longitude, 12° 59' West. Being aware of the sting existing in the tentaculæ, I was desirous of trying its effects on myself, for the purpose of ascertaining from personal experience the constitutional irritative effects resulting from it. On taking hold of the animal, it raised its tentaculæ and stung me on the second and ring fingers. The sensation was similar at first to that produced by the nettle; but before a few minutes had elapsed, a violent aching pain succeeded, affecting more severely the joints of the fingers, the stinging sensation at the same time continuing at the part first touched. On cold water being applied, with the intention of removing or lessening the pain, it was found rather to increase than diminish the painful effects. The irritation resulting from the poisonous fluid emitted by the animal extended upwards, increasing in extent and severity (apparently acting along the course of the nerves), and in the space of a quarter of an hour, the pain in the forearm (which was more particularly referable to the inner part) was very violent, and at the elbow-joint it was still more so. It may be worthy of remark, that when the joints became affected the pain always increased. The pain became at last almost unbearable, and was much increased on the affected arm being moved; the pulse of that arm was also much accelerated, and an unnatural heat was felt over its whole surface. The pain extended to the shoulder-joint; and on the pectoral muscle becoming attacked by the same painful sensation, an oppression of breathing was occasioned, which we find produced in a similar manner by rheumatism, when it attacks that muscle; and it proved very distressing during the time it remained. The continuance of the pain was very severe for nearly half an hour, after which it gradually abated, but the after effects was felt during the remainder of the day in a slight degree of numbness and increased temperature of the arm. On this offensive power of the animal, Lamarck observes, that "lorsqu'on touche ou que l'on prend un de ces animaux avec le main, il repand une humeur si subtile, si pénétrante, et en même temps si veneneuse ou si caustique qu'elle cause aussistôt une chaleur extraordinaire, une demangeaison et même une douleur cuisante, qui dure assez long temps*." About two hours after I had been stung, I perceived that a vesicle had arisen on the spot; and when children have been stung, I observed that numerous small vesicles arose, similar to those produced by the nettle. The intensity of the effects depend on the size and consequent power of the animal; and after it has been for some time removed from the water, it is found that the stinging property has diminished. This irritative property, unattended, however, by any of the constitutional effects, remains for a long time in the tentaculæ, even after they have been removed from the animal, for in touching a handkerchief some weeks after it had been used in wiping off some of the portions of the tentaculæ, the stinging property was found to have remained, although it had lost that virulent quality which produced on a recent application such violent constitutional irritation. Dr. Clarke Abel, when off the great Lemna island, experienced an attack from a species of physalia which (although from his description it was of very diminutive size) produced severe symptoms, he thus mentions the circumstances. "Whilst employed in collecting some sea-weed floating about the ship, I observed a species of physalia, so small and transparent, that I at first mistook it for an air-bubble; but on catching it in my hand, was soon convinced of my error, for wrapping its long tendrils round one of my fingers, it stung like a nettle, but with much more severe effect. In about five minutes, the pain in my finger abated, but an uneasy sensation extended up the inside of the arm, which soon terminated in an aching pain in the armpit, accompanied by a sense of restriction within my chest: within fifteen minutes all uneasiness ceased. The manner in which the animal produces these effects is, I believe, unexplained; but it is not improbable that they are occasioned by a peculiar poison, secreted by it, and contained in a glutinous matter, which covers its tendrils; as this, when applied to the skin, apart from the animal, excites a smarting pain*." This poisonous quality does not, however, exist solely in this species of mollusca; several of the medusæ have similar properties, which *may*, perhaps, be considered as both offensive and defensive; and it has been, and no doubt correctly, supposed to be given to these animals as a means of procuring their food, the benumbing principle existing in the tentaculæ rendering their prey when touched unable to escape. For what purpose this offensive, or perhaps defensive property, exists in the vegetable kingdom, it is difficult to decide, and all that has yet been said on the subject may be considered as merely hypothetical: for instance, at the Island of Singapore there is a remarkable species of the order Fuci, usually found growing in isolated patches upon coral banks. Finlayson thus mentions it. It is pinnated, plumose, elegant, about a foot and a half in length, and of a whitish colour. It is endued with a property of stinging like nettles; the sensation produced is more acute and more penetrating, more instantaneous, but somewhat less permanent. The hand is scarcely brought into contact with it, before the wound is inflicted. A small, corrugated, granular bag, filled with a transparent fluid, would seem to be the organ by which it produces this effect. These are no sooner touched than they discharge the fluid they contain. The plant soon loses this power after being removed from the water." This plant seems, therefore, to possess an offensive or defensive property, analagous to that of the physalia, but for what purpose it is difficult to form an opinion.

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* Sur les Animaux sans vertebres. Tom. ii. page 480.

* Sur les Animaux sans vertebres. Tom. ii. page 480.

The usual method adopted for the preservation of this curious and beautiful mollusca, described in this paper, is by placing it in spirits; the form is thus well preserved; but its beautiful colours, the subject of so much admiration, are totally lost. Several fine preparations in spirits are in the museum of the Royal College of Surgeons in London As in the case of the beautiful but evanescent colour of flowers, no method has been discovered by which their natural brilliancy can be preserved, and it is impossible to retain that peculiar brightness given only by life and health. I have lately preserved the physalia in the following manner*: I at first detached the whole of the tentaculæ from the membranous portion of the animal, on account of their being too soft and perishable to enable them to be dried *en masse* with any chance of success, their *form* only being preserved well in spirits; the membranous, or inflated portion of the animal is then punctured, by which the air contained in it escapes; being then pressed between sheets of paper, (which are to be renewed as often as required until it is dry) the form of the upper portion of the animal (making a lateral view) is well preserved, and some faint portion of the colours remain. This being gummed on paper, resembles a drawing, and the original colours being imitated by the pencil, and the tentacula, drawn and coloured, the *tout ensemble* conveys an idea of the brilliant appearance of the animal as far as can be produced by artificial colours. The annexed drawing was made from one prepared after the manner just described. It is, indeed, only by repeated trials that

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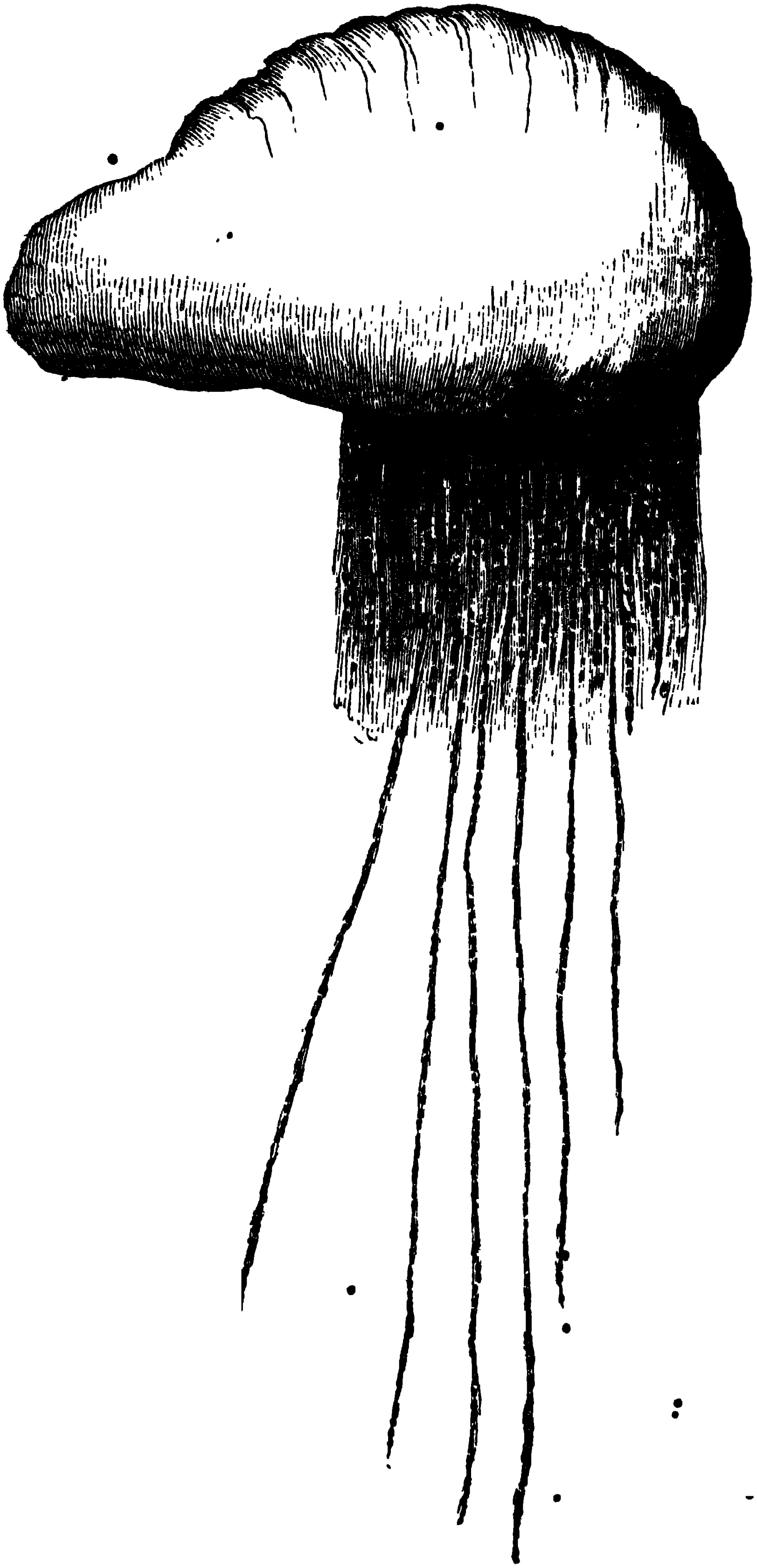
* Abel's Narrative of a Journey in the Interior of China, and of a Voyage to and from that country. 4to. page 59.

* A specimen preserved after this manner was laid on the table of the library of the Royal Institution during the evening conversazione of June 10, 1831.



the best and most accurate methods of preserving objects of natural history can be discovered, the greatest and sole difficulty existing, being that of preserving them accurately in their natural appearance.

London, August 10, 1831.

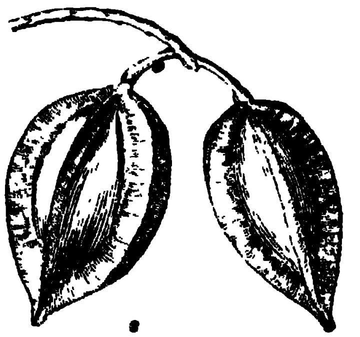
OBSERVATIONS
ON THE
SEEDS OF A SHRUB OF THE NATURAL FAMILY *COMBRETACEA*,
(*Probably a Combretum or Quisqualis,*)
POSSESSING VERMIFUGE PROPERTIES.

By Geo. Bennett, Esq.

Corr. Mem. of the Medico-Botanical Society of London, &c. &c.

As observations on any new production of the vegetable kingdom possessing medicinal properties cannot be uninteresting to the medical profession, I am induced, under this impression, to give publicity to the following brief remarks on a shrub, a native of the Philippine Islands, the seeds of which have vermifuge properties. It is not, however, "transient visitors," anxious as they may be, or whatever enthusiasm they may evince in scientific pursuits, by whom extensive information can be collected, respecting the actual value of the reputed properties (whether medicinal or otherwise) of certain plants. It is from those individuals (more particularly of the medical profession) resident abroad, that we ought to look for new discoveries, as well as for the confirmation, by repeated trials, of the advantages to be derived from those, the properties of which are but as yet imperfectly known, and also to ascertain whether their medicinal powers are such as to render them a valuable addition to our materia medica. Endeavours should also be made, if the seed or plant is the growth of a distant clime, of discovering a method of preserving to countries far remote the seeds, or any other portion of the plant in which the active principle is resident; for, doubtless, many worthy of high consideration have been too hastily rejected, as not possessing the valuable properties attributed to them, solely from the circumstance of the active principle having from time or other causes become inert. In the native country of the plants the best opportunity also exists of ascertaining the full effect of their medicinal or other properties from recently gathered seeds or leaves, attention being directed also towards the season during which the latter more particularly ought to be collected. It is from our colonial surgeons that we ought to expect information to be elicited, not only of the virtues assigned to the various medicinal plants scattered over our extensive foreign possessions, but in other branches of natural history; and it is much to be *regretted that so very few devote a leisure hour to those objects, which, by a* numerous collection of facts, would confer a lasting benefit to science, and prove a frequent source of recreation to themselves.

It was to me a subject of regret, that I was unable to ascertain, by actual inspection, or procure specimens of the shrub, yielding the seeds now under observation, which, during my visit to Manilla, (during the months of July, August, and September,) was unfortunately dried up. It was described to me as being a shrub, attaining the height of from four to five feet, a native of the island of Luçonia, and others of the Philippine group. It is to be found in flower and fruit during the months of January, February, March, and April: during the two latter months the fruit is to be found in a mature state. The remaining months of the year the shrub is dried up. From the fruit (of which the following cut shows the form) alone an opinion can be formed of its family and probable genera, and from these it is considered to be of the natural family combretacea; probably a combretum or quisqualis, and from the description of the shrub, it may more readily, perhaps, be placed to the former. The medicinal property it possesses is that



of a drastic purgative, acting as a powerful vermifuge for children, but seldom or never used for adults, on account of the large quantity requisite to produce sufficient beneficial effects. The shrub and seeds bear the appellation of "liñugans," in the Tagalo, or native language; and "piñones," in the Spanish. They are administered by the outer shell being broken, the kernel extracted, and then given to the patient to be eaten; and as the flavour is not unpleasant, children make no objection to them. The dose for a child five years of age is from four to six in number (without any regard to weight,) which is increased or diminished according to the age of the patient: if the first dose has not the desired effect, it is to be repeated on the following morning. It was mentioned as being beneficial to give the child some tea to drink after taking the seeds, but this is probably serviceable only in hastening its effects. An over-dose will produce violent hiccoughs (*hipo* of the Spaniards,) and sometimes, but rarely, terminates fatally. Cold water is the antidote generally administered. Don — Caldez, a very intelligent Spanish gentleman resident at Manilla, informed me, that they had several other vermifuge medicines, but none that equalled in efficacy these seeds; he judged from the beneficial effects resulting from cases occurring in his own family: his statement was also corroborated by both Spaniards and natives, who highly extolled their medicinal properties. These seeds are sold abundantly in the bazaars at Manilla and Cavité, at a very cheap rate. Considering that if these seeds were found on a farther trial possessed of the medicinal properties attributed to them, they would be valuable in this country, I brought to England several packets, which have fortunately arrived in good order, and my endeavours will be now directed to the raising of the shrub in this country, both to ascertain the probable genus to which it belongs, as also to have a better idea of its vermifuge powers from recent, rather than from seeds which must, from time, in some degree have lost their power. I have presented seeds, therefore, for this purpose to Kew Gardens, the Botanical Garden of the Apothecaries' Company at Chelsea, Messrs. Loddiges at Hackney, &c.; and at the meeting of the Medico-Botanical Society of London, on the 14th of June last, I had the honour of presenting a packet of the seeds, with some brief observations. The establishment of this society, if its extensive and valuable objects are well supported, will materially increase our knowledge of medicinal plants, and render their properties better known; and we may hope that the time will arrive when a botanical garden will be added to it for the cultivation solely of medicinal plants, though this desideratum is now in some measure supplied by the laudable exertions of the Company of Apothecaries.

London, August 10th, 1831.

MEDICAL JURISPRUDENCE.

To the Editor of the London Medical Gazette.

Sir,

Having perused your number of August 13th, I perceived a statement made by Dr. Venables, of Chelmsford (in his case of poisoning with corrosive sublimate) that there is no known medicine which, by its internal administration, can produce the miscarriage of a pregnant woman.

I should not readily doubt what such a clever man as Dr. V. would assert, and should be very glad if his assertion was true. I hope to see the mistake (if any) rectified in a future number, but (if true) the fact confirmed.

Query. What were the circumstances which led to the late discovery of a valuable obstetric medicine in America?

I am, sir, yours, &c.
T. H. H.

CAJEPUT OIL IN CHOLERA.

To the Editor of the London Medical Gazette.

46, Dover-Street, 23d August, 1831.

Sir,

According to promise, I send you the case of cholera, referred to in your journal last week.

I have the honour to be, sir,
Your obedient servant,
M. J. Tierney.

A lady, æt. 28, felt indisposed at half-past nine, P. M. on Wednesday, the

10th instant: she had been in good health throughout the day, and dined, as usual, at eight o'clock. At a little *before ten vomited the contents of the stomach, reported to be merely the food taken at dinner:* the bowels were moved shortly after. At half-past ten the vomiting and purging again took place, and she felt "very uncomfortable." Continued occasionally purged and sick at the stomach till a little before one, when she fainted, and remained insensible for about ten minutes: on recovering, she was seized with violent spasms in the lower extremities, more particularly in the feet, the toes being remarkably affected. The nausea and vomiting again distressing.

On my visiting this lady (for the first time since the invasion of the symptoms,) at a quarter before two, A. M. on Thursday, the 11th, I found her in a profuse perspiration, with a death-like coldness of the extremities; the pulse at the wrist scarcely perceptible; insatiable thirst; countenance expressive of great anxiety, with a remarkable shrinking of the features; and extreme restlessness: the mind perfectly clear. She said, "I believe that I have got the cholera. I took twenty-five drops of the cajeput oil about half an hour ago, and, in a few minutes after, fifty drops more: it has done me good; pray let me have another dose." I assented, and, fearing that what she had already taken might not have been genuine, I sent for some which I had received from a friend lately arrived from India: in the meantime, she took three tea-spoonfuls of brandy, in a little water, which was repeated in five minutes. The body and limbs having been well rubbed with hot, dry flannels, were wrapped up in the same: this was attended with considerable difficulty, from the great restlessness and jactitation.

At two o'clock, A. M. I gave forty drops of the oil, in half a wine-glass of warm water: this at once quieted the stomach, and in half an hour the spasms were somewhat relieved: the pulse became more perceptible, and she said that she felt better; but the thirst continued unabated, and she called for iced water, of which she was permitted to take half a wine-glass repeatedly, with the addition of a small quantity of brandy, and a little sugar.

At half-past three the extremities became quite warm, indeed they were rather above the natural temperature; but the restlessness was at this time excessive, and a stool was passed, consisting of about six ounces of *fluid resembling thick rice-water.* Plain water, soda-water, lemon peel water, iced, with a little brandy, occasionally given. The feeling of weakness excessive: there was a disposition to sleep, but this was interrupted by extreme thirst: the stomach and bowels now quiet. At five o'clock she anxiously requested a saline effervescing draught, which was given, but immediately rejected by the stomach: the pulse became more languid; another dejection, similar to the last; hiccup; the spasms increased in violence, and she complained of excruciating pain across the loins. Took ten drops of laudanum, with ten grains of Epsom salts, in a little peppermint water, but this was soon rejected, and not followed by any alleviation of the symptoms.

At six o'clock, A.M. I had the pleasure of having Dr. Holland associated with me. A blister was directed to be applied to the epigastric region; a draught ordered, containing a small quantity of Epsom salts, in a little peppermint water, which was immediately rejected by the stomach; the small quantity of brandy in iced water, soda water, &c. directed to be given occasionally.

The thirst continuing unabated, the patient was permitted to have small bits of ice in her mouth, which gave great comfort. During all this time *no urine was passed;* throughout there was no pain in the stomach or bowels.

The extremities becoming again cold, and the pulse giving way, it was agreed, at eight o'clock, to give forty drops of the cajeput oil. Great relief followed the exhibition of this dose; within an hour, however, the violent spasms in the muscles of the legs and feet returned, but they lasted only for a short time; she then became composed, and soon after had a short refreshing sleep.

At twelve o'clock great improvement; and at two, P.M. (the stomach and bowels having remained quiet) she took two grains of calomel, with three of the compound extract of colocynth. At three o'clock the patient took a small breakfast cup of mulagatawney soup; and had afterwards refreshing sleep for an hour and a half; at five took a wine-glass of sherry, and slept again for an hour; at seven took another cup of the soup, and afterwards slept till half-past eight, when a dark, scanty stool passed; and for the first time since the attack, some urine; at half-past nine another scanty motion, and soon after a greater quantity of urine, of a pale colour; at ten o'clock, P.M. she took four grains of the extract of colocynth, and she was removed to a sofa while her bed was being made; at half-past ten, after taking a little more soup, she fell asleep.

Friday 12th, nine, A.M.—I found the patient asleep; her maid reported that she had passed a good night, and slept comfortably; urine passed freely; at noon met Dr. Holland; we found our patient free from complaint, having taken some of the soup for her breakfast. It is to be noted, that throughout this attack the tongue was clean and moist, although the thirst at times was intolerable. In the course of this day she was moved from her bed to a sofa; at nine in the evening, the bowels not having been moved, eight grains of the extract of colocynth were taken.

Saturday, 13th, morning.—Found that the patient had been disturbed by the pills during the night, and had had loose bilious motions.

Sent for at five, P.M. in consequence of her suffering occasional pain in the stomach and bowels, with vomiting. A draught, consisting of camphor, julep, and opiate confection, was ordered, but this was immediately rejected by the stomach. The pain and sickness continuing, thirty drops of the cajeput oil were administered, and soon after the pain and sickness subsided. The cajeput oil was always very grateful to the stomach. The lady has continued well. No other case of cholera has occurred in the family.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

A Treatise on the Means of Preserving Health, and particularly the Prevention of Organic Diseases. By A. P. W. PHILIP, M.D. F.R.S. L. and E. &c.

THE learned and elaborate paper of Dr. W. Philip, in our last number, has induced us to direct more particular attention than we had previously done to his work on Organic Diseases, published a few months ago. In bringing it under the notice of our readers, we shall first present them with a faithful analysis, (which, if somewhat extended, is not more so than the importance of the subject requires,) and then offer a few remarks upon the questions discussed.

The author begins by informing us, that it is not his intention to present to the reader a regular treatise on the diseases he is about to consider, but to state the result of his own experience, and apply the investigations which have long occupied a considerable portion of his time, to improve our knowledge of their nature and treatment.

"I believe," he observes, "a physician who has been long engaged in practice cannot better promote the objects of his profession than by simply relating with accuracy the facts he has himself observed, and the reflections they have suggested."

The work is divided into two parts: in the first, the nature and symptoms of the states which precede the establishment of organic disease, is considered; in the second, the means of preventing these states, and counteracting their effects.

With the exception of simple fever, and a state of inanition and plethora, he considers all diseases as depending on local affections. For the phenomena and treatment of the various species of fever, he refers to his treatise on febrile diseases.

The state of inanition he considers as always temporary, except it be supported by some local disease. It is only where due nourishment has been withheld, or the patient has suffered from previous disease, that we find the quantity of circulating fluids deficient, independently of a failure of function in some of the organs of digestion and assimilation; and in these cases, provided no such failure supervenes, their due proportion is soon restored. Plethora the author regards as a state of much greater importance, and as laying the foundation of many diseases. He treats of the manner in which it disposes to and modifies the symptoms of inflammation; but he regards congestion, that is, morbid distention of the larger vessels of particular parts, as the class of diseases most intimately connected with, and consequently most apt to arise from a state of plethora. The author made many experiments for the purpose of ascertaining the state of the vessels in an inflamed part, the results of which have been confirmed by others. An account of these experiments was published in the introduction to his treatise on symptomatic fevers. From thence it appears that every cause debilitating the capillary vessels of a part is capable of inducing inflammation, these vessels, in consequence of the debility, suffering themselves to be morbidly distended by the vis-a-tergo. In this state of these vessels, and the consequent increased action of the larger vessels of the part, and, in inflammations of importance, of the whole system, inflammation consists.

In a state of plethora, where the whole circulating system is more or less in a distended and debilitated state, such partial failure of power in the capillary vessels is most apt to take place; but from the diminished power of the vis-a-tergo, the symptoms of excitement consequent on the debility of the capillaries, will not rise so high as in more vigorous states of the habit. Hence the plethoric are liable to inflammatory attacks, but in them inflammation is more languid. It is thus, the author observes, rendered more insidious—more apt to run to its unfavourable terminations without the usual warnings. But experience tells us that in plethoric habits the larger vessels of the part are still more liable to an increase of debility than the capillaries, giving rise to a congested state, which impairs the function of the part, without producing the symptoms characteristic of inflammation.

The author points out the reasons why a plethoric state of the habit does not always betray itself by external signs, and why when it does so shew itself, the risk is not always proportioned to its external indications.

It seems at first view surprising that the state of the external may not always be regarded as an indication of that of the internal vessels, all parts of the circulating system freely communicating. In a mere system of inanimate tubes it would be so, but we cannot thus reason respecting the vessels of the living animal.

"Although," the author observes, "the power of the heart and blood-vessels, we shall find, is independent of the nervous system, it is throughout the whole frame capable of being influenced by it. By it the animal body is formed into a whole, every part of which is under the control of the central parts of that system where all its powers, if we except merely the powers of conveying its influence, reside; and the cause of deviations from the healthy state often so affect this system that their influence is felt most in certain parts, and consequently the vessels of those parts are most affected. Now when the causes of plethora produce too great fulness of the vessels, it is evident that if the action of any particular set, from some peculiarity in the exciting causes, or in the habit of the patient, be more debilitated than the rest, these will be most distended, and their preternatural distention will tend to relieve the others. Thus the morbid distention sometimes takes place chiefly in the external, and sometimes in the internal vessels. When we consider local affections, we shall find that even with respect to particular parts of the body, this fact is clearly demonstrated. Nay, we have sufficient proof that a weakness of particular sets of vessels, is often the sole cause of the partial plethora which attends it; for although the quantity of blood on the whole may be no greater, or even much less, than it should be, if any set of vessels be debilitated, they will yield most to the general force of the circulation, and thus receive more blood than their due porportion."

A plethoric state the author considers as not merely disposing to, and modifying disease, by the general distention of the vessels, but also by the change which necessarily takes place in the blood itself, from the languor of the secreting and other assimilating processes, which necessarily attends it; and particularly from the effect produced on every part of the body through the impaired power of the nervous system. This leads him to consider the relation which that system bears to the circulating system. Of this part of the work he himself gives an ample analysis in his reply to Dr. Prout, which appeared in the last number of this Journal; and the chapter concludes with some observations relating to the manner in which functional are changed to structural diseases; and the necessity of keeping in view the relation which the different parts of the animal frame bear to each other, in order to understand the manner in which the foundation of the latter set of diseases is laid, and their progress influenced.

He then proceeds to consider the diseases of individual organs, beginning with the brain as the most important. The third and fourth chapters relate to the diseases of this organ, which tend to affect its structure; the third to the acute, the fourth to the chronic diseases having this tendency. The author's object in the first of these chapters is chiefly to prepare the reader for what is said in the other, and consisting in the application of the results of his experiments to explain the phenomena of the acute diseases of the brain, does not admit of being much abridged. We shall, therefore, extract from it as a specimen of the manner in which the application is made, what is said of concussion of the brain, which the author classes with nervous apoplexy.

"Thus nervous apoplexy," he observes, "is either the effect of a sudden and excessive application of stimulants, which often produces instant death, and always threatens it, or of the slower operation of sedatives—namely, of those causes which debilitate the action of the brain, and through it of the nervous system. We have a striking instance of the effects of the former cause, in what surgeons call concussion of the brain—that is, the effects of a blow on the head, which so shapes the brain as to endanger the immediate failure of the powers of life.

It has just been observed, that in the experiments above referred to, a blow, which instantly and wholly deranges the mechanism of the brain, was found at the same moment to deprive the heart and blood-vessels of their power. A case of concussion is in its first stage only a less degree of the same state.

It will place what has been said in a clearer point of view cursorily to enumerate the effects of such an impression made on the nervous system as suddenly lessens the power of the heart and vessels. They will be found very accurately to correspond with the best accounts given by surgeons of concussion of the brain*, a disease whose nature has been considered obscure; nor is it possible to understand it without being aware of the immediate influence of the brain on the heart and blood-vessels. Where the brain is by any case powerfully and suddenly impressed, but not sufficiently so wholly to destroy its mechanism, it debilitates without destroying the various functions.

The sensibility is impaired; the heart acts more frequently and feebly, and for the most part irregularly, and the circulating system suffers a similar loss of power in every part of the body. The sphincters of the rectum and the bladder do not merely cease to be excited by any voluntary effort, as in sanguineous apoplexy, but have the power on which the degree of contraction, constituting their state of rest, depends, more or less impaired; so that the contents of these cavities often escape.

This state is succeeded by some improvement in the symptoms. The heart and blood-vessels in some decree recover from the shock they received. The former begins to beat with less frequency, and with more force and regularity, and the latter to convey the blood with greater velocity, and in a more uniform stream. In proportion as this change takes place, the various functions, as I have very frequently observed in animals, improve; a greater degree of sensibility returning.

If the offending cause be comparatively slight, the symptoms continue to improve; if severe, the heart soon begins again to beat more languidly, and with it all the functions gradually fail. This second failure is always final.

If the injury done to the nervous system be of such a nature as particularly to debilitate the capillary vessels of the injured part during that interval in which the vigour of the circulation is in some degree restored, the vessels of this part yield to the force of the blood, and the symptoms of inflammation are thus added to those more immediately arising from the injury.

The reader will perceive, from the foregoing view of the subject, that the nervous is a much more complicated disease than the sanguineous apoplexy. In the latter, although the powers of the nervous system are impaired, those of the sanguiferous system are, in the commencement of the disease, entire, and only become affected through the failure of respiration and the assimilating processes. In nervous apoplexy, not only the powers of circulation suffer directly from the injury done to the nervous system, thus producing a combination of diseased states of both systems, but the debility of the heart and blood-vessels has a secondary effect on the nervous system itself. The action of the brain and spinal marrow fail from defective circulation, and a state of these organs analogous to that which takes place in fainting, is superadded to that produced by the cause of the disease. Hence the immediate danger in this form of apoplexy.

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* See the third part of Mr. Abernethy's Surgical and Physiological Essays, and other works on concussion of the brain."

From the whole that has been said of the more acute diseases of the brain, we may clearly perceive the general laws which regulate them; and although those with slower progress appear with less marked symptoms, we may still observe the same tendencies in them. They are still such as lead to derangement of its mechanism, or morbid distention of its vessels."

The author considers the affection of the brain, and its tendency to organic disease, from severe and long-continued stomach complaints and other causes of nervous irritation, as of a nature similar to other symptomatic diseases — such as dyspeptic phthisis, which arises from a long continuance of what are called bilious complaints; but the difficulty of the diagnosis with respect to the brain is greater, because the affection of this organ produces general, not local, symptoms, as happens in the affections of all other organs.

The brain is not only the organ of perception, but the source of nervous power. While it conspires with the spinal marrow in maintaining the secreting and assimilating processes, and occasionally influencing the powers of circulation, it is at once the seat of the sensorial functions and the source of the powers on which they depend. It is not necessary, therefore, that headache, giddiness, or other local symptom, should attend, in order to indicate an affection of the brain. This may be indicated by no other symptom than disorder and failure in the general functions of the system, all of which depend on the state of this organ. Thus it is that the more serious affections of the brain, in their early stages, often assuming a form little different from that of common nervous complaints, the difficulty of distinguishing the serious from the trivial disease is often very great, at the only period at which there is a possibility of affording relief. This diagnosis is the subject of the fourth chapter.

The author conceives that we can only distinguish the more serious tendency of the nervous symptoms by an attention to the whole of the circumstances of the case, which he arranges under the following heads. The habit of the patient; the nature both of the predisposing and exciting causes; the general course of the symptoms; the changes produced by the continuance of the disease, particularly the patient's complexion and general appearance, and even the expression of his countenance.

Our space does not allow us to follow the author in his observations on each of these heads, but the general conclusions arrived at are, that the less irritable and variable the nature of the patient's habit, the more is to be feared from the continuance of the nervous symptoms; that more is to be apprehended from the permanence of the nature of the predisposing and exciting causes, than from the degree of their immediate effects; and more from the degree in which the symptoms are uniform and obstinate, than from the severity of the symptoms themselves; — and we are to judge of the danger rather by the effect produced, on the whole, on the patient's appearance and habits, than from the occasional consequences of the more severe attacks. Any thing like a permanent feverish tendency, in particular, is unfavourable; and the same may be said of permanent debility and a tendency to chilliness; and any degree of that expression of countenance which experience teaches the physician to associate with organic disease, is particularly unfavourable.

"From all that has been said," the author observes, "of the circumstances which indicate a disposition to organic disease of the brain in what is called nervous complaints, we infer that this termination is most to be feared in scrofulous and debilitated habits, when there is not such derangement in the digestive or any other set of organs as accounts for the severity of the nervous symptoms, and the patient is not of a variable and hysterical disposition; when the occasional causes have been of a serious and permanent nature, and the nervous symptoms have not shewn themselves for some time after their application; when the symptoms both of mind and body are less variable than usual in nervous complaints, and the latter, particularly, apt to affect the same parts of the body; when there is constantly a more or less general tendency to derangement in the secreting system; when the heart is more irritable and the lungs less free, the nervous symptoms do not yield so readily as usual; the sensations less various, and the depression of spirits more uniform; the light pulse and tendency to increased heat, or, on the other hand, the sense of chilliness and debility more constant; when the constitution seems more affected than usual by the continuance of the disease, the strength and flesh on the whole wasting, and, particularly, where the countenance assumes a sallow colour and an habitually irritable and anxious expression; — when these or several of these circumstances are well marked in what are called nervous complaints, I have been assured, by repeated observation, that they are not to be safely disregarded."

The author observes, he has dealt the longer on this diagnosis, because all long continued, and particularly all organic disease of vital parts, as appears from all he has had occasion to say relating to the part of the subject before us, and from all the evidence afforded by dissection, tends to produce the chronic disease of the brain, the diagnosis of which is here pointed out, and their phenomena cannot be understood without keeping this tendency in view.

The affection of the brain we are here considering, if it cannot be arrested, terminates fatally, by gradually exhausting the strength; or organic disease being established in the brain, produces a similar affection elsewhere, and the patient sinks under more than one disease. The lungs are the most frequent seat of this secondary disease. After being reduced to a state of great debility, the patient begins to cough, to which he had no previous disposition, and the worst symptoms of pulmonary consumption rapidly shew themselves. The author repeatedly calls the reader's attention to the tendency of disease of the brain to produce corresponding disease in other parts of the body, which he regards as more than any other circumstance influencing the course of protracted diseases; nor need this surprise us, when we see organic disease of the lungs established in a few hours when they are deprived of the influence of this organ. In cases of long-continued nervous irritation, terminating in fatal organic disease of the brain, the author has seen on dissection almost every organ of the body more or less organically diseased.

Affections of the spinal marrow, which have a similar, though less powerful influence than the brain, on every part of the system, the author considers as rather belonging to the province of the surgeon than physician.

In the fifth chapter he treats of the appearances on dissection, in organic diseases of the brain, and points out the causes of their affording less assistance to the physician than those of other organs. This difference he ascribes to the great variety of its functions; the delicacy of its organization, which may be sufficiently deranged to produce the most serious symptoms, without leaving traces to be discovered by dissection; and the particular circumstances in which this organ is placed.

The chapter closes with the following observation:—"Nothing more strikingly shews the difficulty of associating the morbid appearances of the brain with the symptoms, than the fact, that it is only necessary that the same cause of injury, even acting on the same part of the brain, should make its impression more or less suddenly, in order that it shall in one case excite to morbid activity the whole circulating system, or so reduce its powers that it is hardly capable of its function."

The morbid affections of the heart, as the organ next in importance to the brain, are next considered. These organs, the author observes, are strikingly contrasted, both in their structure and their functions. Of all the vital organs, the one is the most complicated, the other the most simple. So numerous are the functions of the former, that it is difficult to assign their limits; the heart has but one function, that of propelling the blood—a function, however, on which every other depends.

The diseases of the heart, like its structure, are simple. The author divides them into two classes; those which impair the power with which it propels the blood, and those which impede the passage of the blood through it. The former the diseases of the heart itself; the latter of its valves.

As the chief object of the treatise is

to lay down rules for preventing disease of function changing into disease of structure, and the simple organic diseases of the heart seldom shew themselves till they have arrived at such a stage as defies our art, there would have been little advantage in dwelling on them in such a treatise; but the heart, like other organs, is liable to other diseases, which may terminate in disease of structure. These are the proper subjects of such a treatise. It is liable to inflammation and irregular action from other causes; and here, as in other cases, the continuance of deranged function often terminates in derangement of structure.

The author enters fully into the means of distinguishing inflammation of this organ from that of the lungs, in which there is often great difficulty. The seat of the pain and other uneasiness, and the difficulty of breathing, for reasons pointed out by the author, but which we have not room to enter on, afford no certain diagnosis. From the state of the circulation we should, *à priori*, expect to find such a diagnosis of carditis; and in this disease the pulse is apt to be irregular and fluttering, and a tendency to, or actual syncope, to take place. But the last is not a frequent symptom, nor is the irregular and fluttering pulse a constant one; and in inflammation of the lungs alone, the pulse in different cases is variously affected. When, however, the latter symptoms are more strongly marked than usual, we have reason to believe that the heart is inflamed; of which there can be no doubt if the attack in the chest has been preceded by rheumatic pains of the limbs. The author observes, that when these pains precede inflammatory affections of the heart, he has found that the patient had laboured under hepatic affection.

There are few phenomena of disease more curious, and at first view more unaccountable, than the well-known fact, that inflammation of the heart is apt to supervene on rheumatic pains of the limbs. The author thinks that this only happens where the pains are connected with an obstructed state of the digestive organs, of which they are sympathetic. When in such cases the causes of pulmonary inflammation are applied, the great sympathy which exists between the stomach and heart often determines the effect of these causes to the latter organ; and, as constantly happens in sympathetic affections, the new affection relieves the former one; and thus, on the supervention of the cough and difficulty of breathing, the rheumatic pains disappear.

Irregular action of the heart may proceed from sympathy with other organs, or increased irritability of the heart itself; and as it is one of the most frequent symptoms of organic disease of this organ, it is of great consequence to determine from what cause it arises. The author enters fully into this part of the subject, in which our limits prevent our following him. He considers a uniform inability of bearing strong exercise, or any other cause quickening the motion of the blood, as the most unequivocal symptom of organic disease of the heart; although even this he has known the effect of sympathy with the state of the digestive organs; but, combined with the sallowness and anxiety of countenance which indicates organic disease, it may generally be regarded as decisive, when accompanied with other symptoms of disordered action of the heart. The author has seen many cases of irregular action of the heart, which had for years been treated as organic disease of that organ, prove to be only sympathetic, and wholly disappear on the removal of the original disease.

The state of the pulse but little assists the diagnosis, being often as much affected in functional as structural disease of the heart. We cannot afford room to follow the author in his rationale of origina pectoris and sudden death, in structural disease of the heart, for which we must refer to his treatise. The chapter concludes with the following observations:—"It appears from all that has been said, that the only states with which we are acquainted which dispose to organic disease of the heart, if we except a state of habitual plethora, which tends to derange all the vital organs, are inflammation and too irritable a state of its fibres, arising either from a fault in the heart itself, or a cause of irritation in some other organ affecting it through the medium of its nerves.

We have seen, in the experiments above referred to, that, independent of the brain as the power of the heart is, it is wholly subjected to its influence; and that although for a certain time it is capable of its perfect function, after the brain has ceased to exist, yet while the brain and its nerves are entire, it is placed so completely under the influence of the nervous system, that through it, its power may even be instantly destroyed. This arises from the direct operation of the brain on the muscular fibre; but the heart, it farther appears from those experiments, is also, in common with all other parts, subjected to the power of the brain through the assimilating processes, on which the healthy structure every where depends. We have seen that when the influence of the brain was withdrawn from the lungs, the most formidable organic disease was established in the space of a few hours.

Thus it is that long-continued nervous irritation—that is, deranged or defective nervous power—is so frequent a cause of disordered structure: and it seems to be chiefly in this way that our frame at length decays. The due mechanism of every part depending on the healthy action of the nervous system, decays in proportion as its vigour is impaired. This observation applies to no organs more remarkably than to the heart and large vessels. As old age advances, an evident change takes place in them, even where nothing occurs that deserves the name of disease; they acquire too great a degree of firmness, and often in some parts a bony hardness. When the change is slow, the other vital organs at the same time, and for the same reason, gradually losing their power, little inconvenience is experienced, except that of increasing weakness."

The seventh chapter treats of the morbid affections which precede change of structure in the lungs. The author considers the lungs of all the vital organs the most liable to change of structure, which, together with the nature of their function and extensive sympathies, render them peculiarly liable to disease. But our limits prevent our entering at any length into this chapter, to which we refer the reader for the manner in which the sympathies which have so great a share in determining the course of disease influence this organ, whose strongest sympathy is with the liver, as that of the heart is with the stomach. We particularly refer to this chapter for a sympathetic affection of the lungs which frequently shews itself about middle life, and gradually, though often very slowly, undermines the constitution, and affords a striking instance of the manner in which disease at first so trivial as hardly to command the attention, gradually becomes complicated, and at length, involving many of the vital organs, incurable. In speaking of this case the author particularly calls the attention of the reader to the nature of those cases in which, by the sympathy of parts, more than one organ partakes of the disease. "That the reader," he observes, "may understand the nature of this disease, it will be necessary to direct his attention to the following circumstances. I have already had occasion to observe, that it is allied to dyspeptic phthisis—that species of consumption in which the disorder spreads from the liver to the lungs. It is in those who have been subject to indigestion that the disease we are considering is apt to shew itself, and here, as in dyspeptic phthisis, it is chiefly in proportion as the indigestion affects the function of the liver that the lungs are affected by it.

"Some degree of morbid acidity and flatulence of stomach, and some occasional derangement in the biliary secretion, are so common, that by many they are hardly regarded as deserving the name of disease; yet even these slight affections, if long continued, often produce a corresponding affection of the lungs, which like themselves is seldom considered of sufficient importance to attract much attention: but, however mild the symptoms may be, the state of the patient, when more than one vital organ is affected, is essentially different from that in which the disease is confined to one.

"However slight, it is a case of complicated disease; and, from the sympathy established between all parts of the body through the nervous system, the affection of each part tends to increase both the symptoms and obstinacy of the other; and it adds not a little to the evil that, from the nature of such cases, the organs affected must necessarily be such as greatly sympathize with each other. The effect of every cold, cause of fatigue, &c. falls chiefly on the debilitated parts, and thus also the evil accumulates." The two great sources of organic disease the author considers disordered circulation and disordered nervous power. He here points out the effect of each of these causes on the lungs. We shall have occasion to recur to this subject in giving an account of the second part of the work, in which the treatment of the diseases we have been considering is laid down. The author's account of the effects on the lungs of defective nervous power was first published in the Philosophical Transactions for 1817.

The eighth, ninth, and tenth chapters are devoted to the morbid affections which precede change of structure in the abdominal viscera, but our limits will not permit us even to enter on this part of the subject. The extensive sympathies of the liver, and particularly its sympathy with the brain, which with its liability to disorder causes it to partake of and modify so many diseases, the author enters into at considerable length; and he particularly dwells on the means of ascertaining the presence and degree of its first deviations from the healthy state, in which it is almost always in our power permanently to restore its healthy function, and thus often to prevent a long series of accumulating evils. We must here close our account of the first part of the Treatise. In our next number we shall review the second and last part, which treats of the means of correcting the states which precede organic disease, and counteracting their effects.

MEDICAL GAZETTE.

Saturday, August 27, 1831.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

LONDON UNIVERSITY.

A MEETING of the proprietors was held last Saturday, on which occasion an unsuccessful attempt was made by the friends of Mr. Pattison to procure a reconsideration of his case: the proposal, however, was negatived by thirty-seven to twenty-five. We presume, therefore, that so far as his professorship goes, we may look upon the matter as definitively settled. Having fully expressed our opinion last week, it is not our intention to enter upon the question again unless some fresh circumstances should occur to warrant us in doing so. There is one point, however, to which we would briefly advert. It seems to us, from the manner in which Mr. Pattison expresses himself in his "Statement," that he intended to convey the idea that Professor Thomson was to a certain extent implicated in the proceedings of Dr. Alexander Thomson. Now this is uncandid, and ought to have been avoided, because the public, knowing the parties only as father and son, might very naturally be misled. It is quite notorious to those acquainted with the facts, that Dr. Alexander Thomson, unfortunately for himself, was not in any degree under the controul of his father, nor at all likely to receive any influence from him. It might appear indelicate on our part to allude to such matters, but the step taken by Professor Thomson relieves us from this charge, and a desire to be strictly impartial, leads us to censure this as the least commendable part of Mr. Pattison's "Statement."

Professor Thomson, if the words he made use of at the meeting on Saturday be correctly reported, charged his late colleague with "ignorance, and unfitness for the situation which he held." This to be sure appears rather extraordinary after Dr. Thomson's testimony in his favour at no distant period; but, nevertheless, it has something tangible about it. Let Mr. Pattison be *proved* to be deficient in knowledge of his profession, and then indeed there will be some shew of justice in his dismissal, though his judges, with unrivalled obliquity of intellect, followed up the severest sentence which they had it in their power to pass upon him by a declaration of his unimpeachableness, and of his having passed through all his trials without any stain on his private or professional character. We suspect from all this that Mr. Pattison has not the art of making himself *popular*—that he was placed in a situation which rendered it the interest of some to expose and magnify his imperfections—that at first his colleagues were anxious to support him for the general benefit of the school—but that afterwards, fancying they found him deficient in those attractive manners which secure the good opinion of an audience, or those commanding talents which crush all opposition, but that the tide still ran against him, they at length made up their minds to throw him overboard, with a view to their common safety. Our belief is, that the other Professors in the London University were not men of such transcendant talents as to make Mr. Pattison shew, amid *their* brilliancy, like a spot on the sun's disk, unless very considerable exertions had been made to blacken him. At all events, it has been a most unfortunate connexion for the Ex professor as well as for the Institution, which has been most seriously injured by these discussions.

CHOLERA.

SOME "Papers relative to the disease called Cholera Spasmodica, &c." have just been published by the Board of Health. They contain a clear and succinct account of the disease, taken from various sources, but without any attempt to direct the opinions of medical men in this country. "So much knowledge and intelligence (observes the writer) are diffused among them, that until more uniformly successful modes of practice are devised, the Board wishes to leave their minds unbiassed." To the above are appended extracts from the Reports of Dr. Keir, from Moscow; and of Drs. Russel and Barry, from St. Petersburgh. The latter, together with the "Preliminary Steps," we subjoin, as most calculated to interest our readers:—

Extract from the joint Report of Drs. Russel and Barry.

St. Petersburgh, $\frac{3}{15}$ *July*, 1831.

SIR,

ALTHOUGH there can be no doubt that the disease now prevailing here is strictly identical, in all essential points, with the epidemic cholera of India; and although there are many descriptions extant of that malady, much more ably and accurately drawn up than any which we can pretend to give, yet we are induced to believe that a short account of the symptoms which we ourselves have actually witnessed and noticed at the bedside in some hundreds of cases, since our arrival here, may be useful,—first, because we are not aware that any description by an eye-witness of European cholera has yet been addressed to the British Government; secondly, because the disease, as it has shewn itself in this capital, when closely compared with the Indian cholera, appears to have undergone some modifications; thirdly, because, having now studied the disease in all its stages, our description, however imperfect, will at least assist towards establishing a standard of comparison with other local epidemics of cholera in Europe, and may, perhaps, enable those who have not seen this disease to recognize it with more certainty than they would otherwise be able to do.

The cholera morbus of the north of Europe, to which the Russian peasants have given the name of "chornaia colezn," or black illness, like most other diseases, is accompanied by a set of symptoms which may be termed *preliminary*, by another set which strongly mark the disease in its first, cold, or *collapse* stage, and by a third set, which characterise the second stage, that of reaction, heat, and fever.

Preliminary Symptoms.—We have but few opportunities of witnessing the presence of all these symptoms, some of which precede the complete seizure by so short an interval, that the utmost diligence is scarcely sufficient to bring the patient and the physician together, after their occurrence, before the disease is fully formed. Diarrhœa, at first feculent, with slight cramps in the legs, nausea, pain, or heat about the pit of the stomach, malaise, give the longest warning. Indeed, purging, or ordinary diarrhœa, has been frequently known to continue for one, two, or more days, unaccompanied by any other remarkable symptom, until the patient is suddenly struck blue, and nearly lifeless. Often the symptoms just mentioned are arrested by timely judicious treatment, and the disease completely averted. When violent vertigo, sick stomach, nervous agitation, intermittent, slow, or small pulse, cramps, beginning at the tips of the fingers and toes, and rapidly approaching the trunk, give the first warning, then there is scarcely an interval. Vomiting or purging, or both these evacuations, of a liquid like rice-water or whey, or barley-water, come on; the features become sharp and contracted, the eye sinks, the look is expressive of terror, wildness, and, as it were, a consciousness on the part of the sufferer that the hand of death is upon him. The lips, the face, the neck, the hands, the feet, and soon the thighs, arms, and whole surface, assume a leaden, blue, purple, black, or deep brown tint, according to the complexion of the individual, varying in shade with the intensity of the attack. The fingers and toes are reduced at least a third in thickness; the skin and soft parts covering them are wrinkled, shrivelled, and folded; the nails put on a bluish pearl-white, the larger superficial veins are marked by flat lines of a deeper black; the pulse is either small as a thread, and scarcely vibrating, or else totally extinct. The skin is deadly cold, and often damp; the tongue *always moist*, often white and loaded, but flabby and chilly, like a bit of dead flesh. The voice is nearly gone; the respiration quick, irregular, and imperfectly performed. Inspiration appears to be effected by an immense effort at the chest, whilst the alæ nasi (in the most hopeless cases, and towards their close), instead of expanding, collapse, and stop the ingress of the air. Expiration is quick and convulsive. The patient asks only for water, speaks in a plaintive whisper (the "vox cholerica"), and only by a word at a time, from not being able to retain air enough in his lungs for a sentence. He tosses incessantly from side to side, and complains of intolerable weight and anguish around his heart. He struggles for breath, and often lays his hand on his stomach and chest, to point out the seat of his agony. The integuments of the belly are sometimes raised into high irregular folds, whilst the belly itself is violently drawn in, the diaphragm upwards and inwards towards the chest, sometimes there are tetanic spasms of the legs, thighs, and loins; but we have not seen general tetanus, nor even trismus. There is occasionally a low, suffering whine. The secretion of urine is always totally suspended, nor have we observed tears shed under these circumstances; vomiting and purging, which are far from being the most important or dangerous symptoms, and which, in a very great number of cases of the present epidemic, have not been profuse, generally cease, or are arrested by medicine easily in the attack. Frictions remove the blue colour for a time from the part rubbed; but in other parts, particularly the face, the livor becomes every moment more intense and more general. The lips and cheeks sometimes puff out and flap, in expiration, with a white froth between them, as in apoplexy. If blood be obtained in this state, it is black, flows by drops, is thick, and feels to the finger colder than natural. Towards the close of this scene, the respiration becomes very slow, there is a quivering among the tendons of the wrist, the mind remains entire. The patient is first unable to swallow, then becomes insensible; there never is, however, any rattle in the throat, and he dies quietly after a long, convulsive sob or two.

The above is a faint description of the very worst kind of case, dying, in the cold stage, in from six to twenty-four hours after the setting in of the bad symptoms. We have seen many such cases just carried to the hospital from their homes or their barracks. In by far the greater number vomiting had ceased, in some, however, it was still going on, and invariably of the true serous kind. Many confessed that they had concealed a diarrhœa for a day or two, others had been suddenly seized, generally very early in the morning.

From the aggravated state which we have just described, but very few indeed recover, particularly if that state has been present even *for four hours* before treatment has commenced. A thread of pulse, however small, is almost always felt at the wrist, where recovery from the blue or cold stage is to be expected. Singular enough to say, hiccough coming on in the intermediate moments, between the threatening of death and the beginning of reaction, is a favourable sign, and generally announces the return of circulation.

In less severe cases, the pulse is not wholly extinguished, though much reduced in volume, the respiration is less embarrassed, the oppression and anguish at the chest are not so overwhelming, although vomiting and purging and the cramps may have been more intense. The coldness and change of colour of the surface, the peculiar alteration of the voice, a greater or less degree of coldness of the tongue, the character of the liquids evacuated, have been invariably well marked in all the degrees of violence of attack which we have hitherto witnessed in this epidemic. In no case or stage of this disease have we observed shivering; nor have we heard, after inquiry, of more than one case, in which this febrile symptom took place.

Fever or Hot Stage.—After the blue cold period has lasted from twelve to twenty-four, seldom to forty-eight hours or upwards, the pulse and external heat begin gradually to return, headache is complained of, with noise in the ears, the tongue becomes more loaded, redder at the tip and edges, and also drier. High-coloured urine is passed with pain, and in small quantities; the pupil is often dilated; soreness is felt on pressure over the liver, stomach, and belly; bleeding by the lancet or leeches is required, ice to the head gives great relief. In short, the patient is now labouring under a continued fever, not to be distinguished from ordinary fever. A profuse critical perspiration may come on, from the second or third day, and leave the sufferer convalescent; but, much more frequently, the quickness of pulse and heat of skin continue; the tongue becomes brown and parched; the eyes are suffused and drowsy: there is a dull flush, with stupor and heaviness, about the countenance, much resembling typhus, dark sordes collect about the lips and teeth, sometimes the patient is pale, squalid, and low, with the pulse and heat below natural, but with the typhous stupor; delirium supervenes, and death takes place from the fourth to the eighth day, or even later, in the very individual, too, whom the most assiduous attention had barely saved in the first or cold stage. To give a notion of the importance and danger of cholera fever, a most intelligent physician, Dr. Reimer, of the merchant hospital, informs us, that of twenty cases treated under his own eye, who fell victims to the disease, seven died in the cold stage, and thirteen in the consecutive fever.

The singular malady is only cognizable *with certainty* during its blue or cold period. After reaction has been established, it cannot be distinguished from an ordinary continued fever, except by the shortness and fatality of its course. The greenish or dark, and highly-bilious discharges produced in the hot stage, by calomel, are not sufficiently diagnostic, and it is curious that the persons employed about these typhoid cases, when they are attacked, are never seized with ordinary fever, but with a genuine cold, blue cholera: nothing, therefore, is more certain, than that persons may come to the coast of England, apparently labouring under common feverish indisposition, who really and truly are suffering under cholera in the second stage.

The points of difference between the present epidemic and the cholera of India, when the two diseases are closely compared, appear to us to be the following:—

First. The evacuations, both upwards and downwards, seem to have been much more profuse and ungovernable in the Indian than in the present cholera, though the characters of the evacuations are precisely the same.

Secondly. Restoration to health from the cold stage, without passing through consecutive fever of any kind, was by far more frequent in India than here, nor did the consecutive fever there assume a typhoid type.

Thirdly. The proportion of deaths in the cold stage compared with those in the hot, was far greater in India, according to Dr. Russel's experience, than here.

Fourthly. The number of medical men and hospital attendants attacked with cholera during the present epidemic, in proportion to the whole employed and to the other classes of society, has been beyond all comparison greater here than in India under similar circumstances; twenty-five medical men have been already seized, and nine have died out of two hundred and sixty-four. Four others have died at Cronstadt, out of a very small number residing in that fortress at the time the disease broke out there. Six attendants have been taken ill, at a small temporary hospital behind the Aboucoff, since we wrote last. It is certain, however, that in some cholera hospitals, favourably circumstanced as to size, ventilation, and space, very few of the attendants have suffered.

Of these facts we are likely to receive accurate statements in answer to the written questions which we have submitted to the medical authorities through the Government here.

Convalescence from cholera has been rapid and perfect here, as is proved by the following fact.—The Minister of the Interior had given orders that all convalescents, civil as well as military, at the General Hospital, should be detained fourteen days. We inspected about two hundred of these *détenus* some days back, with Sir James Wylie, and found them in excellent health, without a single morbid sequela amongst them.

Relapses are rare in this epidemic, nor have they been often attended with fatal results: hospital servants seem to have been most liable to them. One physician had three attacks, the second severe, in which he states that he derived great benefit from the Magisterium bismuthi.

In our next we shall resume the medical history of the disease, and

Have, &c.

(Signed) William Russel, M.D.
D. Barry, M.D.

Preliminary steps advised to be taken on the first appearance of Cholera.—It is of great importance that each town or village, particularly those on the coast, should be prepared with the best-arranged means to meet such a calamity as the breaking out of the disease now raging in the North of Europe, so as to prevent confusion upon the emergency of the moment, and be ready to act upon a well-considered system for preventing the spreading of infection.

With this view, the Board *recommends* the formation of a local Board of Health at each place, to consist of the chief magistrate, the clergyman, one or more medical gentlemen, and two or three of the principal inhabitants, who may immediately, and as occasion requires, correspond with the Board of Health in London, the medical members of the local Boards being deputed to write upon all subjects relating to any symptoms of the disease.

The best means of preventing the spreading of infection are, the immediate separation of the uninfected from the sick, by their prompt removal from the house of any infected person, or by the removal of any individual affected with the disease, if possible, to some house in a dry and airy situation, appropriated to the purpose; but in the event of such removal not being practicable, on account of extreme illness or otherwise, the prevention of all intercourse with the sick, even of the family of the person attacked, must be rigidly observed, unless the individuals who desire to stay shall submit to such strict rules of quarantine as the public safety may demand, and the local Board of Health, advising with the Board of Health in London, may consider expedient.

As success in the treatment of this disease, and preventing its spreading, has been found greatly to depend upon *early* medical assistance, it is of great importance that the heads of families and others should be vigilant in guarding against concealment or delay in making known every case which may occur.

On the removal of diseased persons, the rooms they may have inhabitated, and the house generally, should be thoroughly exposed to a constant current of air, and recourse had to all the well-known means of purifying houses, particularly the use of chloride of lime; and the bedding and clothing of the sick person, after removal, should be soaked in a slight solution of the chloride in water, and well washed. It is impossible that ventilation and cleanliness can be carried too far in the houses of the sick after removal; whitewashing, and a variety of means of effecting so important an object, will no doubt occur to the local Boards of Health; and a continuance of ventilation for some days as the best means of preventing contagion.

In large towns the local Board of Health should be composed of sufficient numbers to admit of sub-division into district committees, always attaching to each committee at least one medical gentleman.

For the information of the public, and to secure a ready and instant reference to authorized persons, the names and residence of the persons composing the local Boards of Health should be placed on the Church door.

In the event of so great a calamity falling upon this country as the introduction of this disease, rules and regulations upon an extensive scale, suited to the rigid system of quarantine which such an event would demand, will be immediately circulated by the Lords of his Majesty's Most Honourable Privy Council, who will, upon the earliest intimation of the existence of the disease, send down a medical practitioner, who has been acquainted with the disease as it occurred in India.

In the name of the Board,
HENRY HALFORD, President.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

GRAY'S HOSPITAL, ELGIN.

Case of Suicidal Laryngotomy—Nutrient Clysters—Recovery.

D. S. a young man, about 22 years of age, was admitted into Gray's Hospital, Elgin, on the 12th of May, under the care of Dr. Paul, having an hour before he was brought to the hospital attempted suicide by cutting his throat with a razor. This unfortunate lad, as he afterwards confessed, made three cuts, following very exactly the same line of incision, which passed immediately under the os hyoides, and almost severed this bone from the thyroid cartilage, a very small portion of the circle of the larynx being left undivided on its back part. The incision on each side took a direction upwards, towards the angles of the lower jaw, and laid open the pharynx. When the patient's head was raised, the wound had a very unpromising aspect, the larynx gaped widely open, and the stimulus of the air, and drops of blood trickling into it, brought on awful convulsive fits of coughing, which threatened immediate suffocation. His face and lips were blanched with the loss of blood, which had been excessive, as the place where he committed the deed bore ample proof. Still there was no vessel that required to be secured. He was in a state of constant shivering, the pulse scarcely perceptible at the wrists, and the extremities icy cold. The edges of the wound, after it was cleared of coagulated blood, were brought together, and kept in apposition by seven or eight stitches. He was placed in bed in a semibent position, with his head approximated to the sternum by means of a bandage, and warmth in every possible way applied to the surface of the body. In the course of a few hours he became naturally warm, and passed a tolerably quiet night. Next day clysters of beef-tea were had recourse to. On the third day a flexible gum catheter was passed through the left nostril into the œsophagus, and about ten ounces of beef-tea poured into the stomach. In doing this a good deal of irritation was excited, when the beef-tea was instantly rejected, the greater part of which came through the wound on the right side, and tore it very much open. Two days afterwards a similar attempt was made, but with no better success. No further attempt was made to introduce a tube through the nostril till the 23d, when the irritation could be borne, and from twelve to sixteen ounces of milk, or beef-tea, were passed into the stomach daily, till he was able to swallow liquid nourishment without its passing through the wound, which he began to do on the 30th of May. Till this time clysters of strong beef-soup, with the yolks of eggs, were administered four times daily; every third day a purgative one being used. The nutrient clysters were generally well retained; when they were not, the addition of a few drops of laudanum had the desired effect. The wound on the left side, as far as the centre of the thyroid cartilage, adhered by the first intention; but on the right side it was found necessary to put in fresh stitches twice after the first dressing; and at this part the saliva continued to escape for a long time, and the edges of the wound required to be frequently touched with the nitrate of silver. It was not fully closed till about the end of July, and then there remained no injury to the function of any part farther than the loss of sensation in the chin.

Remarks.—At the first view of this case, little expectation of a favourable result was entertained, so frightfully large did the gap appear; but contrary to expectation, the healing process went on remarkably well, and little disturbance of the system was induced. The incision extended very close indeed to the carotids on both sides, and even appeared to pass the course of the linguals, but it must have been somewhat below them, because the division of one of the lingual arteries would in all probability have been followed by fatal hæmorrhage, unless surgical aid had been at hand. The hemorrhage was very profuse; but it may be satisfactorily accounted for from the division of veins that were necessarily in the way, as well as branches from the lingual and superior thyroidal arteries. In wounds of the throat, more particularly when the pharynx is opened, no attempt should be made at swallowing for at least ten days or a fortnight; nor should a tube, for the same space of time, be introduced through either nostril, because, however adroitly done, it invariably excites a great deal of irritation and a desire to vomit. The reason for avoiding deglutition is obvious: when the wound is close to the os hyoides, and an attempt made to swallow, part of the liquid passes through the glottis, and excites frightful paroxysms of coughing, by which the wound is torn open, as well as the patient's life endangered from suffocation; and when lower down, although not attended with the same dangerous consequences, it is still most desirable to keep the parts in as quiet and undisturbed a state as possible, because this will facilitate the healing of the wound, and be the means of guarding against inflammation in the respiratory passages. I am not at present prepared to state facts to shew how long a patient may be supported by nutritive clysters, but at all events long enough to allow the healing process in wounds of the throat to be so far advanced as not to be afterwards materially disturbed by the act of deglutition.

MEATH HOSPITAL AND COUNTY OF DUBLIN INFIRMARY.

Cases treated by Mr. Hamilton, *under the superintendence of Dr. Stokes; with Mr. Hamilton's remarks.*

Acute Rheumatism.

Julia Maher, ætat. 26, was admitted on the 14th of March, 1831. Complains of severe pain of the right knee above the patella, on the inner side of the ankle of the same side, and on the outer and back part of the left knee. The first two places are slightly swollen, but not red; the last two both red and swollen. The pain in all is constant, aggravated by motion or pressure, and becomes particularly severe at night, depriving her of rest, it also shoots from the knees to the hips and ankles. Some mitigation takes place from the application of heat or sweating.

Her history is briefly as follows:—Three weeks since perceived her knees and ankle-joints to be slightly swollen and stiff; a week after she was seized with pain in the back and right side, without any other symptom. The joints of the lower extremities next became affected, the pains being erratic.

Since their commencement up to the present time, she has had little sleep, and generally sweats at night. Has lost her appetite. Tongue clean; pulse regular; bowels free. Was ordered leeches to the knee, followed by a poultice, and

℞ Opii, gr. j.
Cal. gr. j.
Ft. Pil. vj sumat unam ter in die.

Rep. Pil. et applicetur Hir. viij. utrique genu.

Yesterday she vomited, after taking the pill of Cal. et Op.; the former was ordered to be omitted, the latter continued. The pains are greatly relieved! A profuse perspiration came on from 4 to 7 p. m. Mouth sore; pains less.

℞ Pulv. Dover. gr. x. ter in die.

She now complains of a severe pain of the head, darting from the forehead to the occiput; also a pain in the back. The other pains less. Pulse strong and quick; tongue slightly furred; bowels confined.

Hir. xii. temporibus.
Haust. Rhei.

20th.—Head ache gone, as also nearly the pains in the knees, numbness and stiffness being now more complained of than pain. No medicine.

21st.—Some little return of head-ache, and small lumps can be felt under the skin in the temple. No medicine.

22d.—Slight pains in knees complained of, attended with a good deal of stiffness;

sweats profusely in the night, so as to wet the sheets through and through.

℞ Liniment. Camph. ʒvj.
Tinct. Opii, ℥ss.
Perfricantur genua.

23d.—Very severe pain last night, extending from the right knee into the hip, attacking also the left shoulder and side of the chest, so as to impede respiration.

Rep. Lin et Hab. Opii, gr. j. ter in die.

This relieved her; she was then put on the use of sulph. quinine gr. i. ter in die, and dismissed entirely free from pain on the 8th April, about three weeks after admission.

Periostitis, with Abscesses.—Constitutional Treatment.

William Darcy, ætat. 32, a labourer, admitted April 5th, 1831. Complains of very severe pain in the right side of the head, extending down the side of the face and neck as far as two abscesses; one situated at the upper part of the sternum, not yet open, and of a dull red colour; the other at the internal third of the left clavicle, open, and in which a probe can be passed more than an inch and a half, discovering the bone to be carious. From the left side of this abscess a hard tender swelling extends for more than two inches along the clavicle, and a very short way up the neck.

All the right side of the head, face, and neck, is very tender, and in some places red and very slightly swollen, with a sensation of numbness. The pain in the head is constant, worse at night, and aggravated by cold or wet weather, or exposure to wind. He has no other pain. His history is as follows:—He had several attacks of the venereal disease, for which he was salivated two or three times. The last attack was six years ago. Since that time his health continued pretty good, without pain or eruption, till a year since, when he first experienced a severe pain about the middle of the sternum, with some degree of swelling, worse at night, and increased by pressure. and, a week or two after its first appearance, accompanied by a swelling every night in the throat and face, which went off in the morning. After several months he was relieved from this affection in this hospital.

At the time of the snow, about nine weeks ago, he caught cold, and pain was experienced at the left clavicle, and side of the neck; the pain leaving the left side, attacked the parts at present engaged with great severity; since which time he has been deprived of rest at night, and lost his strength and health. The abscess on the upper part of sternum appeared a fortnight since, preceded by pain in the spot. Appetite good; tongue loaded, red at tip; bowels confined; pulse slow.

Hir. xii capitis parti sinistræ; et habeat
Decoct. Sarsap. ℔i.; Acid. Nit. ʒi.

6th.—Little relief of the pain. Two needles were inserted about an inch deep into the temporal muscle, with great mitigation of the pain, though not its entire removal.

Rep. Decoct.

7th.—He slept yesterday two hours after the insertion of the needles, and his headache for the rest of the day was not so severe; to-day, however, it is nearly as bad as ever. The abscess in the sternum was opened, and the opening in the one on the left clavicle enlarged.

Rep. Decoct. et applicetur Vesicat. Capiti.

The leeches and blisters were repeated, with the daily use of the decoct. sarsap. and occasional purgatives. By these means the pains became gradually less, and at present he has no pain any where, but only some slight degree of swelling and a sensation of numbness in the right side of the face (April 23d). The abscesses, though not yet healed, are greatly improved in appearance, the centre granulating, and the parts are less red and swollen, and no longer tender.

Remarks.—The result of this case is very satisfactory, as, from the obstinacy and severity of the pain, little good was at first expected to be produced. At present the man is at least in a state of comparative ease from a very distressing affection. Some of these cases of periostitis in bad constitutions, and where the system has been shattered by repeated courses of mercury, assume a very severe form, the periosteum not only being painful and tender to the touch, but a semi-cartilaginous deposition taking place between the bone and periosteum, ending in suppuration and disease of the bone, as in this case. An abscess, however, frequently forms, without being preceded by the semi-cartilaginous deposition from inflammation of the periosteum; such was the case in Graydon, the man with jaundice, under Dr Graves. The semi-cartilaginous deposition appears also, when occurring in good constitutions, to have a disposition not to run into suppuration, coming and going for a number of years with little change in size or situation. The pain is always most severe when this disposition exists, and is often of an intermitting character, being worse at night. So great was the agony in one woman with a swelling from this cause on the lower part of the tibia, that greater part of the night was passed in pacing up and down the ward. In her it was of many years standing, and had been often treated. When abscesses form in broken constitutions, such as this man's, they are of a dull purplish red, sluggish, and from the diseased bone, very difficult of cure. The best way appears to be, to open them freely, dress from the bottom with red precipitate and lint, and on the appearance of granulations to touch with the sol. argent. nit. and apply a strap. The first favourable impression on the pain was by acupuncturation.

ON FAILURES IN LITHOTOMY *.

[Continued from p. 608.]

CASE IV.—*Fatal case, from injuries to the Bladder, in attempting to extract the remnant of a Stone.*

THIS was a young man of eighteen years of age, who had for some years been suffering from a stone in the bladder, and having made up his mind to have it removed, placed himself under the care of a good operating surgeon for that purpose.

Nothing could be better than the style in which the operator reached the bladder, by his superficial and deep incisions; the prostate cut, (with the knife,) the forceps introduced, and the stone seized. Unluckily, it broke in the gripe of the instrument, and notwithstanding the means employed, which lasted for a very long period, the fragment of stone was left in the bladder.

A few days passed without developing any very bad symptoms; there were, indeed, some slight pains about his loins, and a little tenderness about the lower part of the abdomen, but not of a kind to require active treatment. Soon, however, irritative fever shewed itself; he wasted, and died a fortnight after the operation. The difficulty of using the forceps, of catching hold of the stone, in this case, probably arose from a portion of the bladder having contracted upon it; for the forceps would seize the point of the stone repeatedly, and as frequently slip from its hold.

This contraction, however, did not subside, as it is said to do, after some time had elapsed, for the operation lasted nearly two hours without this subsidence taking place; and that a spasmodic contraction alone held the stone thus firmly, is probable, from its being found loose at the bottom of the bladder after death.

The following is the report of the dissection given to me:—

Sectio Cadaveris.—"The intestines, liver, and the whole of the digestive organs, were in a healthy state. The right kidney was in a state of suppuration; the left contained fetid serum. The ureters were enlarged, their coats being much thickened, and highly vascular. The coats of the bladder were full half an inch thick. The internal part of it was every where covered with black fetid mucus, except the fundus, which appeared more healthy, but inflamed. A piece of stone was found in the lower and back part of the bladder. The prostate gland was hardly any thing else but a mass of fetid matter."

This young man unquestionably died from the long continued violence of the operation, in the attempt to extract the portion of stone. The condition of the prostate gland, the kidney, and bladder, sufficiently proved the fact, that their injuries, by the production of a hectic or irritative fever, destroyed the patient. The operator was placed under trying circumstances—to take the stone or leave it; choose which he would, there was mischief in his choice.

Perhaps, however, too much fear is entertained of leaving a fragment of stone in the bladder; but should not this yield to the greater and more rational fear of lacerating and contusing the prostate and bladder, by a violent operation of long continuance, in a vain attempt to extract it? Why not leave it there, when moderate and well-directed exertions have failed to remove it?

We know that stones in the bladder have passed away safely through the external incisions repeatedly, many days after a fruitless operation to remove them; but, it is probable, a patient rarely escapes death, whose bladder and prostate have been at once bruised and torn by the forceps, or pinched and rubbed by them, during a long operation. We also know, from experience, that delaying the extraction of a stone, where it would have been highly dangerous to have attempted its immediate removal, has been purposely practised with safety*.

Nothing can be more dangerous than rubbing, or perhaps pinching the bladder, for an hour or two, with the blades of the forceps. Leaving the stone to its chance of a natural exit, had we sufficient moral courage to do so, would be incomparably preferable to this treatment.

On the whole, therefore, and in a case similar to the foregoing, I should prefer relinquishing the fragment of stone, rather than continue a violent irritation in searching it out, or in vain attempts to pluck it from the spasmodic grasp of the bladder. There is an evil to be apprehended in both practices, but the least in that which requires the least violence. The spasm of the bladder, which holds the stone, may subside; and in such a case, the stone might be expected to offer itself at the external opening. The spasm, indeed, is more likely to yield after the teazing of the bladder has been withdrawn, by a cessation of all further attempts. This was certainly the case in the foregoing example. As long as the excitement from the operation lasted, so did the spasm which held the stone. It is, indeed, a law of all spasmodic actions, that they last not long if their source be removed, and this would seem to be an additional argument for leaving a fragment of stone in the bladder, at least for a time, under the circumstances already described.

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* From Mr. Fletcher's Medico-Chirurgical Notes and Illustrations.

* See Covillard—Observat. Med. Chir. Liv. 4, Collot. Trait sur la Lithot. p. 178.

That the mucous lining of the bladder is prone to inflammation, to a degree that cannot be sufficiently impressed upon the mind of the surgeon, is clear, from the following interesting case, which is an additional proof of the caution and gentleness required in the treatment of this viscus.

CASE V.—*Death, from sounding for a Stone.*

A very engaging little boy, of six years of age, was brought to a hospital, with symptoms of stone in the bladder. He was sounded twice by the receiving surgeon, who satisfied himself, on both occasions, of the existence of a stone.

A day was fixed for removing it, and a large party of medical practitioners were assembled, to view the operation. The boy was placed in position, tied, and the sound introduced; but the operating surgeon could not now feel the stone, although he took the proper motions with the instrument for that purpose. The boy's bladder was full of water, the penis having been closed, and abundance of milk and water drunk about an hour before the scene which was now passing. "Let me try," said the consulting surgeon; he did so, but very roughly, and for no short period of time, with no more success than the operating surgeon. Another and another then tried, in succession, but nobody could feel the stone,—and the whole assemblage of eight or ten visitors would have continued to hunt for the stone in this unfortunate boy's body, had not the operating surgeon very properly interposed, by saying, "Stop, gentlemen, if you please; this is my patient, and I fear you will do his bladder no good by proceeding further to-day." He was right, though his decision came too late. The little boy was carried to bed, complaining that his belly ached, and from that bed he was doomed to rise no more. Peritoneal inflammation of a very active kind followed, and in spite of the utmost attention, and early vigorous treatment, he died, on the fourth day from the date of the sounding, asking piteously and affectionately, though without avail, for his far distant mother.

On examination of the body, it was found that the inner lining of the bladder was highly inflamed, spotted red every where, and that its peritoneal covering, at the fundus, was glued to the intestines, which were, on all sides, inflamed, and smeared with lymph.

In addition to this melancholy illustration of the necessity of treating with caution and forbearance the human bladder, it teaches also another kind of practice than that which is common in our operation-rooms.

If sounding for a stone is sufficient to kill the patient, without the operation of lithotomy itself being performed, it is clear, that sounding at all, immediately before the operation, is a bad practice, if continued beyond the period of a ready and immediate striking upon the stone. The movements of the sound in the bladder must irritate its mucous lining, nay, very possibly inflame it in some degree, and then comes the succeeding and more violent irritation of the operation itself; and who could wonder if, under such circumstances, fatal inflammation should follow? There can be no question, therefore, that sounding or ascertaining the presence of a stone in the bladder, by the surgeon and consulting surgeon, should be an act performed many days before the operation itself is executed, and that this preliminary measure, to any considerable extent, should never be allowed on the same day with the final operation.

On the present occasion, a sharp dispute arose between the operating and the consulting surgeon, as to the propriety of cutting the patient, upon the strength of the fact that a stone had been distinctly felt by the operating surgeon, some days before the one on which the operation was to be performed. "If," said the consulting surgeon, "it was there a few days before, it is there now." There could be no denying of this position, and yet the operating surgeon was right in refusing to cut this patient without the consulting surgeon having satisfied himself also of the presence of a stone. In this particular case he was more than commonly right, in saving, by his firmness, the operation from the discredit of a failure, but more especially, the unfortunate child from great additional punishment in an useless and terrible operation; for, as the remark and advice of the consulting surgeon were given subsequent to the sounding, the operation itself would have been, probably, both cruel and unavailing.

CASE VI.—*Death, from continued violence in seeking for a small stone, after the Bladder was opened.*

A surgeon made his way very skilfully into the bladder of a little boy, in which a stone was distinctly felt, and he could, on the introduction of his finger, occasionally touch it. The forceps were introduced, with closed blades, and the point of the instrument every now and then would strike upon the stone, but when the blades were opened, and the surgeon endeavoured to grasp the stone, he found it constantly eluding their gripe, or slipping out of them. The operation continued in this way for nearly half an hour, the patient complaining greatly of how much he was hurt; but at length the forceps seized the stone securely, which was extracted with the utmost ease—for its size was singularly small.

The boy was put to bed, struck heavily

by the operation—he was cold, and somewhat torpid, with a very feeble pulse: and from this state he never recovered, although cordials and opium were given to him.

There was some, but very slight, tenderness of the abdomen on the following day; the patient was bled, and took opening medicine, and, of course, treated for peritonitis; but he died on the fourth day from the date of the operation.

On inspecting the body, no signs of inflammation could be detected in the bladder or peritoneum—all was pale and healthy. The bladder was, indeed, thickened, but this must have been the work of times past.

That this boy perished from the effect of a long-continued and worrying operation upon his nervous system, is sufficiently clear from the dissection, and also from the circumstance that he never rallied, but remained cold, with the peculiar torpid and fatal heaviness upon him, which is seen when little children are sent into hospitals, with dreadful burns about the trunk of the body. The destruction of the power of the brain and nervous system, by the violence of the shock, is the cause of death in both instances.

There is much variety of opinion as to the propriety of bleeding after lithotomy. Some practise it, and strongly recommend that it should be had recourse to upon the detection of any tenderness about the abdomen, notwithstanding a feeble pulse. There was certainly tenderness in this case.

For my own part, and from experience it is stated, I should be slow of bleeding after lithotomy, whilst evidence of decided prostration of the nervous system remained, in the shape of languor, indifference to external objects, sleepiness, and diminished temperature, even should some tenderness be present.

Above all things, it is presumed, the surgeon should be cautious in bleeding children under these circumstances, and especially gentle in his treatment of them during operation. It is very true, that in lithotomy children do better than grown persons; more recover. But this is to be accounted for on the ground that the operation in them is comparatively nothing. The stone is always small, and the operation throughout is much easier and quicker to perform, and, therefore, the little patient has less to endure; for the explanation of the greater success of lithotomy in children cannot be in their superior power of bearing suffering. Their irritability is greater, their nervous system sooner excited; and hence, in dentition, and in irritations within the alimentary canal, are we often obliged to witness the most distressing sufferings. It is probable that the majority of deaths from lithotomy, in young children, is from the blow inflicted on the nervous power by the necessary severity of the operation, and not from inflammatory actions set up by it.

In the second part of these sketches will be detailed some fatal cases, arising out of the irritability of children, especially from the application of blisters, and this will be done without any intention of supporting or advocating, by their publication, the practice of Mr. St. John Long, whose patients, however, were not children, though a certain impertinent noble lord has—possibly from a highly culpable belief in their credulity—most ungallantly bestowed that term upon them.

In the same part will be detailed a successful method the author employed to pass a stone from the bladder, of the size concerned in the foregoing case, without cutting, or forceps, but which could not consistently appear here, in the midst of a note devoted to failures.

Case VII.---*Death from Peritonitis, the effect of a lacerated Bladder from Violence in the Operation.*

The two following cases are in the words of my predecessor, who, it is well known, was an excellent and a successful lithotomist.

"I operated, in lithotomy, on a tall, well-made man, twenty-nine years of age, who had been upwards of two years suffering severely from the stone, but, though somewhat emaciated, and apparently of an irritable constitution, he appeared by no means an improper subject for the operation.

"In the introduction of the conductor I met with some resistance. I am not certain whether it was from my not having divided the muscles with sufficient freedom, and consequently the urethra close to the prostate gland, or from my pressing the beak of the conductor with too much force against the groove of the convexity of the staff. However, I resumed my knife, and divided the urethra and a very small portion of the prostate, and then the conductor passed with ease. The stone was readily laid hold of by the forceps, *but the resistance was so great, that it was not without spending some time, and using much violence, that I could extract it.*

The man bled very profusely, immediately after the coming away of the stone. However, after his legs were untied, and his thighs brought together, the hæmorrhage very much diminished, and he was sent to bed. He complained immediately of great pain just above the pelvis. After he was put to bed an opiate was given to him. A very little weeping of blood from the wounds was observed. An hour after I left him I sent my pupil to examine if it continued. He brought me word, that there was a continuance of the hæmorrhage in an increased degree, and that the man was in great pain. I immediately went to him, and found him in the condition described. I perceived that the discharge, though thin, was not urinous, and, therefore, I concluded that it was chiefly the serum oozing from coagulating blood, and consequently that there was a considerable lodgment in the bladder. I gently dilated the wound, and had the mortification to find my opinion confirmed. I brought away a large quantity of coagulated blood. The poor man expressed a sense of great ease, but then the blood flowed most copiously through the external wound. I introduced my finger covered with lint, and took other alike ineffectual means to restrain the hæmorrhage. Dr. Cheston was so obliging as to lend me his assistance. I tried a cannula covered with lint dipped in astringent liquids, and a variety of methods, with little or no benefit. The man lost an immense quantity of blood. At length we left the wound to itself, and applying a solution of sal ammonia cold over the belly, and over the wound, and rags wetted in the same to the hypogastrium and between the thighs, the hæmorrhage was entirely suppressed. The pain, however, continued. It increased during the night. The next morning his belly was sore, and somewhat tense. He was immoderately thirsty. He was sick, and troubled with frequent and feeble eructations.

"He continued to grow worse and worse, and died on the fourth day.

"On dissection there was a *considerable laceration of the bladder*, in a variety of directions, though none of them extensive.

"It seemed that the hæmorrhage was from the bladder, but this we could not fully ascertain. The peritoneum was generally inflamed, but there was no other unnatural appearance.

"I do not, upon a review of this case, see any thing which could have been done in addition to, or variation from the means used, unless it was that I should have made a more free wound in the muscles, *if I had been aware of the size of the stone*. For though the resistance was apparently altogether in this bladder, yet it is probable that the wound in that organ would have been sufficient, and the opening would have dilated, if I had by a very free and large incision through the muscles, removed all support to resistance, which the bladder itself had given."

There can be no doubt of the original incisions being imperfect in this case—that room enough was not obtained by them to transmit the stone easily; and that the attempt to force a passage lacerated the bladder, and produced fatal peritonitis.

The hæmorrhage was great, but had no share in producing the death of the patient. On the contrary, it was a likely means of preventing or checking the growth of the peritoneal inflammation which followed; and that it did not accomplish the desirable event, furnishes an additional and most powerful reason for our being constantly upon our guard to avoid violence, which may produce an inflammation, so often intractable, and quickly fatal. In the present case it was scarcely controlled by the loss of more than three pints of blood;—its undoubted origin was the violent working of the forceps.

The case has other points of interest, which the reader will readily discover and appreciate.

CASE VIII.—*Peritonitis from violence in operation; from which the patient partly recovered, though he ultimately perished of Abscess of the Pelvis and Kidney.*

"I cut a boy of ten years old. He lost a good deal of blood in the operation, from a branch of the pudica. In the evening his pulse was frequent, but in other respects he was doing well. In the night he complained a little of his belly. The next morning, his pulse being frequent, and his belly somewhat tense, I took from him about eight ounces of blood,—by the time of his having lost it, his lips became pale, and he shewed signs of faintness, and soon after was a little sick, and a profuse sweat broke out. His pulse grew a great deal quicker, and smaller, and it is scarcely credible how rapidly the peritoneal affection increased. Before the bleeding, he could bear his belly to be struck gently, or to be handled without pain. It was not so much swollen; but within half an hour after the blood had been drawn, the abdominal region became universally very much swollen, tympanitic, and exquisitely sore to the touch. He appeared to be in a great deal of pain, and his countenance grew expressive of great distress. His pulse was uncommonly quick and feeble, probably at least 180. He had had no stool since the operation. His urine was freely discharged through the wound, and some had passed through the urethra.

"Clysters were given him, a blister was applied to the false ribs on one side, and a sinapism to the other. Warm fomentations of poppy heads, decoctions, and crude sal ammonia, were used to his abdomen for an hour or two; but they seemed to do mischief and to increase the pain and the swelling. Infus. Senn. co. P. I. was given him, and afterwards some Ol. Ricin. after using the fomentations for two or three hours. Finding they afforded no relief, I changed them for Spt. Vin. Camph. cu. Tinct. Opii, applied cold, which seemed to lessen both the soreness and the pain. The next day he was better, the soreness of the abdomen was lessened.

"The twenty-first day after the operation he died. In the course of seven or eight days he was very much amended, having natural stools, and a great part of the urine passing through the urethra. His abdomen was free from pain, but his pulse was always quick; his appetite did not return; he had generally upon him a thirst, and his tongue was in general whitish; nor had his abdomen a natural feel; so that it was pretty evident there was some latent mischief, though of what kind I could not tell; beside, he every day grew more and more emaciated. His nights, though not painful, were disturbed. The nurse observed that his water was often whitish.

"Upon opening his body, I found the bladder contracted to a very small size, so that its cavity would not have contained more than two or three spoonfuls of urine. It was nearly half an inch in thickness;—red spots, as of inflammation, appeared here and there on its mucous coat, which, however, seemed in no place to be ulcerated.

"The wound which was made by the operation, and which, probably, had been somewhat increased in magnitude by ulceration (*for the sides of the wound had sloughed*), appeared to be larger than I expected, or intended it should have been; for the prostate gland was completely cut through, and the incision was continued quite through the neck of the bladder; the orifice of one of the seminal ducts was obliquely wounded, notwithstanding the great care which I took in directing my prostate knife.

"One kidney was very little altered from a natural state; its pelvis, however, and the beginning of the ureter, were very much enlarged. The other kidney was merely a leathery cyst, full of matter, but a great deal diminished from its natural size. In the neighbourhood of the bladder, the intestines were adherent one to the other, and appeared to be considerably inflamed. Upon tearing the adhesion through, the posterior part of the pelvis was found full of thin pus; probably there was a pint and a half of that fluid. The sacrum was even bared by its action, and the rectum was loosened from its attachment to it.

"*Remarks.*—It is a good maxim for a surgeon, when he has lost a patient after an operation, always to suspect that, in performing it, he was guilty of some error or oversight. For if he persuades himself that he has done every thing right, he is precluding himself from the very chance of improvement. Now, to criticise upon my own operation, I suspect that I made the wound with my knife too deep within the pelvis, by which means the peritoneum was too much exposed, and probably was bruised in the extraction of the stone; secondly, I suspect that I did not make my wound far enough in the urethra, that is, towards the bulb, but that, if any thing, I leaned too much towards Raw's method of cutting, instead of rigidly adhering to Cheselden's lateral method, as described by him, but more particularly by Bromfield.

"I rather think that in small subjects the neck of the bladder is completely divided, almost always, though in adults that it is not the case, on account of the greater size of the prostate."

As in the former case, so it will be seen in this, that the great loss of blood did not prevent the occurrence of peritoneal inflammation, which was probably the effect of the rough use of the forceps; and this was evident by the sloughing condition of the wound into the bladder, or the points on which the instrument acted: or, to take the operator's own explanation, it was violence or bruising of some sort, unnecessarily bestowed, either by the knife, or in the extraction of the stone.

Nor was Mr. Trye an unlikely person to commit such sort of violence occasionally. A good operator generally; in lithotomy he was a master. But like many other operating surgeons of great eminence, distinguished by the splendour and success of their undertakings, his high qualities of boldness and decision—the offspring of a superior anatomical education—would sometimes run a little wild, and be mischievous. Knowing what he could do, he was now and then somewhat proud of his really fine talent, and gave his patient no quarter. In plain English he was rough: and at times and seasons when roughness might well have been spared;—but, in his roughness, lay the stern proof of his uncommon excellence as an operator,—a rare union of science, with a cool, calm courage, that was never known to flinch, or be for a moment disturbed, by the most embarrassing circumstances of blood and difficulty.

The foregoing eight cases are the whole number of deaths (with the exception of one) which followed the operation of lithotomy, or attempts to perform it; and the whole that perished out of fifty-nine operations which have been performed within my knowledge. They all died from violence.

Whilst it is admitted that the success of the operation, in the number of lives saved by it, was greater than usual—yet it may be safely affirmed, that, with the exception of the first, and perhaps the third case, the entire loss of life arose from a violence that may be deemed unnecessary; which, with ease, coolness, and constant reference to the principles of the operation, might have been avoided, and the patients' lives saved.

It may so happen, indeed, that, in irritable subjects, the ordinary violence of a well-conducted operation would be sufficient to excite some of those fatal affections which destroy life from lithotomy. But, amongst the foregoing cases at least, there is no proof that such was the fact. Marks of unnecessary injury were visible in all which were examined, and which satisfactorily account for the death of the patients. But, to prove that excessive irritability of constitution, excited by a more gentle operation, was the cause of one or other of the destructive morbid affections which destroy life after it, it would be necessary to shew that no marks of excessive violence could be traced upon the parts by dissection.

There is one particular case in which the mark, or proof, is left only in the constitution. Circumstances will sometimes arise, which will unfortunately cause a protraction of the ordinary or necessary violence to be continued for a very long period.

Here the nervous power is exhausted by the long continued irritation; the patient never rallies, but, in a few days, drops, without a struggle, into the grave.

In the first case detailed, or that of a large stone, but where the openings by the knife were made very free, the violence used in extracting it cannot well be called unnecessary; for, unless there are certain means in the power of the surgeon to break the stone, (which is not the fact,) he must either pull it away by main force, or leave it in the bladder. The last is rarely done, if the surgeon's personal strength is sufficient to accomplish this deed of butchery. But such is human nature, that it is much to be feared the patient's safety is not enough considered amidst the desperate and excited feelings of the moment, which occupy the mind of the surgeon;—self-love and pride are awakened, and he will too often be eagerly thinking,—if he thinks at all,—of success, come how it will, rather than allow his reputation to be disgraced, by not obtaining possession of the stone.

This, in the refined language of modern times, would be called a moral weakness,—but perhaps there is an error in the term, could it be shown that, at such a fearful period, the surgeon was quite master of his understanding.

But why, it is humbly inquired, is the lateral operation performed at all, where the stone has been ascertained to be above the safe size for extraction in that direction? It is surely any thing but a scientific operation to force a rough stone, by such a route, as big as your fist, through an aperture, in a delicate and important part of the human body, only one inch wide!

But, if you have done this yourself?—True; and all I can plead is the sanction of my betters, and the improbability, nay, impossibility of doing otherwise, in the lateral operation, with a stone—impenetrable—and nearly as hard as flint.

Upon the detection of a large stone, (and no man should undertake lithotomy without being previously acquainted with its probable size,) I would relinquish all thoughts of the lateral operation, for with our present knowledge, that mode allows of no certain method of disposing of such a stone. With all their disadvantages,—and they are such as will not admit of comparison with the lateral operation, when the stone is not very large,—I would prefer the recto-vesical, or median section, when it is of a great size. If the fears of fistulous remains, or injury of the seminal ducts, should make surgeons reject the first, the last method is free of such objections. The bladder is opened the shortest road, the wound admits of the greatest dilatation, being exactly in the centre of the outlet of the pelvis, and surrounded by soft parts only. As to the danger of wounding the seminal ducts by the first method, it is by no means an accident confined exclusively to the recto-vesical section, inasmuch as in Case VIII. of these notes, it will be seen that this important vessel was cut by a first-rate lithotomist, in performing the lateral operation. This last, however, in a general way, is probably superior to all other methods of cutting for a stone; but, in the parcular case of a large stone, the median section, the last pointed out by Professor Vacca, is the one which would oppose the least obstruction to its passage, and where the operator could not be embarrassed, in the delivery of the stone, from having cut a large vessel, which, in this mode of operating, is altogether impossible;—and which circumstance is of great importance, and vastly in its favour,—for nothing can be conceived more embarrassing, among all the difficulties of lithotomy, than to encounter, at the same time, great hæmorrhage and a very large stone.

APOTHECARIES' HALL.

Names of Gentlemen to whom the Court of Examiners granted Certificates of Qualification on Thursday, August 18, 1831:—

Hen. Augustus Betts.
William Elliot.
Thomas Eden.
Edward Ingram.
Salter Livesay.
Richard Roe.
Thomas Salt.

BOOK RECEIVED FOR REVIEW.

Papers relative to the Disease called Cholera Spasmodica in India, now prevailing in the North of Europe. Printed by Authority of the Lords of His Majesty's Most Honourable Privy Council.

NOTICE.

Dr. Prout's answer to Dr. W. Philip, in our next.

W. Wilson, Printer, 57, Skinner-Street, London.

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DR. PROUT'S REPLY TO DR. PHILIP.

To the Editor of the London Medical Gazette.

Sackville-Street, August 25, 1831.

SIR,

I WAS a good deal surprised at Dr. Wilson Philip's observations on my lectures, in your No. for Aug. 20, which, as far as I am concerned, appear to be founded on mistake from beginning to end. Since these observations were published I have seen the Doctor, and succeeded, I believe, in convincing him of his errors—on some points at least; but as the observations have been made public, they require a public answer.

In the introductory observations to the first lecture, it is observed that almost all our reasonings on physiological subjects are either of a mechanical or *metaphysical* nature. Now it need scarcely be observed that the term *metaphysical* is here used in a general sense, and as opposed to *physical** ; but, to prevent the possibility of mistake on this point, a few lines below, the nervous or living principle, or rather action, is expressly included under this term. Near the termination of the same lecture, there is a paragraph which Dr. P. quotes at length, in which the same term is employed in a similar sense. The substance of this paragraph is, that chemistry may be considered as holding an intermediate rank between anatomy, on the one hand, and metaphysics, or living action, on the other; and that, while these two extreme branches of knowledge have been cultivated to the utmost, comparatively little has been done in chemistry (the expressly assumed *intermediate science*) for the last twenty years.

With all this before him, however, Dr. P. most unaccountably mistakes my meaning, and observes, upon the quotation above alluded to, "the questions which this passage suggests are, how far does chemistry fill the intermediate space between metaphysics, *the science of mind*, and anatomy, to which we owe a knowledge of the structure of the body; and *has, in fact, no advancement of any importance been made in this intermediate space for the last twenty years?*"

Dr. P. then proceeds to consider these two questions in detail; but, as might be supposed under such circumstances, most of his observations are irrelevant, and do not require an answer. There are, however, a few points incidentally touched upon, which I think it right to notice briefly.

1. Dr. P. remarks, upon the assumption of the living agent as an ultimate principle, the nature of which is beyond our comprehension, that this may be "a very scientific, and perhaps a just, explanation, but one which, from the nature of things, cannot be brought to the test of experiment, and which, be

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* I need scarcely observe that the term *physics*, almost everywhere but in this country, is used to designate what we term *mechanical* or *natural philosophy*; while *metaphysics*, "the science which considers beings abstracted from all matter," may be considered, in this general sense, as contrasted with or opposed to it. The term *psychology*, was meant to be contrasted, in the same general manner, with *anatomy*—in the sense of *inanimate* and *animate*, or *corporeal* and *incorporeal*. After the lectures had been printed, I regretted that I had used these terms, thinking it probable they might give origin to some verbal dispute; though I scarcely thought that the sense, whether right or wrong, in which they were used, could have been mistaken. Indeed, Dr. P. evidently saw the sense in which these terms were used, and had he confined his criticisms to this point I should have thought nothing about the matter, as the subject, I admit, is fairly open to criticism.

it observed, on the supposition of its being correct, includes *a principle that operates only in the functions of organized bodies, and whose effects we can never even hope to see exemplified in unorganized nature.*" To which I reply, that it is not in general necessary to the understanding of a process, that we should be able to comprehend the abstract nature of the agent performing it, or to imitate the process ourselves artificially. Even in common chemistry, how little are we able to do in this way! What chemist, for example, professes to understand the abstract nature of the cause of chemical action, or will undertake to imitate exactly even the commonest mineral?—yet he does not doubt its composition on this account. So in organic chemistry, though I cannot imitate nature's processes, this does not prevent me from understanding them. I cannot, for example, make sugar or albumen; but analysis and observation will enable me to understand the composition and laws of change of these substances. If nothing were admitted in chemistry but what can be proved synthetically, the science would, indeed, be contracted within narrow limits.

2. Dr. P in attempting to explain the effects of inflammation chiefly on mechanical and metaphysical principles, concludes, "I therefore conceive, that here the laws by which the increased quantity of blood in the vessels, however occasioned, is capable of producing the effects just detailed, and on which the relief obtained by lessening it depends, *are those which chiefly demand the attention of the physician*" The mechanical expedients of bleeding and purging are, and ever must be, no doubt, the means to be chiefly trusted to in inflammatory affecions; but notwithstanding this, I never could bring my mind to believe that the effects of inflammation are merely of a mechanical nature. Admitting them, however, to be so, how many diseases are there which are *not* inflammatory! and in which, consequently, something more than mere mechanical expedients demand, or ought to demand, the attention of the physician: now in such affections it is that chemistry, perhaps more frequently than any other science, steps in to his aid, and enables him to detect derangements, and apply remedies, that no mechanical or metaphysical knowledge would have suggested to him? for so far from chemistry being principally limited to urinary diseases, as Dr. P. imagines, there is scarcely *any disease*, more especially of the assimilating functions, in which it may not occasionally be advantageously applied.

3. The alimentary matters most proper for a healthy human stomach are solids, or, at least, solids partially reduced by cooking; and this power of reducing solids to a fluid state, may consequently, be justly inferred as constituting a portion of the functions of this organ. But in some forms of dyspepsia this portion of the functions of the stomach is very much impaired, and in this case solids are very apt to disagree, by promoting acidity and all its consequences. In this state of the stomach, "when the reducing powers of the stomach are weak," I have particularly recommended the adoption of French cookery, in preference to a crude English diet of beef-steaks, &c.; but Dr. P., by some unaccountable mistake, makes me recommend this kind of diet generally in all affections of the stomach—a thing I never dreamt of. I believe, however, that a portion of this kind of aliment (not too fluid) is more generally proper than is at present supposed, particularly among the sedentary and dyspeptic inhabitants of towns.

4. I have said that crystallized sugar is, generally speaking, an unwholesome article of food; and I believe so still, particularly in certain forms of dyspepsia. Dr. P. admits that it sometimes offends the stomach, but contends generally for its wholesomeness. To this I reply, that the pernicious effects of sugar, like those of alcohol, are generally much less felt in the stomach than in other parts of the system. Thus a diabetic patient can generally take as much sugar as you please, and with impunity as far as the stomach is concerned; but notwithstanding this, will any one contend that sugar is proper food for him? Indeed, I must say, that, as far as my observations go, the stomach is often a very bad judge of what is right, and what is wrong, in matters of diet. Dr. P. concludes this part of his observations by triumphantly asking, whether these, and certain idiosyncrasies with respect to diet, &c., which he enumerates, can be explained by chemistry? to which I answer, no; but I will venture to assert, that they never will be explained *without* chemistry.

The second part of Dr. Philip's observations is entirely confined to his own discoveries, and which, by mistaking my meaning, he has thought it necessary to bring forward as examples of what has been recently done in physiology. With these I have nothing whatever to do, but shall merely observe that, as a chemist, I cannot help regretting that they had not been carried on in conjunction with this science; in which case some of the results would, I think, have been more perfect and satisfactory; for it has always appeared to me that the physiologist who confines his attention solely to the laws of living action, takes a very limited view of his subject, and is quite as likely to be led into error as the mere anatomist, or chemist, who neglects these laws altogether. The living principle is only known to us in conjunction with material organization; the study of physiology, therefore, has been very properly supposed to include the laws of living action, on the one hand, and anatomy, or the structure of organs, on the other; but as neither of these branches of inquiry gives us any insight into the nature of those important changes constantly going on in the living animal, and which appear to be necessary to its existence, something more is obviously required to render the science complete. This something, I conceive, is best supplied by chemistry; and my object in these lectures was, *not to depreciate*, much less to *supersede*, the mechanical and metaphysical branches of physiology, as Dr. P. seems to have supposed, but to point out the importance of chemistry, and its capability, *when rightly applied*, of filling up, to a certain extent, the intervening chasm between the other two branches of the science, and thus of rendering it more complete.

Your obedient servant,
Wm. Prout.

CHOLERA MORBUS.

To the Editor of the London Medical Gazette.

Sir,

* * * * * *

The first case to which I was called was a very severe one, and it yielded speedily to calomel, opium, and small doses of magnesia. Magnesia, calomel, and opium, seemed therefore to be the proper remedies, and for a short time I proceeded with satisfactory precision; but I soon found that the purging continued in some cases, despite of my infallible remedies; and then the chalk mixture, with aromatic confection, seemed a very prudent addition. Suddenly, however, aromatic confection, chalk mixture, opium, calomel, and magnesia, entirely failed, and I was driven back upon my own resources. I called to mind the cajeputi oil, which I administered with marked and determinate relief. The cajeputi oil next failed to allay the vomiting, which was decisively checked by a mineral acid; and I lastly discovered that a mineral acid, with opium, was a very speedy and efficient remedy in the milder forms of the disease. After these failures and successes the conclusion at which I have arrived is, that, in the milder cases, magnesia is a sedative, by acting as a chemical substance in the neutralization of some unnatural acidity; that, in the severer cases, cajeputi oil, or a mineral acid, is efficacious by acting as a powerful styptic and a non-stimulating tonic; and that opium is a useful and necessary adjunct under all circumstances, by soothing the morbid irritability of the stomach. The kind of practice, and the particular remedy, can never be predetermined in the treatment of any complaint; for the mode of treatment must be judiciously varied, according to the age, sex, and constitution of the patient, and the intensity of the diseased action. I do not think that it would be wise to check the discharges abruptly in the milder cases, since any evacuation established by nature may be salutary, so long as it be not excessive. It is, however, probable that if the disease were permitted to pursue its own course, the patient would die of exhaustion, and that the mucous membrane of the intestines might, possibly, be found spacelated.

Of the cause of cholera morbus many conjectures may be framed. A state of atmosphere at once humid and of an uniformly elevated temperature, would lessen the quantity of cutaneous perspiration and pulmonary exhalation, while, by interfering with two important excretions, it would dilate and disturb the whole circulating system, and throw a larger quantity of fluid to be excreted by the kidneys, or by the bowels; and if the kidneys refused to act, the bowels would become the only remaining outlet: moreover, if any sudden electrical changes should happen at the same time, the body would be also debilitated and relaxed. I will not decide whether such have been the atmospherical changes during the last six or eight weeks, of which I have made some careful notes; for this conjecture concerning the cause of cholera morbus has been offered, and it is to be accepted, only as a probable conjecture. It would be requisite to institute very extensive observations, subservient to pathology, throughout England, and throughout all tracks of the world from Hindostan to Russia, whithersoever the cholera has travelled, before any definite opinion could be offered upon the subject; and it may be reserved for a future generation of men to recognize the existence of an universal principle, which can regulate the origin, progress, duration, and violence of disease, in all its protèan forms and combinations. An extensive region lies open and uninvestigated before the diligent physician; but its boundaries and dispositions will never be known by gathering particular facts and single observations, instead of endeavouring to explore its universal character. And here is the error under which our science now labours: medical men, like other philosophers of the present day, are lost in a crowd of infinite particulars. The true spirit of philosophizing is generalization; and generalization, however false, will ultimately lead to a generalization absolutely true. Wherefore, without some universal principle to act upon, it will be very difficult to come to any conclusion as to the morbific influence of atmospherical changes.

All that has been here written is perhaps concerning a disease very different from the *true* cholera morbus; nevertheless, I am conscious of having copied from nature; and if the picture be faulty, the fault lies, not with the object, but in him who copied.

Let us, for a few moments, revert to the writings of Sydenham, the patriarch of English physicians, and ascertain what were his opinions concerning this disease. It happened in the year 1669, and had been absent from England for ten years; it acceded towards the close of summer, in the month of August, and was characterized by the following symptoms: vomitings, purgings, and tenesmus; thirst, heat of the stomach, and anxiety; a small rapid pulse, faintings, sweatings, "contractions of the arms and legs," prostration; it was sometimes fatal in twenty-four hours. This was not the same disease as that which has now invaded our island. The present disease is not marked by tenesmus, nor by a quick pulse, nor by "contractions" of the arms; Sydenham speaks indefinitely of the cramps, and is almost silent concerning the profuse fluid evacuations. He seems to have possessed no certain treatment, for he was afraid of purgatives, and unwilling to make use of astringents, until after much solemn consideration he at last arrived at a very sapient mode of practice. "I found out this method," he says, "several years ago, and have long experienced it; and have by it many times reduced the disease to good order." A poor chicken was boiled in three gallons of water, "so that the liquor had scarcely any relish of the chick." With this liquor the patient was drenched, and a plentiful inundation was made of draughts and clysters, "until all the broth was consumed, and then evacuated upwards and downwards." Some syrup, made from a pretty nosegay of lettuces, purcelain, violets, and water-lilies, was occasionally added, "though," as Sydenham ingenuously confesses, "the broth will do very well without such addition." This singular operation entertained the patient, the physician, and probably a decent circle of friends and relations, for three or four hours. At length the chick defeated the cholera; the patient was alleviated; and an appropriate dose of opium composed the agitation—the "hurly-burly" (so says the translator of Sydendam's writings,) of the abdominal viscera. (See Sydenham's works, translated by J. Pechey, M.D. London, 1731. Cap. ii. p. 114.) If the patient was unseen or neglected during the first six or twelve hours, or the physician did not arrive till "the sick was worn out, so that the extreme parts waxed cold," then the only means which could save the patient was an anodyne; and it may be here remarked, that Sydenham seems to have been very ready, and, according to his own account, successful in the exhibition of laudanum. Some practitioners dissent from the frequent use of opiates, while others as frequently rely upon their efficacy; but the truth is, that many a drug is like Orlando's trusty blade, which required Orlando's arm to wield it: in the hands of an unskilful possessor it might become either dangerous or useless. And hence it is that each man is so fond of his own remedy, and that different practitioners accomplish the cure of the same disease through the medium of different remedies. Individual dexterity depends upon experience and habit.

And so much for the cholera morbus of 1669 and 1831; and so much for my opinions, which, like the opinions of many other men,* may be fit only for Astolfo's palace in the moon.

I think it but right to put you in possession of the three following cases, of which you can make what use you please.

Case I.—August 4th I was called to Mrs. J. H——, a female of a tall spare condition of body, and of active habits, upwards of forty years of age. Last night, about twelve o'clock, she was roused by pains all over her limbs, and cramp in the belly, speedily followed by vomitings and successive alvine discharges profuse and fluid. These evacuations I did not see. About nine o'clock, A.M. I was sent for, and found her pale, insensible, covered with a damp cold sweat, the lips blanched, and the veins on the back of the hands collapsed; the pulse weak and little, and the respiration low. She had just vomited something dark, like coffee, and the bowels had just been copiously evacuated. The belly was tense and painful midway between the navel and ensiform cartilage; the pain was not increased by pressure. The tongue was quite clean, smooth, and moist. There was painful micturition. She was stretched horizontally on the carpet till a convenient bed could be prepared; hot bottles were placed against her feet, and the whole body was enveloped in a blanket. She had just swallowed a dose of castor-oil. I gave her one grain of opium and five of calomel. She became easier, but she remained cold, with a dull pain over the stomach. A draught composed of aromatic spirit of ammonia, sweet spirit of nitre, and tincture of opium, was given. The draught felt hot in the stomach, the veins at the back of the hands became full, and the radial pulse dilated; but the peristaltic action of the bowels seemed to be increased by this diffusive stimulant, and the stomach to be offended at the draught, which, together with the castor oil, was soon rejected. Towards noon she became hot and flushed, and the pulse became round and full, at 80. She complained of much thirst, and the tongue was clean, but dry; the stomach was easy, and the bowels quiescent. One drachm of the carbonate of magnesia was given every four hours, and toast and water was supplied as the readiest and most acceptable beverage. In the evening she drank and relished her tea; and at ten o'clock in the evening, rather more than twelve hours after my first visit, she was in bed, with a moist and warm skin of its natural colour, red lips, clear eyes, clean and moist tongue, easy stomach, quiet bowels, and a soft steady pulse, 72. She was now lying on her left side. During the night the bowels quietly relieved themselves of a small quantity of yellow fluid, of a feculent odour; which evacuation afforded much relief and satisfaction. She made no urine. She partook of beef-tea, and continued the magnesia every six hours. From this time she recovered; the bowels quietly returned to their duty, and in a few days she was quite well, except that she suffered great lassitude, which was counteracted by regulated doses of the dilute sulphuric acid.

The tongue is said to have been clean and *moist*. The moisture was not that of health; it should be said that the tongue was *not* dry. The mouth and fauces were in this case deprived of fluid, and the patient craved to have them moistened.

Case II. August 12th.—A gentleman, 63 years of age, of a florid sanguineous temperament, very temperate in his habits, and very attentive to the condition of his bowels. Yesterday his abdomen felt full and distended, and the bowels were somewhat relaxed; in the evening he was exposed to a heavy rain, after a hot sultry day, the thermometer standing at 79 degrees, and the wind being from the north-west. When he went to bed he felt bilious, and took a calomel pill; about two o'clock, A.M. he awoke, and felt a desire to go to stool. He arose, staggered, sank, and fainted. He parted with a copious fluid stool, being at the same time bedewed with a cold clammy sweat. There was neither griping nor tenesmus; the desire to go to stool was gentle, though imperative, and the fluid streamed out without an effort. The evacuation was of an earthy smell, and seemed like water of a light straw-colour. He crawled into bed, and was shortly after seized with cramps in both legs; he started up and vomited, while another copious fluid evacuation quickly washed out the alimentary canal.

I found him (3 A.M.) faint, with a feeble voice, stretched upon his back in bed, the bowels threatening to be again discharged. I instantly gave him a grain of opium and one scruple of the carbonate of magnesia. He became easier, and the pulse, which was lowered by fainting, rose to its natural standard. The violence of the symptoms was at once mitigated; the bowels continued to drain, but without fainting, and the vomiting returned, but it was checked by the magnesia. The magnesia seemed to act like a sedative—perhaps by neutralizing some acid generated in the stomach, and in this manner soothing the irritated nerves. The vomit smelt and tasted acid, and was yellow and turbid. The tongue was white, and scarcely moist; inordinate thirst prevailed, and he was permitted to quench it by draughts of veal broth. During the day he took a second grain of opium, and continued the magnesia every four hours. In the evening the tongue was beginning to clean, the stomach and bowels were passive, he took a third grain of opium, and slept. The pain over the pit of stomach had not gone; it was wearisome, but not severe.

Some pints, nay quarts, of fluid must have been parted with, the last quantities of which were colourless.

The next morning I found my patient much improved; he had passed a tranquil night, but he rose up weak and thin, the features being visibly sharpened, and the complexion being of a dull blueish hue. He was directed to persist in the use of the magnesia, and to partake of veal broth and of bread and milk. The bowels were at first distended with gas, which was discharged in the manner and with the inclination of passing the fæces, and then they kindly returned to their duty.

For the three following days he made scarcely any urine, and the thirst was still great. On the fourth and fifth day all the functions of the body seemed healthy, but they were proceeding at a lower gradation of health. Apathy of mind and body ensued. This gentleman is now quite well, and considers himself by no means the worse after so great a drainage.

I now fancy that I might have cured this case expeditiously by the use of the cajeputi oil; as it was, my treatment was merely palliative, and only prevented the disease from becoming excessive.

CASE III.—A poor old woman, aged 72, had been labouring under a bowel complaint, but, as she was in poverty, she was unwilling to send for me. To-day, August 22d, she was at last obliged to send, saying that she had vomiting, purging, and sweating. I sent five grains of the soap-pill with opium, to be taken immediately, and half a drachm of the carbonate of magnesia as often as she was sick. About one o'clock, as soon as I reached home, I received another message, to say she was much worse. I went and found her pale, cold, covered with a chilly dampness, faint, and prostrate, but quite conscious; the pulse was steady at 80, rather contracted; the tongue clean and smooth. She was suffering terribly from cramp in her legs; she was being purged of an opalescent fluid, which poured from her in profuse quantities, and she was vomiting up a similar fluid. Her stomach, she said, felt as cold as water; she complained of a most oppressive pain above the navel, and she laid her arms across her stomach. Three grains of calomel, with two of opium, were given directly, and the chalk mixture with aromatic confection was administered. One hour afterwards she was much worse. The countenance was of a dingy hue, the eye sunk in the socket, the features contracted; the skin was very cold, and the cramps were fearful; nevertheless the pulse continued steadily at 80, but its volume diminished. She vomited and was purged while I was talking to her. I hesitated, being at a loss what to do. I hastened and brought back some cajeputi oil, of which I measured out twenty drops in a glass of water: she drank it. "That," she said, "is warm to my stomach." At the next minute she vomited up some yellow turbid fluid, was purged, and distorted by the cramp. Ten minutes after, the cramps subsided and the action of the bowels was restrained; in twenty minutes the cramps were returning, the bowels beginning to be active, and the stomach to heave: she swallowed twelve drops more. The cramps now ceased, and the bowels became quiescent; the pulse became fuller, and the skin warmer. At five o'clock I saw her again; she had been vomiting almost incessantly during my absence, and she had taken a third dose of twelve drops of the cajeputi oil, which had been immediately rejected; but the cramps were gone, the bowels were quiet, the pulse was steady; and as she was at present free from vomiting, I thought it imprudent to be doing too much. Her breath was strongly scented with the oil, and so were the recent vomits, amounting to about a pint of yellowish fluid. Her mouth was moistened with beef-tea, of which she occasionally sipped a small quantity. The vomiting, however, returned, and continued at intervals till ten o'clock at night, when some half-drachm doses of the dilute sulphuric acid immediately and permanently checked it. She fell asleep, and the next morning she was quite free from any unpleasant symptoms, except great debility of mind and body—sheer exhaustion. No urine passed. Her situation is still doubtful, for at 72 years of age life easily becomes extinct.

JAMES ANSLEY HINGESTON.

Finsbury Place, South,
Aug. 25, 1831.

[*Note.*—In my letter on the atmospherical changes during the late influenza, which you did me the favour to publish, there is a slight mistake:—" the *frosty* air-burnt *froze.*" This is wrong. It ought to be, " the *parching* air-burnt *frore,* and cold performed the effect of fire "—***Paradise Lost,*** book ii. line 595. I quoted from my memory.]

CONTINUED REMARKS

ON THE

USE OF CAMPHOR AS A LOCAL APPLICATION IN SOME DISEASES.

To the Editor of the London Medical Gazette.

SIR,

IN my last communication to you, I concluded by stating the convalescence of my patient, who had been suffering from what is generally denominated puerperal fever. When I wrote to you, she had occasionally complained of uneasiness in the left leg, and in the region of the uterus. Engrossed perhaps by what had been previously passing, I entertained no suspicions of any consequent mischief; but on the morning of the 16th, she was suddenly seized with excruciating pain in the calf of the left leg, which quickly extended itself over the whole limb, affecting even the contents of the pelvis. There was great tenderness on pressure over the os pubis; the bowels had not been relieved for some hours. I ordered a dose of castor oil to be administered. In the evening, when I called, I found the limb exquisitely tender, swelled, and œdematous around the ankle and the instep, on the calf, and on the inside of the thigh and groin, and the whole leg perfectly blanched; nor was this appearance confined to the limb; it was to be observed more or less over the whole surface of the body; great anxiety of countenance; pulse 120, and very weak; she had fainted four times in the course of the day from the agony which slight exertions produced; the oil had operated three times. I ordered five grains of the Pil. Sap. c. Opio to be given immediately, with two grains of the Sulphate of Quinine; the Quinine to be continued every four hours, and the limb to be enveloped in a flannel roller, steeped in the camphorated solution that was applied to the abdomen in the previous attack*. When I saw her on the morning of the 17th, she described the relief as almost immediate on the application of the roller; she fell asleep before the opiate reached her; however, it was taken afterwards, and the medicine administered through the night. Five hours sleep were procured. The embrocation was reapplied; the quinine continued; gruel and brandy, with beef-tea, comprised her nourishment. In the evening it was necessary to relieve the bladder, that was distended with urine; and pills, composed of four grains of Pil. Sap. c. Opio, with eight of Colocynth, were administered.

18th.—Passed an excellent night; pulse 90; no increase of tumefaction about the limb, but complains of the calf of the opposite leg, which is excessively tender on pressure. I continued my constitutional remedies, and applied the embrocation to both limbs. In the evening I found the bowels had been very irritable; eight or nine motions; in fact, the bowels acting every time that nourishment was taken. It was necessary to use the catheter. The following draught was ordered. At this time she could move the limbs with perfect freedom; they were still sensible to pressure, and the left leg still œdematous about the ankle. The catheter again used.

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* Dr. Denman, in his chapter on the swelled leg, in his Treatise on Midwifery, recommends an embrocation composed of camphor, one drachm, olive oil, one ounce.

℞ Confect. Cardiac, ℈j.
Confect. Opiat. ℈j.
Tr. Card, Comp. ʒj.
Aq. Cinnam. ℥x. M. ft. haust. statim sumend.

19th.—Slept the greater part of the night; complains very little of pain; has perfect command of the murcles of the limbs; pressure still gives uneasiness, and the left ankle is still œdematous. The catheter again employed night and morning.

℞ Liq. Potass, ʒj.
Conf. Cardiac ℈iv.
Sp. Ether Nit. ʒij.
Decoct. Cinchon. ℥vss. M. ft. Mist. capt. 4tam part. 6tis horis.
Pil Sap. c. Opio, gr. v. h.s s.

She has continued daily to improve. On the 25th she was removed to the workhouse in a chair; and on the 27th, dissatisfied with her situation, she walked to her residence (distant about a quarter of a mile) without much difficulty, nor has the exertion injured her in the least.—I am, sir,

Your obedient servant,
HENRY GEORGE.

22, Lower Phillmore-Place,
Kensington.

CHOLERA.

To the Editor of the London Medical Gazette.

Minster-Yard, August 27th, 1831.

SIR,

As it seems to be the present fashion to report cases of cholera, I shall feel obliged by your inserting the subjoined. In the present excited state of the public mind upon this disease, and when there is so much desire to run after new remedies, I think it only fair to our *old friends*, that they should not be entirely neglected.

Your obedient servant,
H. S. BELCOMBE, M.D.

August 19th.—A young gentleman, who had fatigued himself by much travelling during the hot weather, and had perhaps been a little careless in his diet, though I could not get him to acknowledge it, was seized, at five this morning, with spasms at the stomach, followed by copious vomiting and purging of bilious acrid fluid. I saw him at nine, when he had taken two grains of opium and some camphor and æther. At that time the vomiting and purging were excessive; there were excruciating spasms of the whole frame, and the pulse imperceptible. Every thing was rejected from the stomach *except the opium pills*, which were therefore given in doses of two grains every hour. At eleven the vomiting was a little allayed, and the pulse had risen. The warm bath was now ordered, and seemed to have considerable effect in mitigating the spasms. At two there was further improvement; the pills were then given every two hours, and the immersion repeated. In the evening he felt much better, and passed a stool of some consistence.

20th.—A good night, and the only unpleasant feeling is that of excessive soreness. Towards evening a slight purging came on, which was quickly relieved by calomel and rhubarb.

ANALYSES & NOTICES OF BOOKS.

> "L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

A Treatise on the Means of Preserving Health, and particularly the Prevention of Organic Diseases. By A. T. W. PHILIP, M.D. F.R.S. L. and E. Part II.

(Concluded from page 692.)

THE first part of this Treatise, comprising the nature and symptoms of those derangements which precede change of structure, was reviewed in our last number. We are now to lay before our readers an account of the second and last part, which treats of the means of correcting the states which precede organic disease, and counteracting their effects.

It appears from what is said in the first part, that organic disease is preceded by permanent disorder of function, and permanent disorder of function by that which is occasional; the subject of the present part of the treatise, therefore, divides itself into three heads—the means of preventing occasional being changed into permanent disorder of function, those of preventing permanent disorder of function producing change of structure, and when change of structure has taken place, the means of correcting it or retarding its progress. We have also seen that when the quantity of circulating fluids is too great, in proportion to the powers of the system, and the properties of the blood, (as necessarily happens as well from the causes which produce this state as from its continuance) vitiated, a general tendency to derangement of function is the consequence, and therefore a state of plethora may be regarded as a predisposing cause of chronic functional derangement in particular organs. Acute disease is a still more fruitful source of such derangement. The author, therefore, before entering on the treatment of the functional derangement of particular organs, makes some observations on the means of correcting a plethoric state of the habit, and on the principles of treatment in acute diseases which tend to prevent their terminating in chronic local derangements. As our space is limited, we shall pass over these preliminary dissertations, and enter immediately on what he says of the affections of individual organs. In the third chapter of this part of the Treatise, he considers the treatment of the functional derangement of the brain which precedes change of structure. Here the author is on ground which may be considered as altogether new; for, referring for the treatment of the acute diseases of the brain to what has been said of the principles of treatment in acute diseases in general, he confines himself to a consideration of those chronic cases which have been confounded with what are called nervous complaints, and for which, consequently, no appropriate plan of treatment has hitherto been proposed. It has been known to physicians that nervous affections sometimes terminate in organic diseases, but we are not aware that such cases have been distinguished from the more common forms of these affections, and still less that any appropriate plan of treatment has been proposed for them.

In the first part of this chapter the author points out, that as the functions of the brain are, with the exception of the circulation, those of the whole system, its functions may be deranged without any marked local symptoms: and although the derangement is slight in each particular part of the body, in the brain, which feels the derangement of every part, we can easily understand why the effect should be, as, in fact, we often find it is, attended with the most serious effects. In our review of the first part of the Treatise we have given, as fully as our limits admitted of, the author's diagnosis of such cases—the means by which they may be distinguished from cases of common nervous affections. We are now to consider the plan of treatment which, he informs us, he has frequently found successful in them, after they had defied all the usual means. The treatment of common nervous affections, indeed, either makes no impression on them, or at most gives only partial and momentary relief. "From the case before us," the author observes, "having been confounded with what are called nervous complaints, no appropriate treatment has been attempted in it; and thus it is that it so frequently ends in a fatal disease. I could adduce many cases of this kind, in which the means I am about to point out have succeeded in restoring permanent health, when all others employed for years had failed; and when there was every reason to believe that organic disease of the brain must otherwise have ensued."

In laying down plans of treatment, the first object is to remove the occasional causes and prevent their re-application. It appears, from what has been said of the case before us, that all derangement of function, whether of mind or body, may be considered as a cause as well as a symptom of the disease, for all other derangements tend to derange the brain. The indication, therefore, is to restore the healthy state of all the functions. The various means employed in common nervous complaints are proper here, but they are not equally effectual, for the disease, we have seen, instead of being variable and easily influenced, is, compared with common nervous affections, uniform and obstinate; and we should, therefore, have little hope of frequent success if we were not possessed of means better suited to its nature.'

If the *morbid* state of mind is relieved—that is, if one, and that a very important set of the functions of the brain are rendered more natural, a certain step is gained; for we have no means of counteracting the tendency to organic disease but those of restoring the due functions of the part;—but the mental are neither the most constant nor influential functions of the brain, in a medical point of view. The various digestive and assimilating processes are those which chiefly demand attention where organic disease of this organ is threatened. These are its important, because its most constant, functions. They are those also on which its own structure, as well as that of every other part, depends.

It is in the capillary vessels and extremities of the nerves that all the great changes of our frame take place; and it is only by influencing these changes that we can hope to counteract a tendency to organic disease. "In attempting to relieve the disease before us by medicines," the author observes, "we are not to look for such as powerfully affect any particular part, but gently affect the whole. As the animal body cannot exist under an affection at once both general and powerful, it is not with a violent but extended deviation from health that we have to contend, and which, from the sympathy of the various parts, is obstinate in proportion as it is extended.

We possess two medicines, in large doses, capable of the most powerful effects on individual parts, when given with a view to affect them; and, in small doses, of the most gentle and universal operation when given with a view to affect the whole—mercury and antimony. By these medicines properly administered, we can, without any sensible effect, excite all the secreting surfaces, and often in the case before us, if it is not rendered obstinate either by the great length of time it has continued, or from its depending on a local cause over which we have no power, gradually restore the healthy state of the system, and consequently that of the functions of the brain, when all other means which our art affords have failed. I say this with confidence, because I have repeatedly seen them successful under such circumstances."

According to the author's experience, many circumstances are to be attended to in their employment in the disease we are considering, and other medicines occasionally administered; for, although no other can be substituted for them, there are many which occasionally aid their effects. He particularly insists on the necessity of employing very small and frequently-repeated doses, that, on the one hand, no increase of debility may result from their use, and on the other, the system be kept constantly under their influence. Those, he observes, who have never seen their effects, would smile at such doses as he recommends; but this, he maintains, arises from their being unacquainted both with the nature of the disease and the effects of the medicine.

"Let such objectors," says Dr. Philip, "recollect that our view in the case before us, is to restore the functions of the brain in a disease which is formidable, not from the degree in which any of them are deranged, but from their being all deranged, and constantly so. If any one particular set become more deranged than the rest, which we have seen in protracted cases is apt to happen, a more vigorous treatment, adapted to the greater derangement, must be resorted to, as far as the strength can bear it; but vigorous means, where no great degree of derangement exists, is the surest means of inducing it. The disease is mild in its symptoms, but constant and steady in its progress. The plan of treatment which is opposed to it must have the same characters. After the more severe states have commenced, our means must be proportioned to them, however little hope of relief may remain."

'The author has found mercurials, on the whole, more effectual in restoring the due action of the organs of assimilation than antimonials. He therefore dwells chiefly on the former, at the same time pointing out the states in which antimonials, either alone or in combination with mercurials, have been found beneficial. Our space does not allow us to follow the author throughout his observations on either of these medicines, but we shall, as far as our limits admit, follow his account of the circumstances which influence the employment of the mercurial.

"In considering the mode of conducting such a course," he observes, "I shall, in the first place, point out those effects of the plan itself which oppose its beneficial operation, and then the causes arising from the nature of the disease which have the same tendency; for even this plan is not always free from inconvenience, and sometimes such as is not easily removed. On the other hand, the patient frequently experiences so great a degree of relief even from its immediate effects, that in many instances it has been difficult to prevent his employing it both more constantly and more freely than I judged proper.

"It sometimes happens that mercurial medicines, even in the smallest doses, irritate the bowels; and such is the injurious effects of this irritation, that, unless it can be allayed, the alterative, at whatever expense, must be abandoned; for the disease will bear no serious cause of continued irritation, and in the use of opiates we are greatly restricted. It is only in the smallest doses that they are admissible. The henbane is the best in the case before us, but, like the rest, is often not sufficiently powerful in any dose that is not injurious. If neither this nor any other opiate, in small doses, will answer the purpose, we have no resource but still lessening the dose of the alterative and increasing the interval at which it is given; and if this plan will not leave such a dose as is still capable of making some impression on the disease, the medicine must be laid aside; and then, as far as I know, we have little more but antimonials and palliatives to trust to. The various means which relieve the more common nervous symptoms, although they have a certain effect, afford only very imperfect and temporary relief.

"I have mentioned the eighth part of a grain of blue pill, taken three times a-day, as a dose often attended with very sensible good effects. I believe, that, in many constitutions, even smaller and less frequent doses may be of service.

"The largest quantity I ever give, with a view merely to its alterative effect, is half a grain of blue pill three times a-day in chronic, and four times in acute diseases; and this only when the derangement particularly affects the liver. Under certain circumstances, and particularly, as frequently happens, when acute diseases are attended with, and in some degree supported by, a disordered state of the liver, I have found such an addition to the usual means render them both more certainly and quickly efficacious.

"We sometimes, but not always, derive advantage in irritation of the bowels from the alterative, by changing the blue pill for the *hydrargyrum cum creta*. This preparation is, I think, about one-half the strength of the blue pill; but it is by no means so effectual an alterative, whatever be the dose, being more apt to oppress the digestive organs. In the plan I am speaking of the gums are never allowed to become affected: there is no occasion for so considerable an effect of the alterative; and any thing like salivation must always do harm, where all causes of irritation are particularly injurious.

"Many will be surprised, considering the smallness of the dose, to learn that it is necessary to guard against such an effect: but it is more apt to take place than would easily be believed by judging in this way"

The cause of such a plan sometimes producing a greater effect on the gums than larger doses, is, that the latter are more apt to excite the excretories, by which they are thrown off. It is to the general, steady, and gentle impression that the frequently-repeated small doses make, thus throughout the system supporting an excitement similar to the healthy action, that, the author observes, they owe an efficacy that often surprises those who have not been accustomed to see their effects; which being supported for a certain length of time, are confirmed by habit, and at length go on without the aid of medicine. But it is always a necessary precaution to lay aside the medicine gradually; if it be done too suddenly, the inactivity of the surfaces is apt to return.

The chief circumstances in the disease itself, which oppose the beneficial operation of the alterative, are a great degree of debility induced by its continuance, the inflammatory tendency arising from the same cause, and the determination of the symptoms to particular parts. Our limits will not allow us to follow the author in his observations on each of these heads.

The inflammatory tendency of the disease greatly circumscribes the use of tonics, and the author lays down many rules which must be attended to in their exhibition, otherwise they may prove more hurtful than beneficial. The inflammatory tendency is rather to be subdued by antimonial and saline medicines than by evacuants, where the strength is already much reduced by the disease; and for the treatment when local determinations take place, the reader is referred to what is said in the following chapters.

It is generally of service to combine the alterative with small doses of quieting medicine, except where there is much tendency to pain of the head, or drowsiness. The henbane has been found the best, and, when the nervous irritation is great, the author has found no means of allaying it so effectually as a combination of henbane and antimony. The great objects of the treatment are, to allay irritation, and support, as far as possible, a regular state of the functions. "In short," the author observes, "the principle of the treatment here being to support the action of the extreme vessels and restore the vigour of the nerves, too much or too little general excitement is injurious. We must assist the constitution to maintain that moderate degree which alone is consistent with health, and recollect we have no other means of restoring the brain but that of regulating the functions of the whole system; for, with the exception of the circulation, all are functions of that organ."

The author has not found local measures so effectual in the affection of the brain we are considering, as they usually are in other local diseases. The shower-bath he has found the most useful; cold, when the temperature either of the head or the system in general is increased, and tepid—the temperature being regulated by the feelings of the patient,—when this is not the case. Blisters and issues in the neighbourhood of the head in general, he found of little use, although the former is occasionally of service if it does not excite much irritation, which is always injurious.

In plethoric habits issues are often of use, but seem nearly as effectual in the case before us when established elsewhere, as near the head.

The state of the brain we have been considering is frequently sympathetic of other diseases, and then the cure depends in a great degree on that of the primary affection. The most favourable of such secondary cases the author has found to be, that depending on long-continued indigestion, which not very unfrequently terminates in organic disease of the brain. Unless change of structure has actually taken place, these cases are generally effectually relieved by restoring a more healthy state of the digestive organs.

As the great cause of organic disease is the continuance of disordered function, and the great corrector of a tendency to it, maintaining the healthy actions of the part, the author was induced, from the result of many experiments to which we have referred, to try the effects of galvanism in those obstinate cases of nervous debility which threaten permanent disease of the brain, and he has seen it successful where all other means had failed. For its effects, and the circumstances to be attended to in its employment, we must refer to the Treatise.

In chapter fourth the author considers the treatment of the functional derangement of the heart, which occasionally precedes organic disease of this organ. It arises, we have seen, either from sympathy with diseased states of other organs, or from morbid irritability of the heart itself. The former can only be effectually relieved by relieving the original disease. The latter is relieved by nervous medicines, and the various means which tend to strengthen the general habit, on the morbid irritability of which that of the heart generally depends. Into these means the author enters at considerable length.

The subject of the following chapter is one of still greater interest—the treatment of the functional derangement of the lungs, which precedes change of structure in them. Pulmonary consumption is the most important organic derangement of the lungs: for his opinions of the various forms, both idiopathic and symptomatic, of this disease, the author refers to his treatises on symptomatic fevers and indigestion, and here confines himself to the treatment of those insidious beginnings, which may rather be regarded as indicating a tendency to the disease, than forming a part of it.

"I have said," he observes, "that even a tendency to cough, and that not frequent, and more than usual hurry of breathing on exercise, are symptoms requiring serious attention in those highly predisposed to pulmonary consumption. Of this truth it is difficult to convince those who are unacquainted with the disease. The existence of bilious complaints in any of their various forms, is not a less serious affection in such habits. In them it is not safe to wait till the bilious affection has produced a cough."

When the slight pulmonary symptoms just mentioned appear in an individual of a consumptive family, and at the consumptive age, the first thing to be determined is, whether he labours under disease of any of those organs which chiefly sympathize with the lungs. When it is ascertained that this is not the case, the whole attention must be directed to the lungs, in order to correct this first deviation from their healthy state.

Strong exercise, and all causes of taking cold, should be carefully avoided; the diet should be so regulated as in no degree to oppress the stomach; the patient should be much in the open air, under such exercise as does not in any degree hurry the breathing or cause fatigue, and he should take at regular times such a dose of quieting medicine, (a combination of extract of poppies and digitalis, the author has found the best,) as allays the tendency to cough, care being taken to obviate any constipating effect it may have.

"In short," he observes, "the principle is to bring the patient into the best possible state of health; to allay the irritation which excites the tendency to cough, and to avoid all those circumstances which tend to increase it; and the less the symptoms yield to these measures the more assiduous we should be in their employment. If they do not soon succeed, blistering of the chest is a necessary addition to them. When the tendency to cough is not immediately removed, the blister may for some time be kept open, or a succession of small blisters may be applied to different parts of the chest; and if there be any pain of the chest, or the pulse be in any degree tight, which often happens even at this early period, if the first blister does not succeed, the application of the second should be preceded by that of a few leeches.

The patient must not pause to compare the trouble of the treatment with the mildness of the symptoms. The mildness of the first symptoms is one of their greatest evils. He may be assured that the necessity of the means is proportioned to the trouble required to relieve him, and that the present trouble will probably save ten times as much afterwards, with this additional difference, that he is now at a distance from danger, he will then be on the brink of it, from which no trouble may be able to preserve him.

I am not an advocate for unnecessarily minute attention to health: many evils attend it. A pampered constitution is like a pampered child, ill able to contend with the unavoidable occurrences of life; for the more we are nursed the more delicate we become, and too much care, as well as too little, may spoil a good constitution; but of two evils we must choose the least, and too little care is a more pernicious folly than too much."

If such means do not check the disease at an early period, there can be no sanguine hopes of better success afterwards, for in pulmonary consumption, originating in the lungs themselves, we have only the same care, and the same means to look to, varied as the symptoms vary, with the exception of those necessary for combating particular symptoms, which, from their nature, are only palliative, and the chance of success is always the less the more the disease is advanced.

Dr. Philip asserts, that in the earliest stage, these means, if employed with care and assiduity, will seldom fail. It is after the disease has gained strength by habit, and the organization of the lungs begins to be more or less affected, that we so often find them vain.

When the disease has not originated in the lungs, especially if it has arisen from a disordered state of the digestive organs, which happens in a large proportion of the consumptive cases of this country, the author says he has seen few instances, even where the symptoms had become a little troublesome, in which they did not yield readily to a proper plan of treatment; for here at such a period, the pulmonary symptoms almost always disappear on bringing the digestive organs into the healthy state, and there are few diseases more under the influence of medicine than the usual affections of these organs.

Our limits do not allow us to follow the author in his observations on the manner in which the presence of the pulmonary disease, in such cases, should modify the treatment of that of the digestive organs, nor in his observations on the nature and treatment of the bronchial affection, which is so nearly allied to the more common form of pulmonary disease. He closes the chapter on the means of correcting the early stages of phthisis with the following observations. "Such are the means—few, simple, and of easy application, which never strain the constitution, and to which there is no possible objection, but that they require a little care and trouble at a time when it is not easy to convince the patient that either is necessary; by which the lives of thousands might be yearly saved in this country, and for the most part, at a time of life when they are only entering on its busy scenes, and the feelings of their friends are most deeply interested in them."

The sixth chapter treats of the functional derangement of the liver which precedes change of structure. There is no organ to which the observation made respecting the organic affections of the heart and stomach, that they are often preceded by no perceptible functional derangement, applies so little as to the liver. In this country at least its organic diseases are generally preceded by long-continued derangement of function, which makes itself apparent in all the usual symptoms that attend the more obstinate cases of indigestion.

Long-continued derangement of the liver is the parent of many of our most serious diseases, and the nature of those diseases readily explains to us that of the slighter affections of this organ. "We need not be surprised, for example," the author observes, "that the slighter affections of the liver should be attended with occasional headache, cough, and oppressed breathing, when we see that the continuance of its more severe and obstinate derangement is capable of at length producing apoplexy and pulmonary consumption." These consequences also afford the strongest argument for endeavouring to correct the first beginnings of its derangements.

The extensive influence of the liver, and its consequent effect on the progress of many diseases, leads the author in the present chapter to enter more fully into the laws of sympathy, on which the phenomena of disease so much depend, than in most other parts of his treatise. We shall, therefore, although we have already exceeded the space we usually devote to analyses, enter more fully into the contents of this than the preceding chapter, and with this, and some account of the mode of practice pursued when organic disease has actually taken place, our notice of the work must close.

Here it is still the object of the author to trace and correct the first beginnings of disease. The most temporary fit of disordered stomach, if at all severe, is often attended with a corresponding disorder of the liver, which is generally excited to pour out a greater quantity of bile, which, by its effects on the bowels, carries off the offending cause; and if the patient has not been subject to frequent attacks, with the cause, the consequences disappear. If the attacks are frequently repeated, the functions of the stomach begin to be more permanently affected, and then the biliary secretion never fails to become more or less habitually vitiated and irregular. These symptoms are for some time readily relieved by a better regulation of the diet and state of the bowels, and the attention is seldom arrested till the patient begins to experience some degree of permanent debility, and a greater difficulty than usual in fixing his attention.

He now finds more care necessary, and is generally surprised to learn that he can only be relieved by a good deal of trouble and some sacrifices. "If the evil day," the author observes, "be put off, and still only palliative measures pursued, the next warning is generally of a kind that must be attended to, and then for the most part the patient has some danger as well as trouble to encounter. These results, however, are not certain, or at least are often very distant; for in many cases slight disorder of the digestive organs will continue to recur for years without material increase; but they are sufficiently, and more than sufficiently common, to make it well worth while to attend to the beginnings of the evil;" and an important fact, the author adds, which he wishes particularly to impress on the reader's attention is, that the debility of the parts concerned, and consequently the obstinacy of the derangement, appears often to be proportioned rather to the length of time the symptoms have continued than to their severity; and when the debility has, by the operation of some accidental cause, and the sympathies of the liver affected a part more inclined to organic derangement than the liver itself, it is often found impossible to relieve the original in time to prevent the fatal effects of the secondary disease, this difficulty being not a little increased by the part last affected, although it has not previously suffered in such a degree as to command the attention, having been, in common with all other parts of the system, more or less prepared to suffer, in consequence of the general irritation so long kept up by the original disease. Under such circumstances, the patient often sinks with a rapidity that seems surprising when the whole of the circumstances, and the general laws of our frame, are not taken into account.

When a stricter attention to diet, regular exercise, and the state of the bowels, fail to relieve the stomach, it is necessary to assist these means by stomachic medicines, which, if no degree of permanent disorder of the liver be established, generally succeeds. "If, on the other hand, permanent disorder of the liver has supervened," which the author maintains may always be known by more or less of a distended state of the duodenum, "the means which only influence the liver through the stomach will generally fail without the aid of a mercurial alterative; for although it is not uncommon to employ mercurial medicines indiscriminately in cases of indigestion, we have reason to believe that while the permanent debility is confined to the stomach, they are of little use, and if freely employed, injurious." The author enumerates the symptoms which attend an habitually disordered state of the liver and consequent distention of the duodenum, for which, as well as for the modes of administering the mercurial, we must refer to the work.

In all the more obstinate cases of the disease it is necessary constantly to watch the state of those organs with which the organ chiefly affected sympathises. In affections of the liver of long standing, the state of the brain and lungs must constantly be kept in view, and that of the lower bowels often demands particular attention, both from the sympathies of the different parts of the alimentary canal, and from the constant passage of the vitiated secretions; for although they are little liable to organic disease, and their sympathies are less powerful than those of the stomach, they are sufficiently so to cause the habitual irritation of their highly sensible and extensive surface to prey on the constitution. The author relates a case in which rapidly fatal pulmonary disease had arisen from a long series of severe and frequent attacks in the bowels, not in themselves of a formidable nature, and that in a person neither by constitution nor age inclined to this disease. "Such cases," he observes, "are full of instruction to the reflecting physician, and give us many lessons in the prevention of organic derangement. Habitual disease is like the dropping of water on the stone—every drop does something, though its effect cannot be perceived." The functions, both of mind and body, are influenced, and it is impossible to trace all the effects of the changes in any one, for the animal system is a whole, and no part can be disturbed without being more or less affected. It is among the most prominent laws of sympathy, that the more internal part sympathizes more with the other parts of the system than that which is more external. In the alimentary canal, as we recede from the stomach in either direction, the sympathy with other parts of the system becomes less: it is less in the fauces than in the œsophagus, less in the great than the small intestines.

But more than this or any other circumstance, the sympathies of our frame are influenced by a tendency to disease; the weakest part is almost always that which suffers most. "In proportion," the author observes, "as the vigour of each part is entire, it is independent of others; in proportion as it is enfeebled, it falls under their influence. If the hardy savage produces morbid distension of the stomach by an excessive meal, he neither complains of headache, nor is harassed by cough; with more time the stomach can do its own work without disturbing its neighbours, and they are too much at ease to be easily disturbed. But when a thousand anxieties and irritations, with which he is unacquainted, have given to every nerve what may almost be called a morbid sensibility, depending on the same law by which the gums, naturally of little feeling, become morbidly sensible from the long irritation of a carious tooth; each part feels the state of every other, and a door is opened to a thousand ailments. The complicated feelings of civilized life, while they sharpen the mind, enfeeble the body. . . . . . Almost all the unexpected occurrences of disease may be traced to the sympathies which are constantly operating—which, as I have already had occasion to observe, have obtained too little attention, and which, the more refinement increases, our sensibilities operate with the greater force." We have no room to enter on the principles of diet laid down by the author, an essential part of the treatment in the case before us, but shall conclude our review of the present chapter with the following quotation respecting the regulation of exercise and sleep. "The importance of regularity is equally remarkable in the regulation of exercise and sleep. The early part of the day is the proper time for all the more powerful exertions of both mind and body. Towards night the various impressions of the day have produced their effects, and the languor which succeeds all kinds of excitement is sensibly felt by the invalid. Repose is then more necessary, and exertion of every kind more apt to be injurious.

"In health there is often some increased excitement of pulse in the evening. In the invalid it frequently amounts to a degree of feverishness, and is only effectually relieved by the most perfect repose, healthy sleep. Those are mistaken who suppose the period of the day allotted to exercise and to sleep immaterial, provided the proper length of time be spent in both. There is a diurnal revolution in the animal, which corresponds to that of the natural world.

"We are active and vigorous in the fresh air and rising dews of the morning, and more or less worn and relaxed in the vapid air and falling dews of the evening. The healthy do not sensibly feel each deviation; but there is a sensible difference even in the appearance of those who habitually keep good or bad hours, although the latter may spend more time both in exercise and sleep.

"The languor of the invalid in the evening is not wholly the consequence of the exertions of the day. The evening air has always been found unfavourable to him; and in some countries it is very sensibly so to the most healthy. The peculiar quality of the morning air is given by the contact of the cold and moist surfaces of the night; of the evening air by that of the dry and heated surfaces of the day. Every one has felt the refreshing effects of a sudden shower after a sultry day. The evening and morning air is always, more or less, the air before and after the shower.

"The luxurious neither experience the freshness of the morning air, nor the calmness of the evening repose, which nature dictates. Thus, as in all other instances, if we deviate from her laws we only lose on the one hand, as we gain on the other; and I believe if the balance could be fairly struck, it would always be in favour of her votaries. It requires much reflection to see all the consequences of any deviation from them. One effect produces another, and the tone of both mind and body are influenced by many circumstances, which, viewed barely in their immediate effects, appear of little moment."

On the seventh chapter, on the treatment of the functional derangements of the other abdominal viscera, which precede change of structure, we have not room to enter; and our notice of the last, on the treatment when organic disease is actually established, must consist in two quotations from it; the first giving a summary of the principles on which the author's views of the treatment are founded; and the other a case exemplifying the effects of it, one of the very few related by the author, the frequent relation of cases being regarded as inconsistent with the nature of his treatise.

"As we have seen that the disposition to organic disease is proportioned to the degree in which the vessels of the part are affected—that is, in proportion to the degree in which the irritation, caused by the derangement of its nerves, has deranged the circulation in the part; that this derangement is always of an inflammatory nature; and that all causes of inflammatory action, whether of the part itself, or of the general system, promote the establishment of organic disease; we find, after its establishment, that all such causes tend to confirm it, and promote its progress, and consequently to oppose the operation of the means of cure.

"On the other hand, organic disease of vital organs is for the most part accompanied by a debilitated state of the general habit, which, by weakening the healing powers of the constitution, never fails to increase its obstinacy.'

"In order, therefore, to bring the habit into the state most favourable to the removal of organic disease, and consequently for the operation of the remedy, we must, at the same time that we lessen the inflammatory tendency, do all which the circumstances of the case admit of to support the strength. These indications in a great degree oppose each other, and our practice must lean to that side where the symptoms are most urgent.

"Our great resource is a combination of general with local means, to support by the former the general powers of the nervous and sanguiferous systems, and by the latter to lessen as much as we can the determination to the part affected, the consequence of the greater debility of its vessels, by which we relieve both the inflammatory tendency of the part, and that of the general habit, which depends upon it. If the seat of the disease be very tender on pressure, and the pulse much contracted, striking the finger when lightly touched like a wire, we may be assured that the mercurial will be less useful till these symptoms are mitigated, and will hardly fail, if freely administered, to increase them.

"It is therefore necessary that, under these circumstances, it should be given cautiously; for even under these it should be given. It is one of the greatest advantages of the minute and frequently-repeated doses, to which I have been gradually led by the effects of this medicine, that they do not occasion that excitement of the general system which we observe from larger doses; and by tending imperceptibly to restore the function of the debilitated part, and thus remove a principal source of the irritation which attends the disease, and exciting all the secreting surfaces, they often eventually prove the most effectual means of removing the inflammatory tendency. However little ground is gained by each dose, we keep it, and thus the good effects accumulate. When larger and more distant doses are employed, the effect of one is more or less lost before the next is given; and thus often little progress is made, or, perhaps, notwithstanding the temporary relief, the disease on the whole is increasing."

The author, as the best means of laying before the reader his mode of administering mercury in organic disease, concisely relates the particulars of a severe case in which it was employed.

"An officer between thirty and forty years of age, returned from India in a state of great debility. His countenance was sallow, and at once informed the experienced eye, that he laboured under organic disease. His liver was much enlarged and indurated. He was subject to severe inflammatory attacks, which increased his debility, and frequently brought him into immediate danger; and the whole of his state was such as is supposed rarely to admit of a perfect restoration to health.

"The temporary attacks were relieved by local blood-letting, and such means as allayed the pain, and quickly restored a freer secretion of bile; and, in the intervals, he was desired to take half a grain of blue pill, and a grain of extract of henbane, three times a day, with such medicines as allayed the tendency to fever. The most nutritious diet, of easy digestion, which his state admitted of, was enjoined; and he was desired to be as much as he could in the open air without any degree of fatigue, or the risk of taking cold; and, as his strength improved, to make walking his principal exercise.

"In a short time he experienced a sensible improvement in his health; the severity of the occasional attacks abated, and in the space of some months ceased to return. He could now move about with more ease, although the enlargement of the liver was still considerable; and after being made acquainted with the circumstances necessary to be attended to, he was not prevented from going to the country, and the continent, to which his affairs called him. His recovery advancing, he repeatedly thought himself well enough, according to the directions I had given him, to permit the alterative to be discontinued; but was constantly obliged to return to its use.

"I saw him from time to time, without finding any reason to change his plan of treatment, assuring him that the time would come when the means might be laid aside without a return of the disease, and that it only required the habit of health to be longer maintained by the medicines, in order to render it permanent without their aid; for hardly any enlargement of liver could now be perceived; and, at each interval, the improvement both of his looks and health was apparent. I still advised him from time to time to try how far the alterative could be laid aside, but to return to it as soon as he perceived the least threatening of his symptoms.

"At the end of more than two years from the time he began to use the alterative, during which he was uniformly recovering his strength and healthy appearance, he found my prediction verified. He no longer required the use of medicine; all enlargement of the liver had disappeared; and he had in all respects regained both the feelings and appearance of health. He returned to the service, but not to India; and, although several years have now elapsed, he has experienced no return of his disease.

"I have related the circumstances of this case at greater length, because they tend to illustrate several positions of the foregoing treatise, as well as the effect of the alterative. It is only where organic disease has actually taken place, that so long and constant an employment of the alterative is necessary."

The great length to which our analysis has extended, leaves us no space for any thing in the way of comment; we shall, therefore, content ourselves with observing, that, though we do not agree in all the views of the author, we cannot take our leave of him without expressing the conviction we have received from the perusal of his work, that it is the result of extensive experience and mature reflection, fully maintaining him in the rank he has already acquired among the learned and accomplished physicians of his time[*].

An Account of Inventions and Improvements in Surgical Instruments, made by JOHN WEISS, 62, *Strand.* Illustrated by numerous Engravings. Second edition, much enlarged. 8vo. 1831.

IN noticing the work of which we have just transcribed the title page, it cannot be expected of us to go into its details, as such a work can hardly be considered to come within the scope of our labours: but this much we will say—that no individual ever invented and improved such a variety of surgical instruments as Mr. Weiss. The merits of this ingenious mechanic are well known to the profession; it is only necessary to give him a hint of what is required to be done by the instrument, when his inventive genius is set to work, and he spares neither expense, time, nor trouble, to attain his object. Such a man merits, not only the thanks of the profession and the public, for his peculiar exertions in the cause of suffering humanity, but he deserves something of a more substantial nature, in the shape of patronage and encouragement from the community at large.

Every operating surgeon, at least, will find it convenient to be in possession of Mr. Weiss's book—or rather catalogue, where, by a reference to the plates, he will be able to select the instrument best calculated for his purpose.

MEDICAL GAZETTE.

Saturday, September 3, 1831.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

ON THE STATE OF THE BLOOD, AND EFFECT OF SALINE MEDICINES, IN MALIGNANT DISEASES.

WE have been informed, on the authority of Dr. Harder, a physician to the Court of St. Petersburgh, who accompanied the Grand Duchess Helena to England, that his countryman, Dr. Jachnichen, of Moscow, has demonstrated "that healthy blood contains a notable quantity of free acetic acid, which, as well as the natural portion of its serous fluid, is in a striking proportion lost in the blood of cholera patients; but that these substances are to be regained in nearly their right proportions in the fluids inundating the primæ viæ, in cholera, and voided by the vomitings and alvine evacuations in that disorder." We think it due to our Russian correspondent to make this announcement, the rather as it is intended to correct what he regards as an inaccuracy on our part, though we are free to confess, that we are by no means convinced even now of the error lying with us. We cannot understand how acetic acid can remain *free* in a solution which contains soda; neither, as the addition of acids blackens the blood, does it seem probable that the removal of such agents should also have the effect of rendering that fluid more dark. It is not our intention, however, to enter farther upon a question, for the satisfactory discussion of which we lack materials; but we shall take the opportunity it affords of bringing before our readers the statements of Dr. Stevens, whose views with regard to the state of the blood in malignant diseases appear to us to merit more attention than they have received, and which are, to a certain extent, though indirectly, strengthened by some of the phenomena which the cholera has presented in the north of Europe.

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[*] In the former part of our analysis the following typographical errors occurred:—Page 687, l. 38, *for* "shapes the brain," *read* "shakes the brain." Page 689, l. 12, *for* "light pulse," *read* "tight pulse." Page 690, l. 40, *for* "origine," *read* "angina."

In our number before last, we published a letter from Dr. Barry, in which he says, "Two physicians (Germans), Ysenbeck and Brailow, stated publicly and firmly yesterday, in my presence at the Medical Council, that during the preceding eleven days they had treated, at the Custom-house Hospital, thirty cholera patients, *of whom they had not lost one.* They give two table spoonfuls of common table salt, in six ounces of hot water, at once, and one table spoonful of a similar mixture, cold, every hour afterwards. They always begin by bleeding." A similar treatment has also been tried at Warsaw, by Mr. Searle; and most of our readers will recollect, that this is neither more nor less than the practice which has been so warmly recommended by Dr. Stevens, not only in yellow fever, but also (reasoning from analogy) in the cure of other malignant diseases.

In a paper which was read at the Royal College of Physicians on the 3d of May last year, and of which we published an abstract, which has since been extensively circulated in the periodicals of Europe and America, Dr. Stevens asserts that there is sometimes met with in the West Indies, a malignant form of the yellow fever, in which, from the beginning to the end, it is evident from the symptoms that there is little or no affection of the solids, while often after death even the most able anatomist cannot detect any trace of disease, either in the brain, the stomach, the intestines, or any of those organs whose derangements are generally supposed to be the cause of fever. In those fatal cases there is no excitement in the commencement sufficient to produce disorganization; and we can only ascertain the real cause of death when we open the heart and examine the state of the once vital fluid. We there find, in place of blood, a liquid, nearly as thin as water, almost as black as ink, and evidently so changed as to be totally incapable of stimulating the heart; in both cavities of which the fluid is equally black, and in fact in the whole vascular system all distinction between arterial and venous blood is entirely lost.

Now Dr. Stevens holds the saline materials of the blood to be the cause of its red colour, and certain other properties essential to life. He does not, indeed, identify the saline with the colouring matter; on the contrary, he considers the latter as a mere animal dye, which is naturally black, but which possesses the peculiar property of striking a red colour with a solution of the neutral salts. He also considers the saline ingredients in the blood as the cause of its fluidity, on the assumed ground that the fibrin and the albumen are naturally solid, and that the tendency to this condition is counteracted by the saline impregnation.

"Nature (says Dr. Stevens) does nothing in vain; and all the analyses of the blood have proved that, in health, it invariably contains a given proportion of saline matter. This is not accidental; for it is as essential, and exists as invariably in healthy blood, as either the fibrin, the albumen, or the colouring matter. Arterial must evidently contain a larger proportion, or at least a stronger saline matter, than venous blood; for all the solids, and most of the secretions, derive their saline matter from arterial blood. But the serum of even the venous blood which is left contains a proportion of thirteen ounces to the thousand of these salts, independently of what is lost by evaporation, &c. We well know how active these salts are as chemical agents, and these agents are so constantly found in the blood, and in healthy blood their proportion is so exact, that we are forced to believe that they are placed there for some important use. But the importance of this saline impregnation has been almost entirely overlooked, from the great attention that has been paid, both by physicians and philosophers, to the much less important colouring matter."

Of all the ingredients, Dr. Stevens appears to consider the saline matter of the blood as by far the most essential to its healthy state. Any of the other ingredients may be diminished, and still the vital fluid will perform its functions; but when the saline impregnation is lost, or greatly lessened, as he states it to be, in malignant diseases, the blood becomes black, goes fast to decay—a change which is soon followed by death. "On examining the blood soon after death, of those who had died of the yellow fever, the colour in the whole mass of blood, both in the arteries and veins, was completely changed from its natural scarlet, or modena red, to a dark black. I have frequently filled one glass with the black fluid taken from the heart, and another with the black vomit taken from the stomach. They were both so unlike the blood of health, and resembled each other so completely, that it was almost impossible to distinguish the one from the other."

The oxygen of the air had no effect whatever in reddening this dark fluid; but on adding a small quantity of any of the neutral salts, even to this black blood, the red colour was immediately restored; and Dr. Stevens believes that certain saline agents have a specific effect, when administered during life, in remedying that diseased state of the blood which is uniformly met with in this, and probably in all other malignant diseases.

It appears that Dr. Stevens first commenced this practice with a solution containing two parts of muriate of soda and one of nitrate of potass. An objection to this mixture was, that when given in large doses it was apt to disagree with the stomach; and finding that other saline agents possessed, in an equal degree, the same property of remedying the diseased state of the blood, these were occasionally employed;—the carbonate of soda, for example, was preferred particularly when there were any signs of acidity in the stomach, and the tartarised soda was generally given when a purgative was required. Under this practice the result is stated to have been incomparably more successful than it was under the old treatment.

The resemblance between cholera and certain malignant forms of fever, both as to their phenomena and, if some of the latest accounts be correct, likewise as to the treatment which proves most successful, is very striking. Even in those cases in which there is no excitement, and of course no increased action to be reduced, the removal of a small quantity of blood is equally beneficial in both; by relieving the overcharged heart and enabling it to circulate with more ease that which is left. But in the malignant fevers of the West Indies, much more commonly than in cholera, there is great increased action, and the lancet is then used freely. As early as possible after the first bleeding, the patients are freely evacuated by means of some active purgative. As soon as the excitement is sufficiently reduced by these means, which it generally is in less than twenty-four hours, the use of the saline mixture is commenced. This method of treatment, which Dr. Stevens has the merit of having first proposed, is gaining ground in the West Indies; and it is consistent with our knowledge that Mr. Greatrix adopted it in the Military Hospital of Trinidad, where a mixture containing the muriate of soda and nitre was given in pretty large doses immediately after the patients had been bled, and had the bowels evacuated by enemata. The year 1828 was sickly in many of the West India islands: from the beginning of that year up to the month of August, we believe that, under the old treatment, the Royals alone had lost forty-two men from fever. In the month of August of that year Dr. Stevens visited that island, where, at the request of the Governor, he communicated with Mr. Greatrix on the subject of the West India fevers. This gentleman subsequently adopted the saline treatment, and the result under this new practice is thus described by himself:—"The above system has been applied to 340 cases, or thereabouts, including both the remittent and yellow fevers admitted to the hospital, after the fevers had existed from six to seventy-two hours antecedently to an application to the hospital, *with such success that during the last seven months not a case has died.* Three men have died in that time, having the remittent fever, but they had at the same time abscesses in the lungs and purulent expectoration."

It appears certain that the mortality from fever has been lessened in those islands of the West Indies in which this treatment has been fairly adopted; and it seems to us, that analogical reasoning, as well as the statements of Dr. Barry and Mr. Searle, would fully justify a trial of the same method in cholera. This last is, indeed, so virulent a pestilence, and so many perish in the first stage of collapse, that no treatment which human ingenuity can devise will probably do more than rob it of a certain portion of its mortality; but should even this limited benefit be obtained by saline medicines, exhibited on the principle of remedying the morbid condition of the blood connected with its blackness, it will be chiefly owing to the statements of Dr. Stevens on an analogous subject, for though his paper, as we have said, has never been published, yet our account of it has made his doctrines extensively known, and will thus lead to their confirmation if true, or their rejection if false.

Cause of the Red Colour of the Blood.

As connected with the above subject, we may lay before our readers the following communication, extracted from an American journal. It is a letter from Dr. Sewall, the Professor of Anatomy and Physiology in the University of Washington, which we find published in the Boston Journal for December last.

The cause of the red colour of the blood has long been a subject of keen discussion among physiologists, and nothing has hitherto appeared upon the subject at all satisfactory, and against which powerful objections could not be brought.

Dr. Stevens, an eminent physician of

St. Thomas, has instituted an experimental inquiry into this subject; which has led to some novel and interesting results. From his experiments it appears,

1st. That the blood owes its red colour entirely to the presence of the saline matter which is invariably found to exist in it, while in a healthy state.

2dly. That the dark colour of venous blood arises from the presence of carbonic acid, which, like every other acid, turns the blood black.

3dly. That the oxygen of the atmosphere can only affect the colour of the blood inasmuch as it possesses a powerful affinity for carbonic acid, which it takes from the blood by attracting it through the delicate membrane that lines the bronchial vessels and air-cells of the lungs.

4thly. That the removal of the carbonic acid from the blood by the action of oxygen, does not produce a change in its colour, unless there be saline matter actually present, to impart to it the arterial tint the moment the carbonic acid is removed.

5thly. That acids, alkalies, electricity, and every thing which destroys the neutrality of saline matter, gives to the blood a dark colour.

Whatever practical inferences or change in the treatment of diseases these experiments may lead us to, the idea that the red colour of the blood is owing to the saline matter which it contains, is entirely new; and no one can deny to Dr. Stevens the merit of having been the first discoverer of this interesting fact. He is still prosecuting his inquiries, and his researches upon this and other subjects connected with it promise much to the profession. They will soon be laid before the public in detail. I have had the pleasure of witnessing a number of Dr. Stevens's experiments, as performed by his own hand, upon the blood; and so far as I have had an opportunity to examine them, they have been performed with great care and accuracy, and were entirely satisfactory.

We are aware that Dr. Stevens himself is about to publish on this subject; yet, as there are some parts of Dr. Sewall's letter which we do not clearly comprehend, and as every thing relating to the blood and the treatment of malignant diseases possesses great interest at this moment, we have applied to that gentleman for information on certain points: should we succeed in obtaining this, we shall lay it before our readers in another number.

POPULAR WRITERS ON CHOLERA.

We have a suspicion that some readers think that we, as well as the press in general, talk too much about cholera. They cannot look into this Journal any week, nor into almost any newspaper of the day, without the *terrible word*, and a whole body of facts about it, saluting their timid glances. This is mere childishness. All Europe, thanks to the press, is awake to the impending calamity; nor is there any intelligent government in the same quarter of the globe, that has not taken its measures of precaution for warding it off. The population of every European country is now more or less familiarized with the dread name and description of cholera; and should the epidemic ever come amongst us, it will not come like a stranger, or steal upon us like a marauder; we shall be as well prepared for it as human ability can enable us to be, and, in the alarming announcement of its presence, it will be robbed, we trust, of half its terrors. But in attributing this beneficial influence to the press generally, we are aware that much deduction must be made, on account of the unworthy uses to which the subject of cholera has been turned by many, especially of the political journalists, who have magnified trivial instances of its occurrence, and often created new ones.

We have seen but too many examples of late in which cases of the common cholera of this country have been caught up and put forth for the purposes of alarm: perhaps we should modify the expression with respect to some of the parties, and substitute pure ignorance for their motive; but we could lay our hand upon one newspaper in particular which, within a very short time since, most disgracefully published an hospital report of a case of English cholera with all its—to unprofessional eyes, disgusting—detail in its columns, and advertised the number on the strength of this sporadic instance, as if it was the commencement of an epidemic seizure. The newspaper, fortunately, was one of no character or circulation, and the "doer-up" of it took nothing by his filthy as well as dishonest proceeding. It is in this way, no doubt, that many readers form unfavourable impressions of the labours and services of the public journalists, and venture to express their opinion that these functionaries would better discharge their duty by keeping silence on a subject, the horrible details of which are of purely professional interest. Our opinion is directly at variance with this. Convinced as we are that in this, as in most other things, *semipublicity* is dangerous, and since semipublicity, at least, is inevitable where cases, neither few in number nor despicable in their severity, are so frequently occurring, we hold that there is no better way of re-assuring the public, and begetting in them that confidence which is so essential to security, than by honestly apprising them of the real state of things: that not a year passes in which we have not many cases of cholera amongst us, and some of them fatal too, but that the proper method of treatment is no mystery, nor unknown even to the humblest of our practitioners, and is almost invariably successful when had recourse to at an early stage of the invasion.

It is the political journals of more than ordinary circulation which may do mischief by entering too much into unguarded particulars, and giving them, often involuntarily on the part of the writers, a colouring which does not properly belong to them; their duty it should simply be, not to report cases of cholera, nor to suffer themselves to become the tools of advertising quacks, but to insist upon the essential harmlessness of the complaint as it is endemic here, and to proscribe certain common but dangerous practices—for example, intemperance and sudden chills, which at this season have a positive power of engendering the malady.

A recent case is mentioned by a French medical writer, which, if it happened here, and came within the cognizance of certain newspaper scribes, would no doubt be converted into serious matter of alarm, and be hailed as a god-send to the columns in which it should first appear. It is the case of a gentleman, who one day, after living in his usual manner, and taking but a single glass of Madeira at dinner, was seized in the evening with sickness, which about midnight was developed in gripes, vomitings, purgations, and cramps. Down stairs he went to seek relief; knocked at his sister's door; and threw every body into alarm. His brother-in-law, who was fast asleep, was roused; and having got up in his shirt, made a couch in an adjoining room for the unhappy patient. Here, however, the patient was attacked with fresh vomitings and purgations. Presently after, the brother-in-law, who had gone back to bed, was seized with griping, vomiting, diarrhœa, &c.; and the maid-servant exhibited symptoms of the very same description. They were all well next day. But what was this? Was it contagion—a clear case of transmission of cholera by contact? or can it be accounted for in any other way? We entirely agree with the writer, who naturally attributes the circumstances to the effect of chill (*refroidissement*), and to the agitation and alarm of people being roused out of their sleep, and bustling about in the middle of the night half-naked. Perhaps, if exciting causes were attended to in this simple way in many of the pretended cases of cholera recently reported, a better insight would, ere this, have been attained of the nature and habitudes of the disorder, as it appears among us.

But the non-professional journals should abstain altogether from publishing cases of this sort, which, a thousand to one, are grossly exaggerated, or wholly inexplicable by them. *We* can better appreciate their value; it is our business: we can pass a judgment on them with confidence—can publish our opinion; and, we flatter ourselves, so far from alarming, we can inspire the public with reasonable courage by our communications.

Having said so much by way of protest as to what the political journals should not publish about cholera, we shall add a specimen of the sort of information which they should lose no opportunity of circulating—the precautionary steps and measures adopted by the leading powers of Europe for the security of the people. It is a subject to which the attention of the wisest members of our profession, in all countries, is at present directed; and if safety can be hoped for from human means, from these men assuredly must it come; nor can both gratification and public confidence fail to be derived from all the publicity which their measures may receive.

We find in some of the French papers which have just been received, a brief comparative statement of the arrangements made by the respective governments of Germany, France, and England, to arrest the apprehended progress of cholera into their dominions, and the directions which are to be followed in case it should unhappily make its appearance within them. Some of the particulars we think it worth while to make room for, and we shall begin with—

FRANCE.—A report of the progress of cholera from the east to the west of Europe having been presented to his Majesty by M. d'Argout, the Minister of Commerce, a royal ordinance has in consequence been issued, which establishes branch committees of health in the chief towns of twenty departments; and there are ordered to be other branch committees subordinate to these, in the various subprefectures of the said departments. It is strictly forbidden to import into France, either by sea or land, any articles of cloth or clothing, bed-clothes, hospital or camp furniture, excepting such as belong immediately to individuals as their private goods and garments, in which case they must follow the condition of the owners, either for free pratique or for quarantine purification. Hemp and flax from the north, to be treated with ventilation and the usual precautionary measures, during the number of days appointed by the local committee of health. The persons engaged in the carriage of those articles to be submitted to the same space of quarantine. In Paris a commission of health is established in each of the quarters; the commission consisting of the commissaries of police, two physicians, and an apothecary, who are to correspond with the board of health of the respective division of the metropolis, or with the prefect of police.

GERMANY.—A proclamation of the King of Prussia, dated the 6th of July, organizes at Berlin a supreme council of health, and in each of the other towns a commission acting in accordance with it. Neither travellers, nor their effects, are to enter Prussia by any but by the permitted routes, and after a quarantine of twenty days. Nobody is permitted to travel in the neighbourhood of the threatened frontiers without a bill of health, which is to be inspected every evening. All postmasters and coach-owners are commanded not to take up any travellers who are unprovided with such bill; nor shall these individuals be received into any inn, hotel, or private house. Bales of woollen cloth and haberdashery are strictly forbidden. Rigorous quarantine is imposed on every ship from Russia, no matter what bills of health they may have. Every place within the kingdom ascertained to be infected, to be forthwith surrounded by a *cordon* three miles from the town. In the duchy of Posen no meeting or assembly whatever of the inhabitants is permitted—even within the churches. The government to communicate to the public everything they know about cholera, without distinguishing the facts which may alarm from those which may assure.

Hanover and Saxony have adopted similar measures; and in Hamburg special hospitals for the complaint have been established.

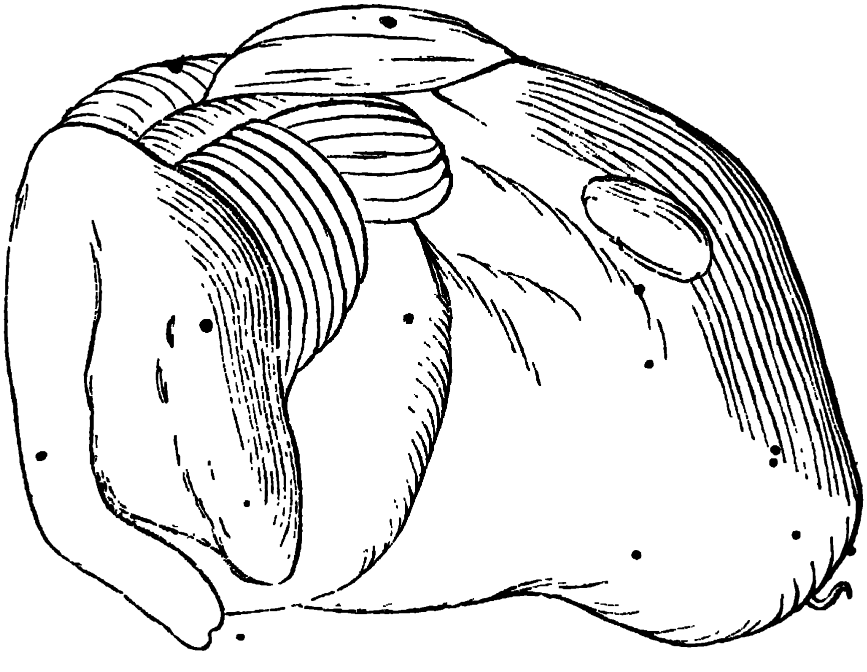
Austria has put in practice methods of equal rigour; and a strict cordon is formed between that country and Hungary.

England.—Under this head the French journalist has put down many facts with which our readers are acquainted, besides stating others with which, perhaps, they are not so familiar. There are seven quarantine stations for England, four for Scotland, and two for Ireland. Twenty vessels of the line are employed as floating hospitals for the reception of equipages and merchandize infected, or supposed to be so. A commission of health is established in every city, town, and village, which is to hold direct communication with the general Board of Health. The towns are subdivided into quarters; and there are places allotted in them for persons who shall have been attacked with cholera, for those who may have been in contact with them, and for those who are convalescent. Nor can the latter be restored to their friends within the period of fifteen days after their recovery.

THE INHABITANT OF THE PEARLY NAUTILUS,

(*Nautilus Pompilius.*)

The inhabitant of this shell, which has so long been a desideratum in natural science, was discovered by Mr. George Bennett on the 24th of August, 1829, in a bay of the Island of Erromanga, one of the New Hebrides group. The animal had only before been noticed by Rumphius, and some further remarks had been made upon it by Sonnini, in his edition of Buffon, but great doubt has been expressed if any reliance could be placed on these accounts. Mr. Bennett's sketch of the animal, taken immediately after it was removed from the shell, fully confirms the general accuracy of Rumphius's figure. We shall give no further count of it at present, as a detailed examination of the animal is in progress, with the intention of attempting to point out the true situation of this genus in the animal kingdom, which is a fact of some interest, as it will at the same time shew the natural situation of that large group of fossil shells called *ammonites baculites*, &c. &c. We have been permitted to give the following representation of the animal when just removed from the shell.



FACULTÉ DE MÉDICINE, PARIS.

On the 9th ult. M. Bouillaud was appointed to the chair of *Clinique Interne*, with the suffrages and approbation apparently of all parties. In fact, this seems to have been the first appointment, since the restoration of the Concours, that has given any satisfaction. The charge of gross partiality has been laid to the judges in every instance but this,—a clear proof that this mode of election is not so infallible as some of our *confrères* on this side the channel would have us suppose. The principal competitors of M. Bouillaud were MM. Louis and Rostan.

DEATH FROM INANITION.

Some Account of Granet, the French Prisoner, who lately starved himself to Death.

The following brief report was transmitted, a few days ago, to the French Academy of Medicine, by the Minister of the Interior. It was drawn up, we understand, by M. Dufour, a physician of Toulouse, who attended the prisoner during the whole period of his sufferings and death:—

Guillaume Granet was twenty-four years of age. It was on the 20th of April last that he formed the fatal resolution of starving himself, with intent to avoid the punishment due to his crimes. On the 21st he was found lying on the ground, dressed, head uncovered, his irons bolted on both hands and feet; he would only answer by signs, and would take neither solid nor liquid food. It was attempted to make him swallow by force; but this only threw him into an outrageous passion. When visited in the evening, however, he was more calm, and thought fit to use his tongue.

22d and 23d. Much the same way; urine fœtid; burning heat in the throat.

24th. Face flushed; breath exceedingly foul; haggard looks; pulse at the wrist scarcely perceptible.

25th. He passed some urine.

26th and 27th. Nothing remarkable.

28th. He came down to the court-yard, and drank up some water from the well: his handcuffs taken off.

29th. Shiverings.

30th. Drank a glass of water.

May 1st. Spoke, but not to be understood.

2d. Rolled himself in the kennel of the court-yard.

3d. Drinks some water; will take no broth or soup.

4th. Passes some urine.—On the 5th, he ran out of his dungeon, with nothing on him but his shirt: upon coming back, he lay down and ate a morsel of bread. He was taken to the Infirmary, and about midnight ate some soup with a little bread and wine.

6th. Took nothing.

7th. Drank of his own urine, and about a quarter of a spoonful of water. In the afternoon he complained that there was poison in the food that was offered him.

On the 9th, 12th, 15th, and 17th, he drank some water.

18th. Drank but half a spoonful—attacked with violent fits of coughing, and oppression of the chest.

21st. Ran out, and wanted to drink of the kennel, but was prevented; same night he drank water.

22d. Wanted to bite and scratch; complained of sharp pains in his bowels; passed water.

23d. Took some broth.

On the 24th he was found lying on his belly; his pulse feeble and slow; his fists shut; drank some water from the kennel, which had been cleansed.

25th.—Suffered much, and abused every body; pulse 53; drank of the kennel.

26th.—Tore his shirt to pieces; same night passed some urine.

On the 27th and 28th he drank eight glasses of water; his fæces were found *carbonized;* said he had still a fortnight to live.

In this way, at one time easy and again in agony, now refusing to drink and now taking a little water, he continued to live on till the 17th of June (the 58th day) when about five in the morning he died, after struggling for four hours in convulsions.

During some of his last days he complained of cold; his legs too were shrunk up, and marked with blackish spots—dry gangrene, according to M. Dufour.

These are all the facts of this wretched man's suffering which have been preserved. The particulars of his *post-mortem* have not yet reached the Academie, but are promised to be sent ere long by the Minister.

EXTRACTS FROM JOURNALS,

Foreign and Domestic.

CASES OF TIC DOULOUREUX, IN WHICH THE CYANURET OF POTASSIUM HAS BEEN EMPLOYED WITH SUCCESS.

By M. le Docteur Lombard, of Geneva.

The cyanuret is employed in friction: it is either dissolved in distilled water, or made into an ointment with pure hog's-lard, and used in either form, according to circumstances. From one to four grains in an ounce of water, is the quantity usually employed for the aqueous solution: from two to four grains in an ounce of lard is the composition of the ointment. It may be mentioned that the aqueous solution is in general of the two forms the more prompt and instantaneous in its effects.

Case I.—*Facial Neuralgia instantaneously cured by the Hydrocyanate of Potash in Friction.*

A lady, of robust habit, 49 years of age, was a martyr to the most agonizing occasional attacks of pain in the space between the temporal region and the ciliary arch and maxilla. She used to scream violently in these torturing accessions, and sometimes lost all appearance of sensibility to such a degree that she has been supposed to be struck with apoplexy. Pulse 84; face rather flushed; no functional derangement. She was ordered to be rubbed with the aqueous solution, containing sixteen grains of cyanuret of potassium in four ounces of distilled water: it was rubbed on the forehead and cheek with a ball of cotton. The pain gave way almost instantaneously at the very first application; and seemed, as the patient said, to be rubbed away with the hand. A complete cure was effected by persevering a little while in the use of the remedy.

Case II.—*Periodic Neuralgia removed by the Ointment of the Cyanuret.*

The cure in this instance was less prompt, but not less certain. A lady, 38 years of age, experienced the severest pains in the temporal region and upper jaw of the left side: they came on regularly every morning at four o'clock; went on increasing in severity until about ten, and did not cease till four in the afternoon. In that interval she laboured under anorexia, fever, headache, &c. and was almost driven distracted. She was bled to twelve ounces to relieve congestion, and then the ointment was applied to the cheek and temple. Two grains to the half ounce of lard were at first employed, but the improvement was more rapid under the application of ten grains to two ounces. Lotions of the cyanuret were eventually used, and the cure was complete.

Case III.—*Facial Neuralgia almost immediately cured.*

A lady, 20 years of age, suffered for several days, at the same hour, the most torturing pains in the orbital and supramaxillary regions. Her face was much flushed, particularly on the affected side. Ten grains of the cyanuret were dissolved in four ounces of distilled water, and rubbed on with cotton. The application was quite successful.

Case IV.—*Chronic Occasional Neuralgia similarly treated.*

A woman, of 80, who had long suffered from irregular attacks of this complaint, was cured by lotions and frictions, compounded pretty strongly, and continued for some time.

The cyanuret of potassium is contraindicated where the nervous affection is complicated with inflammatory action, discharge, &c. It is a useful remedy in non-inflammatory rheumatism.

In sciatic neuralgia it has not been successful—nay, it has been necessarily discontinued, on account of some unpleasant accidents which it occasioned.

In white swelling, attended with acute pain, poultices moistened with the solution had the effect of producing much comfort, though the continuance of their application had no promise of amendment in it.

It is on the whole inferred that the calming properties of this remedy are superior to those of any other known, and that it should always have a preference where inflammation does not exist. Lotions, with hydrocyanic acid, are by no means to be compared with it, for the acid is decomposed with facility, and scarcely to be used without danger.

The first application of the cyanuret in the above way is claimed by M. Buttigny and his brethren of Geneva, but it is disputed with them by Messrs.

Robiquet, Villaumé, and Bally of Paris. —*Gazette des Hôpitaux.*

VISION OF THE MOLE.

Does the mole see? Aristotle and all the Greek philosophers thought it blind. Galen, on the other hand, maintained that the mole saw. He affirmed that it has all the known means of sight. The question has been resumed in modern times. Naturalists have found the eye of the animal. It is very small—not larger than a millet-seed; its colour is an ebony black; it is hard to the touch; and can scarcely be depressed by squeezing it between the fingers. Besides the eye-lid which covers it, it is protected by long hairs, which crossing each other, form a thick and strong bandage. Such an eye should be destined to see; but anatomists do not find the optic nerve. What use could an eye be of, deprived of a nerve, which in other animals transmits the visual sensations to the brain? This consideration naturally tends to the opinion of Aristotle, and the Greeks, and to induce the belief that the mole does not see, and that its eye is only a rudimental point, without use. Direct experiments, however, made at the request of G. St. Hilaire, show most incontestibly that the mole makes use of its eyes, since it turns to avoid obstacles placed in its way. But if it sees, how is it accomplished without an optic nerve? M. Serres was of opinion that the place of this nerve was supplied by a superior branch of the fifth pair, analogous to the ophthalmic branch of Willis. According to Geoffroy St. Hilaire, this change of function in a nerve, which it is not naturally destined to perform, does not exist. The mole sees by aid of a particular nerve, which being unable, on account of the too great extension of the olfactory apparatus, to follow the direction which it takes in animals, towards the *tubercula quadrigemina*, takes another direction, and anastomoses in the nearest point *(au plus près)* with the nerve of the fifth pair.—*Annal. des Sciences.*

AMENORRHŒA.

One of the patients attended for this disease, began to menstruate at the tenth year; she was married when fourteen, and her only child was born before she was sixteen years of age; since which latter period she has never again menstruated, her age being now thirty. —*Midland Medical and Surgical Reporter.*

HÆMATURIA.

The patient, a man, aged 65, affected with hæmaturia, discharged for several days a large quantity of blood per urethram; during one day as much as two pints. Three months after the cessation of that disease, he had an attack of apoplexy, and his right side became paralysed. The right foot is now partially destroyed by mortification, apparently caused by deficient nervous action in that extremity.—*Ibid.*

CASES OF SLOUGHING AND REGENERATION OF THE SCROTUM.

By W. Aitken, Esq. Surgeon*

Case I.—Joseph D. tailor; a healthy man, about 40 years of age, married. When carrying a child up stairs, received from it a slight kick upon the scrotum; this gave him no uneasiness for several days, when he began to feel a slight pain in the part, and on examination found it covered with a reddish flush. This extended in a short time over the whole scrotum, when sloughing commenced, which, in spite of the most energetic treatment, gradually extended, till, in a few days, the whole bag was involved, and separation at last taking place, it was removed. The parts now presented a very peculiar appearance; the testes, completely denuded of their external covering, gave an idea that if recovery did take place, it must be tedious, and in all probability with the loss of part of these generative organs. The result, however, was different; the parts were dressed with oleaginous applications, and speedily put on a healthy appearance; granulation and cicatrization went on rapidly, and in four weeks a covering for the testes—a new scrotum—was completely formed. The skin of the new production was thinner than that of its predecessor, and it was paler in colour, but it performed with equal fidelity all its purposes. During the whole course of the disease he had no febrile symptoms, nor was his health much impaired. His wife has had two children since the period of this affection, and he lately died of phthisis. I have thought it unnecessary to introduce a daily report of the above case, as the treatment presented nothing beyond what is usually followed in similar cases. It is interesting to remark, however, the vast power and accuracy which nature exerts in the reproduction of parts which disease or injuries may have destroyed. To such efforts the patient is often more indebted, though little credit is claimed for them, than to the practitioner, who claims the credit of doing the whole.

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* Glasgow Medical Examiner.

By the Editor.

Case II.—June 30, 1828.—T. A. æt. 21, robust, and of a florid complexion, came under treatment for a superficial sore on the inner surface of the prepuce. The usual remedies were prescribed. On the 1st of July the sore was looking clean, but there were slight redness and infiltration of the most depending portion of the prepuce. In the evening both redness and swelling were increased, and there was considerable constitutional disturbance, indicated by great prostration of strength, furred tongue, and quick pulse. On the morning of the 2d, the scrotum was swelled to the size of a child's head, and the penis proportionably enlarged; both were exceedingly painful, and of a deep red colour, mottled here and there with spots of dark purple. The constitutional symptoms were by no means so severe as on the previous evening. On the 3d, the purple appearance had spread considerably, and the pain continued of a severe burning description. 4th, Decided sphacelus, which continued to advance up till the 12th, when the whole of the scrotum and integuments of the penis formed one large slough, which, by the aid of the scissors, entirely separated, leaving the parts in the following state: penis entirely denuded of cellular membrane, from the orifice of the prepuce (its inner surface was entire) to the pubes, testes and cremaster exposed up to within an inch and a half of the ring, the *tunica vaginalis* of the former of a white colour, and completely exposed. The margin of the sore was within an inch of the anus, posteriorly, and consisted of the integuments of the upper part of the thigh on either side; it looked clean, purulent, and granulating. The treatment up to this period consisted of leeches, purgatives, and repose, on the first appearance of redness and swelling. On the 2d, free scarifications and application of a camphorated lotion, with an emetico-purgative dose of calomel. As the sphacelus advanced, terebinthinate dressings, sulphate of quinine and anodynes, with occasional endeavours to arrest the destructive process by the application of lunar caustic to the sound margin. When the slough separated, the dressings consisted of simple ointment, spread upon the softest lint, with compresses of teazed cotton and a suspensory bandage. This was the dressing employed, along with occasional applications of the solut. nit. argent. until the cure was completed. The process was gradual, but uninterrupted, and its various steps were exceedingly interesting. Both *tunicæ vaginalis*, in about two days after the separation of the slough, became covered with minute granulations; these by degrees became more luxuriant, and at last effected the coalition of both testes. Meanwhile the edges of the wound were sending out small processes of cicatrization, and it was generally undergoing a gradual contraction. The granulations, which were springing up vigorously upon the penis, began likewise to form little insulated portions of new skin, while the prepuce was becoming gradually reflected, and the integuments of the pubes drawn forward by the contraction of the cicatrix. In this manner the healing process went on, until, partly by the formation of new skin, and partly by liberal demands upon the integuments of the groins, pubes, and perineum, the whole parts were covered in. Such, however, was the contraction of the newly-formed cicatrix, that at first the testes were forced nearly up to the verge of either inguinal ring, producing a sensation amounting almost to pain, and the penis was exceedingly retracted. As an instance, however, of the efficiency of the organs, and of how little his mutilation had acted as a sedative upon his passions, the last opportunity I had of examining him was on account of a new attack of syphilis. At this time the general contraction of the cicatrix had disappeared; the integuments of the penis had not, however, become elongated, so that, in erection, the body of the penis extended beyond them, and the prepuce ended about the middle of its length.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

GUY'S HOSPITAL.

Singular Case of Compression of the Brain—Operation five weeks after the Accident—Death.

JOHN FIEND AXFORD, ætat 42, an exciseman, was admitted into Guy's Hospital, under Mr. Key, on Wednesday, December 1st, 1830, suffering under symptoms of compression of brain. His wife states, that five weeks before this time her husband came home in the evening drunk, and that while assisting him into bed, he was seized with a fit of jealousy, and accused her of receiving the visits of another man. Feeling enraged at his accusations, which she alleges to have been unfounded, and seeing a piece of brick, about a quarter of one, lying in the room, she threatened, if he repeated them, to throw it at his head: he did repeat them, when, suiting "the word to the action, and the action to the word," she fulfilled her promise by throwing it with some force at his head: it struck him immediately above the left ear, and made a small wound, which did not bleed much: he appeared to be very little hurt, and was not in the least stunned, but got quietly into bed, and slept the whole of the night. In the morning he got up, went to his duty, and at night, as usual, was drunk. When he came home he was taken to a surgeon in the neighbourhood, who put some plaister on the wound, and gave him a purgative draught. On the following morning two small pieces of brick were removed from the wound, and he was ordered to keep the house: this he did for three or four days, when he thought himself well enough to work, at which he continued until last Monday evening; during the whole time he drank a large quantity of gin. On Monday he complained of his legs aching, and in walking dragged them after him. On Tuesday he had two or three attacks of spasm, and could not get out of bed. The woman also says that for nearly three weeks he has not been able to open his mouth as wide as usual, on account of the jaw being stiff; the wound has remained open, discharging a small quantity of pus; he has not had much pain in the head.

It was between 12 and 1 o'clock when admitted; there was great loss of muscular power, so that he could not walk unless supported between two people; pulse 120, full and labouring; both pupils dilated, but the left more than the right; paralysis of the left upper eyelid, and of the right arm; loss of memory, being unable to give the least account of the accident, or to answer the most trivial question; the jaw so contracted that the teeth could not be separated more than half an inch from each other; there was a wound immediately above the left ear, at the posterior edge of the temporal muscle, a few fibres of which appeared to be lacerated. On introducing a probe, a portion of bone might be felt fractured, and slightly depressed. This ascertained, the wound was enlarged by a crucial incision, and the bone exposed, when a piece about as large as a shilling was found to be fractured in several portions, and a little depressed; neither the saw or trephine were used, one piece being removed by the elevator, the others by the forceps; underneath was seen the dura mater, from which, near the lower edge of the opening, some fungous granulations were sprouting. It was probably owing to the pressure of these granulations that symptoms of compression had more decidedly shown themselves these last three days. In a short time after the operation he appeared much relieved; the pupils contracted; and the pulse rose to 128, and became softer.

To take of Hydr. Submur. gr. ij. Pulv. Antim. gr. iij. quâque secunda horâ.

10, P.M.—Can say a few words connectedly; says he feels better; bowels once opened; pulse frequent, and sharp.

Venesectio ad ℥xij.

2d, 8, A.M.—Has not had any sleep; pulse 120; bowels freely relieved; answers any question, but is occasionally wandering; the paralysis of left eyelid and right arm better; stiffness of lower jaw unaltered.

To continue the Calomel and Antimony every third hour.

Noon.—Complains of pain and stiffness in the loins and both legs, and of an inability to move them.

12, P.M.—The affection of the lower extremity has increased, and now assumes somewhat the character of tetanus; the legs and thighs are extended and rigid; pulse 135, small and sharp; suffers a good deal of pain in the legs.

Venesectio ad ℥xij.

3d, A.M.—Not much sleep; bowels not opened; pulse 120, small and weak; spasm of lower extremities rather increased; at times the legs are a little flexed, but at others they are rigidly extended; abdominal muscles tense; those of the back slightly affected, so as to draw him rather backwards; still, however, the neck and upper extremities are not affected; blood drawn last night not inflamed

To have a blister to the nape of the neck, and to take a drop of Croton oil directly.

8, P.M.—Bowels opened once scantily; pulse 140, small; spasm much the same.

A half-pint Tobacco Enema to be injected statim.

10, P.M.—The enema returned almost immediately; it produced a good deal of nausea, and the pulse rose to 160; it has now fallen to 124; spasm not quite so violent; belly not so tense; less pain.

To be repeated.

12, P.M.—Enema returned without producing much good; pulse 165, small; legs and thighs very rigid; abdomen more tense; jaw continues much the same; paralysis of arm gone.

4th, 9, A.M.—No sleep; pulse 140, small and feeble; spasmodic affection continues, and at intervals is much increased; he has just had a most severe attack of the whole body; this more resembled a chronic than a tetanic spasm, as the hands were alternately contracted and relaxed; blister not risen; bowels not opened; he is unable to speak, but appears sensible.

To take another drop of Croton oil statim.

3, P.M.—The paroxysms have been more violent and frequent, occasionally affecting the upper extremities; the back feels hard, like a board, and he is raised upon his head and heels; pupils sluggish; bowels not opened A tobacco enema has been injected, soon after which the pulse was 125; it has, however, been very variable, from 140 to 120.

To have another drop of Croton oil, in giving which nearer two drops than one were given.

12, P.M.—Bowels scantily opened; tetanus not quite so bad; can move the lower jaw a little; says he is in great pain from the spasm; pulse 130, sharp; breathing rather laborious; a small quantity of unhealthy pus discharged from the wound.

Another Tobacco Enema to be used.

5th, A.M.—Has not had any sleep, but has been much quieter; bowels have been freely relieved of a large quantity of green slimy matter; pulse 108.

11, P.M—Has not had nearly so much spasm to-day; pulse quicker and fuller; skin hot and dry; bowels not again opened.

To take Liq. Ammon Acet. ℥ss. Vini Antim. T. ♏xv. Magnes. Sulphatis, ʒss. ex. Aqua Menth. 4ta horâ.

6th.—Tetanic symptoms much better; he now draws up his legs so as to semiflex them; he is also able to open his mouth wider, but is afraid to put out the tongue, lest it should be caught between the teeth; it appears much excoriated, which may arise, in part, from the croton oil which was dropped into the mouth; bowels scantily opened; pulse same as yesterday.

8th.—Pulse 104, small; bowels not opened; spasm has varied a good deal; at times he has been nearly free from it, at others he has been drawn backwards, with the legs quite immoveable, and the abdomen tense; upper extremities quite free; no pain in head; the wound looking better.

℞ Potassæ Supertart. ʒj. Infus. Sennæ, ℥iss. statim.

9th.—Bowels opened twice; towards night the pulse rose to 120; during the day it had not been so frequent; spasm in back and lower extremities still continues; he is particularly attacked when about to fall asleep.

Another Tobacco Enema has been administered, it remaining in the gut about fifteen minutes without producing any apparent effect.

10th.—Slept well; had this morning another purgative draught; stools of a better appearance; spasm lessened; he can bear his legs to be moved; still complains of pain and stiffness, particularly in the back; pulse 120, small and irritable.

Ordered a dose of Julepum Ammonia, and at night to have some gin and water, which is his favourite drink.

11th.—Gin produced intoxication, and was consequently discontinued; pulse 120, weak; bowels not opened; tongue dry.

To have ℥iss. of Oil of Turpentine in an enema, and take Julep. Ammoniæ, ℥iss. Spirit Ammoniæ, ʒj. bis et ter die.

12th.—Turpentine has produced several motions; pulse thready, small and irregular; pupils sluggish; pain in head; restless, and rather delirious; lips of wound hot and swelled; surrounding scalp inflamed; head to be kept wet with cold wash, and a purgative draught to be taken in the morning.

13th.—Left side of scalp affected with erysipelatous inflammation; pulse 132, small; bowels once opened; tongue dry and brown; skin hot; is able, with a good deal of difficulty, to turn in bed, but occasionally, on attempting to move, he is suddenly seized with spasm; this lasts for a short time, then disappears; during its continuance he lies like a log of wood.

℞ Pulv. Jalapæ, gr. xv. Hydr. Submur. gr. iv. statim, et postea. Ammoniæ Subcarb. ʒss. in statu effervescentiæ cum Succo Limonum sexta quâque horâ sumend.

16th.—Tetanic symptoms have in a great measure subsided; limbs free from pain, and can move them more easily; lower jaw not much fixed; erysipelas has slightly extended on the head and face; bowels opened; pulse not so frequent, and fuller; the erysipelatous parts have been punctured two or three times with the point of a lancet, with apparent benefit.

Contin. Medicamenta.

19th.—Has not had any return of spasm, and can now be got out of bed, and sit while it is made; erysipelas is slowly extending over the face, the left side of which is much swollen; complains of pain in the head; is restless; tongue dry and furred; pulse 120, feeble; wound in scalp discharges a good deal of thin curdy matter, and does not heal.

> Continue the Ammonia in effervescence, with Lemon-juice, and a purgative draught in the morning.

23d.—The whole of face affected with erysipelas; he is completely blind from the swollen state of eyelids, in which there is matter, with a little sloughing of the cellular membrane; there is an obscure fluctuation, with a doughy feel, at the angle of the jaw, on the right side; inflammation leaving the scalp, and spreading down the neck and chest; tongue dry and wrinkled; thirsty; slight diarrhœa; evacuations offensive and dark-coloured; pain in head, and delirious at times; there was a collection of matter under the scalp on right side of head, which has been punctured.

> To be allowed wine. Continue the Ammonia. Have a poultice to the eyelids. And for a lotion, Lotionis Spirit. ℥xvi. Ammoniæ Subcarbonatis, ʒiss. ft. lotio.

No return of spasm.

26th.—Sinking On the 24th he rallied a little, but is now worse; wound in scalp gaping, edges everted, and the bone may be seen bare all round the opening; deposition of pus in right cheek more evident; pulse 156, very small and weak; abdomen tender on pressure; diarrhœa continues; motions dark and offensive; tongue typhoid; black sordes have collected on the teeth; is taking wine, brandy, and ammonia.

27th.—Died this morning.

Examination 27 hours after Death.—Abdominal and thoracic viscera healthy; medulla spinalis, and its investing thecæ, natural; a considerable collection of matter under the scalp; the cellular membrane sloughy; the dura mater entire, and adhering firmly to the edge of the bone all round the opening in the skull. The pia mater, where it covers the medulla oblongata at the base of the cranium, of a darker colour than usual; the substance of the brain, generally, observed to be particularly firm and healthy; but within the middle lobe of the left hemisphere wholly within the medullary substance, and precisely opposite the opening in the bone, was found an abscess, containing about four drachms of healthy pus. The thalami and corpora striata were natural. N.,

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CAJEPUT OIL IN CHOLERA.

To the Editor of the London Medical Gazette.

Brighton, 30th August, 1831.

Sir,

A second severe case of cholera came under my notice in London, a few days since, in which one hundred and twenty-five drops of the cajeput oil, in three doses, were given with success.

The information on which I recommended this remedy did not rest, as you know, on the bare testimony of the servant, but was confirmed by his master, under whose direction he administered it.

In a letter, published in the Madras Gazette of the 20th October, 1825, the cajeput oil is mentioned as having been administered for the first time in a case of cholera in India: I cannot just now find the extract from this letter, but the newspaper may be seen at the East India House. It is singular that the cajeput oil is not known in the Bengal Residency as a remedy in cholera: this I find from the testimony of a gentleman who held a high station there, and of a medical officer, just returned after a service of eighteen years.

Should you have room to insert this in your valuable journal, it may lead to further inquiry.—I am, sir,

Your obedient servant,
M. J. Tierney.

APOTHECARIES' HALL.

Names of Gentlemen to whom the Court of Examiners granted Certificates of Qualification on Thursday, August 25, 1831:—

William Dixon, of Wakefield.
George Morley, Leeds.
William Platt.

NOTICES.

We have received a letter from "A Calm Observer," pointing out an illiberal attack in the Lancet upon Sir W. Burnett. The character of the journal in which it appears renders the refutation unnecessary.

Mr. Middlemore's paper has been delayed owing to accident; it shall appear next week.

W. Wilson, Printer, 57, Skinner-Street, London.

THE
LONDON MEDICAL GAZETTE
BEING A
WEEKLY JOURNAL
OF
Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 17, 1831.

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DR. PROUT'S REJOINDER TO DR. W. PHILIP'S "REPLY."

To the Editor of the London Medical Gazette.

SIR,

I OBSERVE in your last number that Dr. W. Philip has admitted that, at the conversation alluded to, "he expressed himself sorry that he had mistaken the meaning in which I had used the term metaphysics." Now this is principally what I meant to imply, and all indeed that I wished; for this mistake being admitted, a very large proportion of what the Doctor has brought forward both in the last and the present communication becomes irrelevant. I can assure the Doctor that had it not been for this accidental meeting (which, as far as I was concerned, ended amicably) he would have had much more reason to be offended than he has at present; for I had "dipped my pen in gall, and spared him not." What particularly annoyed me was, not so much the uncalled-for attack from a person with whom I considered myself on friendly terms, as the very offensive manner in which he contrived to pin to my back the "wearisome thrice-told tale" of his own discoveries, and thus to exhibit me in the ridiculous light of his *homme affiché.*

With respect to the term metaphysics, which seems to have been used in a variety of senses by different authors, (even metaphorically, if I mistake not, for any thing abstruse,) I shall be brief and final. The definition of the word, as given in my last communication, was taken from the only English dictionary that happened to be at hand, and represented, therefore, I presume, its general meaning. Now this meaning appears to me to be of such a comprehensive character, as fully to authorise the sense in which it was used in these lectures; and indeed so satisfied am I of this, that were I to engage in a work on Physiology to-morrow, I should not hesitate, if it otherwise suited my purpose, to class under this general head (appropriately subdivided, of course) every thing belonging to every kind and degree of those beings, agents, principles, or whatever else we may choose to call them, existing in organized beings, and which peculiarly distinguish them from common matter.

As Dr. Philip meditates another attack, which seems intended to demolish me and my chemistry altogether, I shall of course reserve all my powers for this awful occasion: but if his observations consist, like many of his last, of mere verbal refinements, or inferences from such, he will probably find my answer next week still less agreeable than my former letter. On the other hand, all objections to substantial statements or facts brought forward in the course of these lectures, I shall readily attend to, and if the Doctor can shew that I am wrong, as the truth, and the truth alone, is my object, I shall at once acknowledge my error.

I have always been an admirer of the Doctor's industry and perseverance, and wish that others of our profession would follow his laudable example in these respects. From the first I have felt great interest in his investigations, and as far as I could understand them, and they appeared to be well founded, have immediately adopted them, and shall continue to do so. Now what I wish in return is, that the Doctor will do the same by me, and condescend to understand a little of my chemistry. Whatever he may think of the matter, I can assure him that it will furnish him with a vast deal of curious and important information, which he will not be the worse for, and which he cannot hope to procure from any other source. Indeed, if he wishes his observations to have any weight with the world, he *must* follow this course, or otherwise shew that he knows a *little* at least of the subject on which he pretends to give an opinion.

In conclusion, I beg to remind the Doctor, and indeed all my readers, that these lectures, *the subject of criticism,* do not constitute a finished treatise, professing to be precise and perfect, but merely a brief and general sketch; nay, as printed, they do not even constitute an *entire* sketch, but only the broken outline of one—" the shadow of a shade*." Under these circumstances they must be necessarily very imperfect, and I can readily suppose that they appear much more imperfect to a stranger than to myself, on account of the absence of illustrations, &c. which are familiar to me, but, of course, unknown to him. Thus, for example, the circumstance alluded to by Dr. Philip in his last communication, that negroes are said to grow fat during the sugar harvest, was particularly noticed and explained in the lectures themselves, as many of my hearers no doubt will remember.

In the present number I find myself involved in another wordy dispute with Dr. Roberton. Fortunately for me, however, Dr. Philip is stated to be over head and ears in the same scrape; I shall, therefore, beg leave to cut the affair, and leave the two metaphysicians to settle the matter between them.

How refreshing to turn from this war of words to the valuable practical essay of Dr. Stevens, in your same number; an essay which, in spite of some imperfections, seems to me to contain the germs of discoveries of the last importance to mankind. I notice the subject from the Doctor's statement that the acid found in the stomach on these occasions is the muriatic acid, and I have little doubt that it is so occasionally, in part at least. I have elsewhere, however, advanced the notion that the acid may be the acetic, and I expect it will be frequently found to be this or some other acid connected with the saccharine radical. The argument mentioned by Dr. S., that the matter vomited has not a sour smell, can scarcely be deemed conclusive, as the merorganized acetic acid (lactic acid) usually met with in the stomach, is not volatile at ordinary temperatures, and consequently has little smell.

WM. PROUT.

Sackville-Street,
Sept. 12, 1831.

SOME OBSERVATIONS ON

AN ABSTRACT OF

DR. PROUT'S GULSTONIAN LECTURES,

Delivered by him at the Royal College of Physicians.

BY A. P. W. PHILIP, M.D. F.R.S. L. & E.

THE lectures of Dr. Prout, from the acknowledged talents and industry of their author, and the importance of their subject, have attracted in no common degree the attention of his medical brethren. Notwithstanding this favourable prepossession, it has been alleged by some, that the reader rises from their perusal without a clear conception of their author's meaning, and consequently without that conviction which sound premises and an able exposition necessarily produce. Such a result the author and the reader are very naturally inclined to ascribe to different causes.

Of the reader's intelligence and information we have no means of judging, nor are such means necessary. The author has laid his case before us, and if he is acquitted, the reader is condemned. We have to consider, then, how far Dr. Prout's premises are sound, his deductions legitimate, and his language correct.

As the first essential in all discussions is that the parties should understand each other, I shall in the first place call the reader's attention to the language of Dr. Prout. On this head I have already had occasion to make some observations.

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* Lest this expression should be understood as applying to our reports, we beg to say that the circumstances render it *impossible* that Dr. Prout should allude to the manner in which the portion of his lectures which did appear was given in this Journal.—ED. GAZ.

We have seen him using the term metaphysics in a sense not authorized by the usage of the language in which he writes, without defining the meaning he affixes to it; and called upon to define that meaning, he gives a definition which does not well correspond with the use he has made of the term. In his reply to my former observations, we have seen him using words as synonymous whose meanings have nothing in common; and including under the term mechanics, phenomena of the nervous and muscular powers.

The composition and use of a term introduced by Dr. Prout, and which may be regarded as a kind of introduction to the physiological part of his work, appear to me particularly objectionable. Such I should have considered the term merorganization, had he employed it only as the expression of a chemical fact, because it implies a knowledge of different degrees of organization, of which I can see no proof either in Dr. Prout's paper, or his lectures; but when employed for the physiological purpose to which he attempts to apply it, it involves him in considerable difficulties, without, as far as I can judge, in any degree answering the purpose he has in view.

The following account of the meaning of this term, and of his reasons for introducing it, is taken from his paper "On the Ultimate Composition of Simple Alimentary Substances," &c. in the Philosophical Transactions for 1827.

"It has been known from the very infancy of chemistry, that all organized bodies, besides the elements of which they are essentially composed, contain minute quantities of different foreign bodies, such as the earthy and alkaline salts, iron, &c. These have been usually considered as mere mechanical mixtures accidentally present; but I can by no means subscribe to this opinion. Indeed, much attention to this subject for many years past has satisfied me, that they perform the most important functions; in short, that organization cannot take place without them. This point will be more fully investigated hereafter; at present it is sufficient merely to observe, that many of those remarkable changes which crystallized bodies undergo on becoming organized, are more apparent than real—that is to say, their chemical composition frequently remains essentially the same; and the only points of difference which can be traced, is the presence of a little more or less water, or the intimate mixture of a minute portion of some foreign fixed body. There is no term at present employed which expresses this condition of bodies; and hence, to avoid circumlocution, I have provisionally adopted the term merorganized (*μερος, pars vel partim*), meaning to imply by it, that bodies on passing into this state become partly, or to a certain extent, organized."

To Dr. Prout's inference, that the presence of these foreign bodies, or a little more or less water, is essential to the change which he calls merorganization, there can be no objection, provided it rests on a sufficiently extensive and varied series of experiments; but this fact, if such it should prove, throws no light on the process by which the organic agent operates, or the nature of the connexion which subsists between the slight chemical change in the composition of the body, and the vast change in its sensible qualities; nor does Dr. Prout maintain that it has such effects.

This subject is strikingly illustrated by the very curious and important experiments of Mr. Herschel, to which Dr. Prout refers; and the more so, as the identity of the nervous and galvanic powers appears a necessary inference, from the facts stated in my first communication.

There are circumstances, however, which seem to suggest more than a doubt of the propriety with which Dr. Prout applies the term merorganization to any of the processes of the more perfect living animal.

Words which unfrequently occur in common conversation are often, when they do occur, employed very indefinitely. Thus the word organization is sometimes employed to express the functions, as well as the composition, of living parts; and sometimes their composition only. In the latter sense we apply it to dead as well as to living animal and vegetable substances. Now it is evidently in the latter of these senses alone that Dr. Prout uses it, because he applies it to starch, &c.

In this sense of the word the more perfect animals, I conceive, have no organizing power; they can exist only on matter already organized; and if some of the lower species of animals possess any *degree* of this power, which seems to be the case, we only see in this one of the instances in which they approach the vegetable, as we see in vegetable life plants, the mushroom tribe, possessed of no organizing power, and therefore, like the more perfect animals, nourished only by matter already organized.

If the more perfect animal were capable of organizing its food, like the vegetable, or, as it would appear, some of the more imperfect animals, it could live on air and water alone; for by the vegetable these are converted into the food of the more perfect animals. If Dr. Prout means to attempt the herculean task of tracing the steps by which unorganized becomes organized matter, it is to the vegetable, or, at most, the lower classes of animals, that he must look for the field of his labours; for it is in them alone that this change is effected.

Let the reader, keeping these facts in view, compare the observations of Dr. Prout, which have been quoted from the Philosophical Transactions, with the paragraph marked 2, in the thirteenth page of his lectures, and endeavour to reconcile these passages with each other, and both with the facts just stated. Dr. Prout observes, in the passage just quoted from his paper, that of such consequence are the "minute quantities of foreign bodies," "that organization cannot take place without them;" and again, in the fourth page of his lectures, "that organization cannot exist without them." They, therefore, are received into the stomach with the organized food, on which alone the animal can live. Yet in the passage of his lectures just referred to, we find the following sentence:—"From long attention to the subject I am satisfied that the merorganizing principles are chiefly derived from the living animal itself, at least the more essential ones, and that they are contained in those products of secretion furnished by the stomach and other organs." If the stomach merorganizes the food, it must have a disorganizing power, because the food is wholly organized when it enters it. It will be admitted, that no organization can be more perfect than that of the muscular fibre. Dr. Prout's attempt to remove this difficulty rests on too narrow a basis, and were it correct, would not warrant his use of the term merorganization.

Were there room in such a communication as the present to pursue this part of the subject, we should find similar difficulties in other passages, particularly in those relating to crystallization; but I must hasten to the more important parts of my task, and shall close this part of it by the following general observation. In the chemical department of the work Dr. Prout appears at home. His style is simple and correct, and his observations clear; but in proportion as it becomes physiological, it becomes obscure and perplexed. The author talks less of what has actually been done than of what he conceives will in future be done, and dwells on expectations which I shall endeavour to show can never, in the nature of things, be realized.

I am now to enter on the question at issue between Dr. Prout and myself, namely, how far chemistry is applicable to explain the phenomena of the living animal, and assist us in the cure of its diseases.

In the eighth page of his lectures Dr. Prout observes, "The physiologist, in order to obtain the utmost advantages that chemistry is capable of contributing, must turn chemist himself, and carry on his researches in connexion with the phenomena of life, of which he must never lose sight for a moment." But the circumstance of the same person studying both sciences will not form these sciences into one. They will equally remain distinct sciences. In our language we may disregard the great landmarks of nature; but there are her landmarks notwithstanding.

Chemistry, and the science of the vital functions, are of so different a nature, that if they be pursued with ardour, and without this nothing can be done in such subjects, the one will tend constantly to abstract the mind from, and perhaps in some degree to unfit it for, the other; so that the one or the other will more or less absorb the inquirer, according as his peculiar turn of mind or accident inclines him.

In this subject, as in almost all others which are open to the public, we find the most common view of it the most correct. Physiology and chemistry will always, I believe, be regarded as distinct sciences. They cannot, as Dr. Prout supposes, so coalesce as to form parts of a whole, because there is a chasm between them which no labour will ever enable us to fill up. I cannot, therefore, agree with him that the physiologist should himself become chemist, farther than is necessary to enable him to watch the labours of the chemist, and select from the results obtained by him, such as suit his purpose. To be thoroughly acquainted with the vast science of modern chemistry, and keep pace with the rapid discoveries which every day add to its utility and importance, he must make it his chief business, to the neglect of what more particularly belongs to his own department.

As civilization increases, the sciences as well as the arts extend, and the necessity and advantages of the division of labour increase in the same proportion. In times when we find it necessary to create so many artificial divisions of labour, it would surely be unwise to attempt to destroy those which nature herself has established, and which have consequently at all times existed. Dr. Prout admits the operation of what he calls the organic agent in the functions of the living animal. What would he say to the chemist who should admit of such a principle in his reasonings; and what would he say to the physiologist who should exclude it in his? Now before he can speak of physiology and chemistry coalescing and forming a whole, he must either be the chemist who admits or the physiologist who excludes this principle.

The same prepossession in favour of chemistry which induces Dr. Prout to propose that the physiologist should become chemist, produces an error of greater consequence; which runs through the whole of his reasoning. In speaking of the phenomena of animal life, he keeps only in view their chemical department. Now, although chemistry may unfold the changes which take place in individual processes, and thus greatly aid the physiologist, farther than this, as far as I am capable of judging, it is powerless in investigating the laws of the vital functions, and ever will remain so.

Let us take the stomach, of which Dr. Prout says so much, as the subject of our illustration. He may ascertain what chemical changes are effected in it, but can he, by any chemical investigation, tell us the food which is most grateful to its nerves, and consequently best fitted to excite their action, or that which best supports the muscular action of the stomach itself? For on these, as much as on the chemical properties of the food itself, its digestible quality depends: oily articles are known to be of difficult digestion, yet the fat of bacon toasted is, under certain circumstances, more digestible than the lean of mutton or venison, the most digestible of all food.

We shall suppose that Dr. Prout has, in any particular case, determined every chemical change the stomach effects on the fluids of the body, as well as on the food; he will still be incapable of telling us what food, in any particular instance, is most digestible; because this often depends on circumstances on which chemistry can throw no light. In the introduction to his lectures he justly says, indeed, that in applying chemistry to physiology it is necessary to take into the account the living as well as the chemical powers engaged; but when he comes to make the application, it is still either the chemical powers alone which he keeps in view, or, if the living powers be taken into the account, it is only for the purpose of considering what chemical change is effected by them.

We have not only to consider what food is most soluble in the gastric juice, but what food is most capable of exciting a healthy secretion of this juice; and although the gastric juice be healthy, and consequently capable of effecting the due chemical changes on the food, the function of the stomach may, notwithstanding, be wholly deranged, and no proper chyme transmitted to the duodenum. If that portion of the food which has undergone the action of the gastric juice be not duly separated and kept apart from that which has not, and in this separated state duly carried forward towards the intestine—if an error in any one of these functions take place, the office of the stomach is just as certainly deranged as if the chemical part of the process had failed. Yet, in Dr. Prout's observations on the office of the stomach, the chemical part of it alone is referred to, as if it were in this alone it consisted.

He professes to give us a summary view of that part of the process of digestion which is performed by the stomach, while his attention is wholly confined to the chemical changes which take place in the food*: neither referring to the thousand causes which influence the secretion and consequently the qualities of the solvent, nor to the circumstances which influence the position and motions of the mass formed by the union of the solvent and the food; each of them as essential as that union itself. One might as well attempt to explain the nature of the circulation by detailing the chemical changes which take place in the blood.

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* Page 12, et seq.

Prepared as the reader must now be for Dr. Prout's partiality to his favourite science, he can hardly be prepared for the following prediction in the ninth page of his lectures. "I will venture to predict that what the knowledge of anatomy at present is to the surgeon, in conducting his operations, so will chemistry be to the physician, in directing him generally what to do and what to shun." Does not the surgeon guide his knife solely by a knowledge of anatomy —a knowledge of the parts, and their relative position? Will it be asserted that, let our knowledge of chemistry be as perfect as we can suppose it, the physician could regulate his practice on chemical principles alone? Could chemistry give him any knowledge of even the simplest principles of medical treatment? Could it tell him, for example, that a copious discharge from the bowels will relieve the turgid vessels of the brain, or that that by the skin will allay the symptoms of fever? But Dr. Prout may say it will assist in his choice of the means by which these effects are produced. Undoubtedly it will. Here chemistry is in its proper place: but of what use would the chemical knowledge here be, if the laws peculiar to the animal economy had not, in the first instance, supplied the indication of cure?

On the other hand, let us suppose for a moment that the nature of the animal system is in all respects such as would suit the views of Dr. Prout; that chemistry is to the physician all that anatomy is to the operating surgeon; that all the functions are of a chemical nature, and consequently that all diseases arise from some error in the chemical composition of the whole system, or some part of it; for all this is necessary, in order that chemistry should be to the physician what anatomy is to the surgeon. I say, on the supposition that all this is the case, what opportunity has the chemist of ascertaining the numberless chemical changes which take place in disease? Can he operate on the internal organs and their juices as on an animal or vegetable product, separated from the organic body which produced it, and wholly and conveniently subjected to his art?

But let us examine more particularly the application of Dr. Prout's doctrines to explain the nature and regulate the treatment of disease.

HERE, as in his view of the healthy state of the body, he forgets that the chemical changes which take place in it are but the subordinate parts of its various functions. If the physician can maintain the due action of the living solids, the proper chemical changes necessarily follow, whether he be acquainted with them or not; and although a knowledge of these changes may now and then suggest a remedy, the great source of practical knowledge is the effect of our means on the vital functions themselves; and after all that the chemist can do, to this test must his suggestions be subjected before their value can or ought to be admitted by the physician. It appears, from what was said in my last communication, how fallacious mere chemical inferences respecting the functions of the living animal may be.

One of the greatest errors, as far as I am capable of judging, in Dr. Prout's pathology, is, that his principles lead him chiefly to look to the state of the fluids; whereas we have reason to believe that, in most instances, the changes in them are merely the consequence of some fault in the action of the vital organs on which their state generally depends. Could we correct the fault of those organs, in what Dr. Prout considers the saccharine, oleaginous, and albuminous diseases, the fluids would quickly be restored to the healthy state, from which they would as quickly again deviate on a return of the morbid action of the organs.

The tendency of his observations is to bring us back to the humoral pathology; the subversion of which, due chiefly to the labours of Boerhaave, Hoffman, and Cullen, must be regarded as the greatest improvement of modern medicine. His humoral pathology, indeed, would be very different from its predecessor, but, applied in the way he attempts, must necessarily lead to many of its worst evils. Whatever be the source of disease, to the state of the living solids we must look for the remedy: if their condition be right, that of the fluids will not long be wrong;

if not, there are no means capable either of restoring or preserving the healthy state of the latter.

In the solids the moving powers reside, and on them consequently all the changes of the system depend. By their healthy action, what is injurious or superfluous in the fluids is thrown off, and what is defective is supplied.

As the means of cure in disease operate chiefly on the solids, not the fluids, of the body, they are often such as have little or no relation to the state of the latter. Let us take, as an illustration, the simplest case, and that of most frequent occurrence. The powers of the stomach are weakened, the patient complains of acid eructations, and his breath and perspiration are morbidly impregnated with acid. By alkaline medicines this state is corrected, but constantly recurs on laying aside the antacid. It is finally corrected by such means as restore the powers of the stomach; but these means possess no corrective power with respect to the acid, which was the prominent symptom of the disease.

If the debility of the stomach happen not to be an original affection, but depend on that of some other organ—of the liver, for example—the means which give vigour to the stomach would, like the antacid, afford only temporary relief. To render the relief permanent, the original disease must then be corrected. The cure could only be effected by restoring the due action of the liver; and we might, perhaps, find an acid, instead of an alkali, the proper means. So fallacious would a chemical theory, built on the best possible evidence of the state of the fluids, such an evidence as we cannot have in one of a thousand cases, and suggesting so ready and perfect a means of correction, prove; and equally fallacious must prove all pathological reasoning founded on the same principles.

If, instead of the predominance of an acid, the fault had been in the prevalence of the saccharine, the oleaginous, or albuminous principle, although the proper treatment would have been less evident, it is only on the same principles that it could have been permanently successful; for if we could discover means of correcting such states of the fluids by their operation on the fluids themselves, which seems to be the aim of Dr. Prout, we should gain nothing more by them than was gained by the alkali in the case just stated.

These, like every other permanent error in the fluids, must depend on a morbid state of one or more of the vital organs; because if their action were healthy, they would soon throw off the offending cause, and while their morbid action continues, however perfectly for the time that cause might be corrected; on laying aside the means, the fluids would immediately relapse into the same state; and the means of permanent cure would have no more relation to the morbid state of the fluids, than in the case of the indigestion, and therefore could never be suggested by the state of them.

We know that, in diabetes, there is a prevalence of the saccharine principle in the urine; but has this knowledge enabled us to cure the disease? I have, within the last twelvemonth, had the satisfaction to see a case of diabetes mellitus cured, and, as far as I yet know, permanently (the patient has remained well for about six months), by minute mercurial doses, given in the way I have recommended in my Treatise on Organic Diseases, combined with other means of strengthening the digestive organs; but none of these means had any particular relation to the state of the urine. When the healthy action of the organs was restored, the morbid state of the fluids, as a necessary consequence, was corrected.

Dr. Prout, in the nineteenth page of his Lectures, observes, referring to the morbid prevalence of the different principles stated by him, " But I need scarcely remark that it seldom or never happens that one principle alone is affected without involving, in a greater or less degree, the others," a sufficient ground of presumption that these changes are the effects of a common cause; which, I believe, I may say, will always be found to be the derangement of one or more of the vital organs, and can only be permanently corrected through them; and consequently by means which can never be either suggested or regulated by an exclusive attention to any particular train of their consequences.

As far as these consequences themselves tend to aggravate the symptoms, their correction will of course be beneficial, and a knowledge of their tendencies, therefore, is most desirable; but in founding our general plans of treatment on them, we mistake the consequence for the cause of the evil, and our practice must necessarily be unsuccessful.

Such are the principles which assure me that a pathology founded chiefly on the state of the fluids, which a chemical pathology must always be, cannot fail to be erroneous. Their state, depending on that of the living powers, varies as it varies. The laws of these powers, therefore, and particularly the manner in which they are influenced by our various means of cure, must always be the chief objects of the physician. Dr. Prout's partiality for chemical research, in which he so greatly excels, as far as I am capable of judging, in many ways deceives him. Its tendency is to narrow our views of the animal economy, and lead in pathological reasonings to a neglect of the laws peculiar to it; which have nothing in common with those of chemistry, but on which the phenomena of disease, as well as of health, must always more or less directly depend.

Dr. Roberton's paper in the last number of the Medical Gazette, in which he comments on a supposed opinion of Dr. Prout and myself, proves that he has reflected much, and not unprofitably, on the part of the subject to which it relates; yet it might be shewn, perhaps, that his metaphysics are not wholly invulnerable; but on this part of the subject I have no intention of entering farther than is necessary to explain the sense in which I myself use the term organic agent. That there is a peculiarity in the processes of the living animal and vegetable, all admit; and consequently some cause or principle operating in their constitutions, which is peculiar to them. This something, as far as relates to the animal, has pretty generally obtained the name of vital principle. It is in this sense alone that I have on the present occasion adopted from Dr. Prout's treatise the term organic agent. I here prefer the word organic to vital, because, without any violence done to the ordinary meaning of the word, it may be applied indifferently either to animal or vegetable life; nor do I see the force of Dr. Roberton's objection to the word agent, used in the sense just explained, because, whether we use agent, principle, or any other expression, an agency is equally implied.

In the following sentence, which I quote from his paper, I conceive that he mistakes the meaning of Dr. Prout's expression. "The organic agent must, of course, rank in some grade intermediate, not intelligent, and yet not non-intelligent; yet nearer the intelligent than the non-intelligent, possessing a faculty only little short of intellect." Dr. Prout's expression is, "a faculty little short of intelligence." In this expression, as I understand it, Dr. Prout, so far from ascribing intelligence to the organic agent, denies that it possesses any degree of this property. Dr. Roberton is acquainted with the vulgar proverb, that a miss is as good as a mile. If we say that a man was little short of falling into a river, we as certainly inform the hearer that he had not fallen into it, as if we had simply announced the fact. All I understand by Dr. Prout's expression is, that such are the effects of the agency in question that they suggest the idea of intelligence.

It is curious to observe the simplicity of nature's means in the production of phenomena which, at first view, seem the most unintelligible. "In the germination of a seed," Dr. Roberton observes, "he (Dr. Pritchard*) thinks that all the early phenomena are partly mechanical, and partly chemical; but that the processes which follow the development of the young plant, as when the plume ascends between the cotyledons, and the radical strikes downwards, point to the operation of a higher power than merely mechanical principles; meaning, I presume, by this higher power, the immediate agency of the Deity."

Now the phenomena here selected as the most unintelligible in the process of vegetation, the beautiful and conclusive experiments of Mr. Andrew Knight have explained on the simplest, and therefore most satisfactory principles. He has given an unanswerable proof of the accuracy of his views, by causing both the plume and radical to shoot in any direction required, and even in his first experiment foretelling, with perfect accuracy, the direction in which they would shoot.

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* See Dr. Pritchard's work on the Vital Principle.

DR. VENABLES ON ABORTION.

To the Editor of the London Medical Gazette.

Sir,

A correspondent in your last number, who writes under an initial signature, (whether real or fictitious it is not of much importance to inquire) seems to doubt the correctness of, and wishes me to retract, the proposition which I have laid down—"that there is no known medicine capable of producing abortion, &c." I certainly acknowledge that I for some time felt it a question whether I ought not to have withheld the passage rather than present it for insertion in your Journal: not, however, because I entertained the slightest doubt as to the truth of the statements, but because I felt I was open to some degree of censure for venturing formally and gravely to propound to the scientific readers of your Journal a proposition the truth and correctness of which, perhaps, there could hardly be found a practitioner to dispute. Hence, then, I hesitated; because the fact, so far from being a valuable or interesting piece of information, (so far as the professional readers of the Medical Gazette are concerned) is to be looked upon rather as a work of supererogation; and the only excuse I can offer is, that which will be confirmed by a reference to the passage—namely, the expectation that the public press would, as far as in their power, circulate among the vulgar and the less-informed classes of society the delusion under which so many of them labour, so as to prevent the fatal—too often doubly fatal—consequences, and the penal responsibilities incurred in attempting a crime, in the perpetration of which it is almost a miracle if they succeed.

I know not whether your correspondent be a medical practitioner, or a mere general reader, anxious for more explicit information, and desirous of higher authority. If the former character, I beg to submit the following question for his consideration. If, in the course of his obstetric practice, he were to meet with a woman of so deformed a pelvis that it was impossible she could have a living child, or even go on to an advanced stage of pregnancy without endangering her life; and that, therefore, the premature and early expulsion of the ovum became not only a legitimate object, but an essential to the preservation of his patient, to what internal remedy would he look for the accomplishment of his purpose? I myself know of none; nor in the systematic arrangements of the materia medica can I discover a class of "*abortives*." Nor should I expect the slightest advantage, or anticipate the most distant chance of accomplishing such an object, by any appeal to the powers of the secale cornutum, or ergot of rye, which I take to be the American remedy alluded to. I know that this drug is often administered to quicken the tardy and languid operations of the uterus, and no doubt, in some instances, with advantage. But there is a wide difference between assisting, and wholly preventing the natural functions of the organic system. The languid uterus may be excited to a more vigorous performance of its natural functions by a proper and judicious exhibition of such a medicine; but from what analogies can we infer that a natural operation is to be perverted or arrested, and an unnatural one substituted in its stead. Such may be the result of accident, of idiosyncrasy, &c. which are but mere solitary exceptions to the general principle; and as yet our knowledge has not developed the means of rendering them more frequent, or of bringing such unnatural perversions under the more certain controul of medicinal agents. Ergot of rye, in poisonous doses, for it *is a poison*, may, like any other individual of the class, produce abortion; but it is twenty thousand to one that it rather produces the death of the mother; and when it induces abortion, it is double that, that it also subsequently induces the death of the parent. I could have wished that I had been spared any allusion to ergot of rye, because I am reluctantly compelled to advert to the abuses in its administration. Why does the hurried accoucheur resort to its use? not because he conceives the tardy operations of the languid uterus require the gentle hint conveyed through the stimulating agency of this medicine, but because the natural action of this organ is not in the due proportion to his impatience, nor in a ratio of celerity adjusted to the number of his other engagements.

If, on the other hand, your correspondent should be a general reader, desirous of more explicit information, and of reference to other authorities, I beg to observe that I should not have asserted, in such decisive terms, a fact which was not the result of my own personal observation and experience, without reference to authorities, unless, indeed, as in the present case, it were a mere work of supererogation. Had I an opportunity of making the observation to the judge, so as to have secured the necessary publicity through the press for the advantage of that class of society for whose benefit it was intended, I had never put you to the trouble of inserting it in your Journal; but as the accused *pleaded guilty*, there was no examination of witnesses, and consequently no opportunity of the above description occurred.

Now, for the satisfaction of your correspondent, I beg to cite the following authorities, with such references as occur to me on the moment.

" Every woman who attempts to promote abortion, does it at the hazard of her life."—*Bartley*.

" There is no drug which will produce miscarriage in women who are not predisposed to it, without acting violently on their system, and probably endangering their lives."—*Male*.

" It has frequently occurred that the unhappy mother has herself been the sacrifice, while the object intended has not been accomplished."—*Dr. G. Smith*.

" But we must conclude that there is no medicine, or abortive means*, which always produce abortion, and nothing but abortion; there is none which does not endanger the lives of the mother and infant."—*Ryan*, p. 154.

" Now we shall have occasion hereafter to show that medicines, internally administered, can seldom produce abortion."—*Paris and Fonblanque*, vol. iii. p. 90.

And again—

" From a very early period attempts have been made to devise means of procuring abortion, by the administration of certain drugs, which were considered as capable of acting specifically upon the womb, and of occasioning the exclusion (expulsion?) of its contents. It would be idle to enumerate the various substances which have, at different times, been employed for such a purpose, not a few of which were derived from the fertile sources of credulity and superstition; and yet we are bound to admit, that upon this occasion, at least, credulity has proved a blessing to mankind, by suggesting the substitution of a harmless amulet, or an inefficacious drug, for an application of extreme violence and danger, and perhaps of death. *The physicians of the present age disclaim the existence of any specific class of abortives;* but we are ready to admit that the administration of violent medicines, by *involving* the *uterus* in the *general shock* thus given to the system, will occasion abortion *provided there exist* at the same time *a certain predisposition* on the part of the female: should this latter condition, however, be wanting, the *poculum abortionis* may, by the violence of its operation, *destroy the life of the unhappy mother*, or very materially injure her, without *accomplishing* the object for which it was administered. In the case of *Mrs. Robert Turner*, one of the persons poisoned by *Elizabeth Fenning*, notwithstanding the long and violent sufferings she had experienced during her pregnancy, brought forth a living child at the natural period."—*Paris*, vol. iii. pp. 93, 94.

Mr. Burns also remarks, " it has been observed that those purgatives which occasion much tenesmus will be more likely to excite the expulsion of the ovum. The strong cathartics, however, which are sometimes taken to promote such an effect, act not only by exciting tenesmus, but likewise by inflaming the stomach and bowels, and thus affect the uterus in two ways. " It cannot be too generally known," he adds, " *that when these medicines do produce abortion, the mother will seldom survive their effect.*"

Such are some of the authorities by which I would confirm the fact which I have advanced—a fact perfectly consistent with my own personal observation and experience—an experience, too, upon all the phenomena of pregnancy, and the principles of obstetrics, by no means limited, and possibly but little inferior to that of any man of my age and standing in the profession.

Robert Venables.

Chelmsford, August 31, 1831.

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* The respectable author here has not been sufficiently precise; because " abortive means" will include those mechanical means to which obstetric practitioners resort for the sole purpose of abortion, or, in fact, premature labour at a very early period, and which are not considered as materially endangering the life of the parent.

OIL OF PEPPERMINT IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

AT a period when the subject of cholera continues to be so freely discussed, the mere mention of cases terminating in a favourable manner no doubt contributes in some degree to relieve the strong apprehensions entertained by the public as to this horrible disease. The cholera that is common to this country is generally of a mild character, and readily yields to the power of medicine; but occasionally the practitioner is called upon to witness its appearance in a highly aggravated form, threatening defiance to his skill, and almost setting at nought his best-concerted schemes. Happily it does not very frequently assume an alarming aspect, yet it occurs sufficiently often to make us desirous of seeking out a remedy which would render it more manageable in our hands, and by which we might be enabled to mitigate this affliction of suffering humanity.

In the perusal of your hebdomadal, I find that cajeput oil is strongly recommended in the present epidemic; and emanating from so high a source in the profession, it has certainly descended to us impressed with the stamp of authority. But although its efficacy has been illustrated in a few individual cases, it requires the test of experience to prove that it possesses properties of a specific nature. It appears to me that its influence is dependent upon an aromatic stimulating principle therein contained; and if so, the question might be legitimately proposed, why should not cinnamon, peppermint, or any other of the strong essential oils, be as efficacious in their operation as the one in question, since similar properties have been assigned, by the writers upon therapeutics, to them all?

During the last few weeks, cholera having prevailed to some extent in this neighbourhood, I have administered the oil of peppermint, in two or three severe cases, with the most satisfactory results. In one case, where the patient was attacked with diarrhœa and vomiting of a greenish watery fluid, succeeded by violent spasmodic action of the muscles of the lower extremities, and a constant disposition to syncope, it seem to operate like a charm; and although the symptoms returned at repeated intervals, they were uniformly relieved by the exhibition of this powerful antispasmodic. The dose usually given was from eight to ten mimims of the oil, in an ounce of the cretacious mixture, every three hours.

If these few passing remarks should be considered as meriting a place in your highly interesting and valuable periodical, by inserting them you will oblige

Your very obedient servant,
CHARLES THORNHILL.

Darlaston, Staffordshire,
September 2, 1831.

OBSERVATIONS ON THE SHARK (SQUALUS); THE ANATOMY OF ITS EYE; ITS MUSCULAR IRRITABILITY, &c.

By GEORGE BENNETT, ESQ.
Member of the Royal College of Surgeons, &c. &c.

(With Woodcuts.)

SHARKS are formidable from their muscular strength and the numerous rows of teeth with which their expansive and powerful jaws are armed: they may be considered as the most destructive and voracious of all the inhabitants of the deep. Their stomachs, which are of enormous capacity, are generally found filled with a mixed collection of substances, some of which seem calculated to try the strength of their digestive powers. It does not appear that their sense of smell always guides them in procuring food, as paper, canvas, or indeed any thing thrown overboard which they are capable of swallowing, is greedily devoured by them. To decide correctly on the *habitat*, or extent of range, taken by any particular species, is difficult, and requires a numerous collection of facts. Frequent mistakes are made in this respect; as in the instance of the squalus cornubicus, or porbeagle shark, whose *habitat* was supposed to be confined to the British coasts, but which I have since discovered has been found, although rarely, inhabiting the coasts of New Zealand.

The genus is thus defined by Linnæus, and the species composing it are very numerous:—

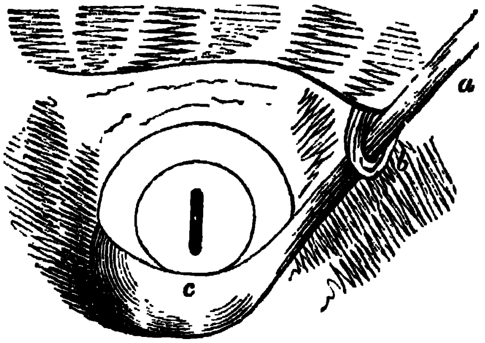
Squalus.

Caput obtusum. . . . . . *Spiracula* 4-7. ad latera colli, semilunaria. . . . . *Oculi* oblongi, verticales, dimidia parte tecti, ante foramen temporale. . . . . . *Os* in anteriore et inferiore capitis parte, pluribus dentium serratorum, acutorum, partim mobilium, partim fixorum, forma inæqualium armatum.

Corpus oblongum, teretiusculum, tenerrimis aculeis asperum.

Pinnæ ventrales, pectoralibus ut-plurimum minores, approximatæ, circa anum, et in mare circa genitalia positæ.

On the 29th of September, 1828, one of the species named squalus carcharias, or white shark (which is the most widely-distributed species known) was captured in latitude 11° 56′ north, and longitude 22° 30′ west. This specimen was small, being not more than six feet in length; but I have seen some of this species taken which have measured ten, and in one instance fourteen feet in length. I availed myself of the opportunity thus offered to examine the structure of the eye in this fish, and the following account is the result of my observations, subsequently confirmed by frequent dissections of the eye in this as well as in other species. The outer coat surrounding the eye is a continuation of the rough and dense integument of the animal; this being removed, the eye is seen embedded in a gelatinous substance, and seems to be capable of a great extent of motion in its socket. The muscles belonging to the eye in this animal are, the levator squammæ oculi, four recti (of which two are lateral, one superior, and one inferior), and two oblique. The most external is that muscle which, from its use, has been named levator squammæ oculi; it arises from the upper and back part of the temporal region, passes downwards and forwards, and in its course passes through a small loop, to which it is attached by loose cellular membrane; this loop arises from a process from the orbit; the muscle passes down, and is inserted tendinous into a cartilaginous cup-like covering. The action of this muscle is to draw the covering over the eye; the use of this covering, however, is not yet explained; it seems analogous to the *membrana nictitans* in some other animals; but why it is found in the shark and not in other fish, it is difficult to conjecture: even the ray, which, in the structure of its eye, so closely resembles the shark, is destitute of it. By some it is supposed to act as a lower eyelid. The following rough sketch is intended to shew this part, and its situation with respect to the eye-ball.



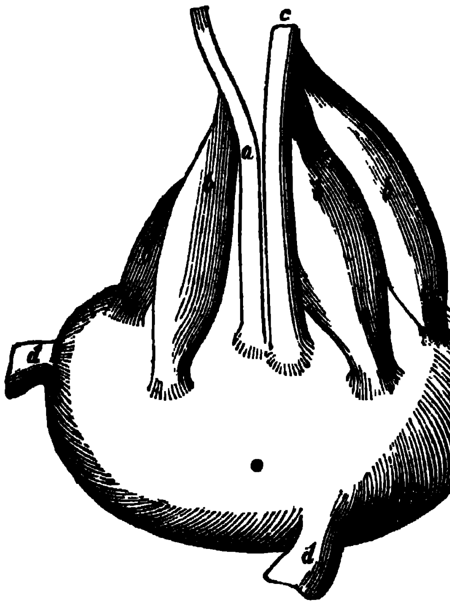
A, the levator squammæ oculi muscle.

B, the loop through which the muscle passes.

C, the cartilaginous cup-like covering.

The recti muscles arise from the cartilaginous stem at its connexion with the orbit, as well as, in a slight degree, from the orbit itself; passing down they diverge, and are inserted tendinous into the sclerotica. The oblique muscles have their origin from the anterior part of the orbit near together; they are the superior and inferior — the superior passing obliquely downwards, the inferior obliquely upwards; and they rotate the eye on its axis. Dr. Clarke Abel observes, that, " in ordinary action, they probably affect only the articulation of the cartilaginous stem with the globe of the eye; but where the greatest extent of motion is requisite, they may also affect its articulation with the socket."

The next part of the structure of the eye to be noticed is the cartilaginous stem, which is found also in the ray, and, I believe, in all the chondropterygii or cartilaginous fish, without an exception. Those in which it is found to exist have a great mobility of that organ, and in those destitute of it the motions of the eye are very confined. This stem is a dense cartilaginous body, nearly cylindrical, attached to the socket of the eye, and to the centre of the ball, by its rounded extremities received into a cup-like cavity, to which they are confined by capsular ligaments; it passes down at a very short distance from the optic nerve, to which it is attached by cellular membrane, and is close to it as it enters the eye-ball. This stem keeps the eye on a fixed point, and is considered by Cuvier to act as a lever for the muscles. Dr. Clarke Abel considers that it "seems rather to give extent than power to the action of the muscles;" and he further observes, that "this organization seems necessary in the shark, which takes its prey by turning on its back, to enable it to keep its object in view when preparing to seize it." The following cut, from my preparation, shews the situation of these parts.



a, the cartilaginous stem.
b, *b*, *b*, *b*, the recti muscles.
c, the optic nerve.
d, *d*, portions of the oblique muscles.

In the eye of this species of shark, I remarked that the pupil was perpendicular; in the blue shark (squalus glaucus, Linn.), of which, however, I had only an opportunity of observing one, the pupil was round.

In the nostrils of the shark there is a pectinated structure, on which the olefactory membrane is placed, and over which the olefactory nerves are distributed. This is subject to disease, resulting from the irritation of animalculæ. I found a specimen of the squalus glaucus, or blue shark (which was captured on the 5th of January, 1831, in latitude 30° 1′ south, longitude 41° 25′ east), diseased in this part; the morbid appearances resembled tumors, and contained a quantity of *epizoæ*. An account of this disease and the epizoæ will form the subject of a future paper. The stomach and intestines of sharks, as well as of other fish, have often several species of animalculæ attached to them.

The teeth of sharks vary in different species. Blumembach observes, that "in most of the sharks the mouth is furnished with very numerous teeth, for the supply of such as may be lost. The white shark has more than two hundred, lying on each other in rows, almost like the leaves of an artichoke. Those only which form the front row have a perpendicular direction, and are completely uncovered. Those of the subsequent rows are, on the contrary, smaller, have their points turned backwards, and are covered by a kind of gum. These come through the covering substance, and pass forward, when any teeth of the front row are lost. It will be understood from this description that the teeth in question cannot have any fangs*."

The shark, no doubt, sheds its teeth at certain periods, and the posterior rows are to supply, in succession, the places of those so lost, as, in a number of jaws that I have examined of different species, the second row may frequently be seen in a perpendicular direction, advancing to supply the place of the first. With respect to such teeth having fangs, those of most species have merely rudiments of them, excepting the squalus cornubicus, or porbeagle shark, which has two distinct fangs to every tooth, and they may be seen in the second and third as well as in the first rows. The posterior rows having their points turned backwards, prevent their prey, when seized, from escaping. The teeth of the shark are used by the Polynesian natives, fixed in rows, as knives. They are attached also to their spears, are used for cutting themselves on occasions of joy or grief, and were employed, previously to the introduction of European knives, for the ornamental carving of their weapons, domestic uteusils, &c.

There is a species of shark at New Zealand which I have heard named by seamen the *ground shark*: the teeth procured from this species differ from all others that I have seen; they are long, rather curved inwards, flattened anteriorly, sharp pointed, *unserrated at the edges*, and have *two rather long fangs*. They are considered rare at New Zealand, and the teeth are highly valued by the natives, who wear them, with a hole bored through them, as appendages to their ears; they carve their green jasper stone also in the form of these teeth, and wear them in a similar manner: these teeth were so highly prized by the natives, that to procure one was a matter of difficulty. I for some time, since my return to England, endeavoured to ascertain the species to which these teeth belonged, but I was unsuccessful, until lately examining the jaws of the various species of sharks in the Museum of the Royal College of Surgeons, in London, I found it to be the squalus cornubicus, or Porbeagle shark, which is thus noticed in the published catalogue of the College:—

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* Blumenbach's Comparative Anatomy, by Lawrence and Coulson, page 76.

No. 1832.—The skull, and part of the spine, of a small porbeagle shark. Squalus cornubicus. Fig. Borlase's History of Cornwall. Habitat. The British seas.

This species, from its magnitude when full grown, has sometimes been confounded with the squalus carcharias, or white shark. — Presented by Dr. Leach, 1820.

The fore-teeth, near the symphisis, accorded in every respect with the New Zealand specimens; more posterior they became equilateral, but were all unserrated at their edges.

I have now to relate an extraordinary coincidence respecting the teeth of this species, which has been found inhabiting the British seas and the coasts of New Zealand. In a collection of fossil specimens in the possession of F. R. Gowar, Esq. I was surprised at perceiving some teeth resembling those of this species, which had been found imbedded in an argillaceous soil, during the formation of the Highgate Tunnel, in 1809 and 10. The enamel of these teeth had become of a light blueish colour, but the fangs were not so perfect as in other specimens in the same collection, which, as well as having the fangs quite perfect, were of much larger size. These latter specimens were supposed to have been brought from some part of Somersetshire, and the enamel had also the blueish tinge similar to those procured from Highgate. The following figures show the form of the teeth. Fig. 1. The tooth of the porbeagle shark, drawn from the specimen in the Museum of the Royal College of Surgeons. Fig. 2. A lateral view of the same. Fig. 3. The tooth from New Zealand. Fig. 4. A drawing of one of the fossil teeth in Mr. Gowar's collection.

Fig. 1.

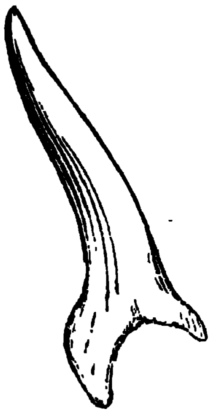


Fig. 2.

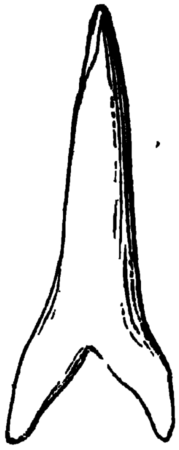


Fig. 3.

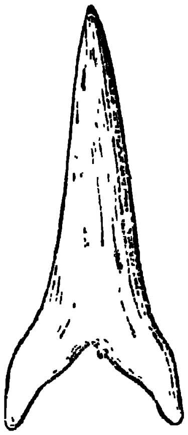
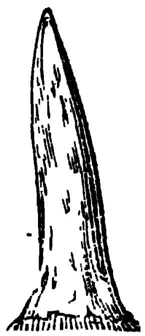


Fig. 4.



The teeth of the squalus monensis, or Beaumaris shark, are similar to the preceding, except in having at the base two sharp processes. The following is the description of the squalus cornubicus, or Porbeagle shark, from Shaw's Zoology, Vol. V. Part 2, pp. 349-50, but the teeth are not described:—

"This species is slightly described by Mr. Pennant in the British Zoology, from an engraving in Borlase's History of Cornwall, which was copied from a drawing by the Rev. Mr. Jago, minister of Loo, in Cornwall. A specimen observed in the year 1793, by the Rev. Mr. Goodenough, on the coast of Hastings, is described in the third volume of the Linnæan Transactions. Its length, from the tip of the snout to the extremity of the tail, was three feet ten inches; the colour of the body a deep blue on the back, and white or silvery beneath; the shape was round, except for about six inches from the tail, where it was depressed; at about an inch from the tail was a semicircular or lunar fossule, or impression, the points of which were towards the tail; where the body was depressed the sides were raised into a sharp angle, or elevated line, of about eight inches in length, running into the middle of the tail, or a little beyond; the nose was prominent and sharp, and on either side from the nose to the eyes were numerous perforations, or minute pores; the tail was of a lunar form, the upper lobe nearly a third longer than the lower. From the number of teeth, which were two rows in each jaw, the fishermen concluded it to be two years old, and Mr. Goodenough was assured that they had been seen of the length of eight feet, with three rows of teeth."

The capture of one of these voracious animals frequently beguiles a tedious hour during a long voyage. Its struggles, when brought on deck, are very great, but a few severe blows on the nose soon disables it from further exertion. When seizing any object, the animal turns on the side, not (as is generally supposed) on the back. The shark, judging by an European palate, is not good eating; the fins and tail are very glutinous, and are the portions most relished by the seamen, and dried they form an article of commerce to China, where they are used in soups, and considered as an excellent aphrodisiac. I have seen several sharks and bonetos about the ship at the same time, but I never observed the former attempt to molest the latter. The shark is eaten eagerly by the natives of the Polynesian islands, and I have often seen them feasting on it in a raw state, when they gorge themselves to such an excess as to occasion vomiting. It is not an unfrequent source of illness among these islanders, and they suffer so much in consequence as to lead them to suppose that their dissolution is nigh, but they cannot be persuaded that the eating of raw fish is the cause. An emetic soon removes the symptoms by removing the cause, and the sufferer considers the cure as almost miraculous.

Attending the shark is seen that beautiful little fish the *gasterosteus ductor*, or *pilot-fish*; who first approaching the bait, returns as if to give notice, when, immediately after, the shark approaches and seizes it [*]. It is a curious circumstance that this elegant little fish is seen in attendance only upon the shark. After the shark is hooked, the pilot-fish still swims about, and for some time after he has been hauled on deck; they then swim very near the surface of the water, and at that time I have seen them taken by a basket from the chains of the ship. When the shark has been hooked and afterwards escapes, he generally returns, and renews the attack with increased ferocity, irritated perhaps by the wound he has received.

On the 28th of October, 1830, in the straits of Singapore, about three miles distant from the Malayan coast, I saw, for the first time, the Squalus zygæna of Linnæus, or hammer-headed shark, (Zygæna malleus, Valenciennes): it was of small size, and passed so close by the ship, swimming on the surface of the water, as to enable me to have an excellent view of its peculiarly formed head. It did not remain long about the ship.

On the 18th of March, 1831, in latitude 44° 56′ north, and longitude 25° 10′ west, in the evening, two sharks of a very large size were seen at a short distance from the ship. A high dorsal fin, projecting from the water, was at first only discernible, and had a resemblance to a rock [†]. It was at first stationary, but soon began to move steadily along, and then occasionally the tail could be seen partially above the water. I know not what species to refer it to; one of the crew on board, who had been in a whaler, said that it was what they named a "*bone shark*," which is seen in numbers alongside the ships when they are cutting up a whale. He said also, that he had seen them as large as a "*twenty-barrel whale*;" that "the mouth resembled the gill of a fish, and they are spotted over the back." Whether the latter part of this account accorded with the actual appearance of the fish, I was not sufficiently near to ascertain, but it appeared correct with respect to their large size.

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[*] The shark is more wary of taking the bait when unaccompanied by the pilot-fish; it will then approach, and retire several times before he ventures to seize it; but when the little pilot is in company it will first approach the bait, (the shark waiting at some distance) and return, as if to report; when the shark advances and seizes the bait without hesitation: this I have remarked in numerous instances.

[†] Being at first stationary, and of a dark colour, a ship passing it rapidly might have considered it as one, and reported accordingly, and such a circumstance has no doubt caused many rocks to be laid down in the charts which have actually no existence.

The natives of the Polynesian islands have such a dread of sharks as to worship some of them as gods; not from any respect or love towards them, but from fear. Ellis states, that, "although they would not only kill, but eat certain kinds of shark, the large blue sharks (squalus glaucus) were deified by them; and, rather than attempt to destroy them, they would endeavour to propitiate their favour by prayers and offerings. Temples were erected, in which priests officiated, and offerings were presented to the deified monsters, while fishermen and others who were much at sea sought their favour. Many ludicrous legends were formerly in circulation among the people, relative to the regard paid by the sharks at sea to priests of their temples, whom they were always said to recognize, and never to injure. The principal motives, however, by which the people appear to have been influenced in their homage to these creatures, was the same that operated on their minds in reference to other acts of idolatry: it was the principle of fear, and a desire to avoid destruction in the event of being exposed to their anger at sea[*]."

An accident occurred during my stay at Cavité (Island of Luçonia). On August 10, 1830, a native lad, whilst bathing, had his thigh so lacerated by a shark as to occasion a speedy death from hæmorrhage. This was not, the inhabitants informed me, an uncommon occurrence at this place, when persons incautiously bathed in deep water.

In olden times, sharks were considered to be allied to the Leviathans of the deep, and afforded then, as at the present day, amusement to passengers traversing the ocean. The following account of the capture of one of these voracious animals, from Dr. Fryer's "New Account of India and Persia," published in 1698, is amusing:—

"Two of the *lesser offspring of the great Leviathan* (the weather being calm, these sort of them else not visible, being of no swift motion) came sailing after us; our men, as eager of them as they of their prey, hastened their engines for to take them; which no sooner in the water but each of them, guided by some half a dozen delicately coloured little fishes, which, for their own safeguard, perform the office of pilots (they never offering to satisfy their hunger on them), who lead them to the baits; when they, turning their bellies up, seize upon them on their backs, hook themselves in the toils, beating the sea into a breach, and not without a great many hands are drawn over the sides of the ship; which seen by the poor silly little fishes (as conscious of their error), they swim to and again, and hardly forsake the ship; but being within board, the ship's company, armed with hatchets, presently divide the spoil. They are not scaly, and therefore imagined to be a kind of whale, being finned like them, with a great fin on their backs near their tails (which dried, is used instead of a slate) of a darkish grey colour on their backs, lighter on their sides, and white under their bellies; their snout on the same plain with their mouths, but their mouth within that a great way; the cause why they turn their bellies when they take their prey. The mouth of one of them extended, is two spans wide, armed within with three tire of sharp-pointed teeth on both jaws, so piercing that needles exceed them not, and of such strength that a leg or an arm, bone and all, is but an easy morsel; wherefore called *sharks* by the seamen, on whom they are bold enough to fasten and dismember, if not shunned, when they wash themselves. They are of a rank smell, and not good to eat but by stout stomachs; of length they are ten, sometimes fourteen feet." Page 5.

I shall now conclude with a few observations on muscular irritability, as exemplified by the shark. That which is termed muscular irritability, and which is met with to a great degree in all cold-blooded animals, is well exemplified in the shark, which perhaps possesses it to a greater degree than other kinds of fish. I have seen a shark transfixed with an harpoon after it had been hooked, so as to cause the viscera to protrude; it was hoisted on deck, when, after a quarter of an hour had elapsed, the lower part was separated from the upper (which detached lower portion for a long time displayed great powers of vitality); the head and upper portion were afterwards thrown into the water, when the pectoral fins were moved as in the action of swimming. How long this irritability continued I cannot say (but from other instances that I had seen, I should consider for a long period), as it soon went astern of the ship. I have frequently seen the animal hauled on deck, the whole of the viscera extracted, and the body otherwise mangled when thrown overboard, swim for some distance in this mutilated state. Again, a shark has been hung up with the abdomen ripped open, the whole of the viscera extracted, and the head detached; yet symptoms of vitality, or rather muscular irritability, remained for three hours from the time of its removal from the water; and this frequently occasions the spectators to consider that the animal is in a state of suffering. It is only in the cold-blooded animals that we meet with it to such an extent; in the warm-blooded animals it occurs, but in a very slight degree.

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[*] In one of the fabulous legends of the natives of the Island of Tahiti, their island is represented "as having been a shark, originally from Raitea. Matarafau, in the east, was the head; and a place near Faaa, on the west, was the tail; the large lake Vaihiria was the ventricles or gills; while the lofty Orehena, the highest mountain in the island, probably six or seven thousand feet above the sea, was regarded as its dorsal fin; and its ventral fin was Matavai."—*Ellis's Polynesian Researches*, vol. i. page 167.

Blumenbach, in his Manual of Natural History, thus mentions the reproductive power and independent vitality with reference to the amphibia:—"The extraordinary strength of the reproductive power in several amphibia, and the astonishing facility with which the process is carried on, depend, if I mistake not, on the great magnitude of their nerves and the diminutive proportion of their brain. The former parts are, in consequence, less dependent on the latter; hence the whole machine has less powers of motion, and displays less sympathy; the mode of existence is more simple, and approaches more nearly to that of the vegetable world than in the warm-blooded classes; but, on the contrary, the parts possess a greater individual independent vitality. Since, in consequence of this latter endowment, stimuli which operate on one part, or one system, do not immediately affect the whole frame by sympathy, as in warm-blooded animals, we are enabled to explain the peculiar tenacity of life which is displayed under various circumstances in this class—viz. frogs still continue to jump about after their heart has been torn out, and turtles have lived for months after the removal of the whole brain from the cranium. The long-continued power of motion in parts which have been cut off from the body, as in the tail of the water-newt and blind-worm, may be explained upon the same principles."—Sect. 98.

The length of time that this irritability exists in snakes, has given rise to the opinion of the vulgar, that "*if a snake is killed in the morning, it will not die before sun-set.*" Among numerous instances of irritability in the warm-blooded class, shortly after death the heart may be stimulated to perform its natural action, by being punctured; and in a limb after amputation, the muscles are excited to contract by a scalpel being plunged into them.

London, August 31, 1831.

MEDICAL GAZETTE.

Saturday, September 17, 1831.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

THE LATE SCHEME OF A COLLEGE OF MEDICINE.

WE have no doubt that some few individuals there may be, bold enough to say, and perhaps silly enough to believe, that the Collegium Wakleyanum still lingers out its miserable existence. There never yet, we believe, was a plot discovered, or a favourite bubble scheme exploded, some of the adherents of which did not cling to its vanishing relics with the attachment of charmed devotees, and even persuade themselves that all hope was not quite extinct. When some great impostor, who has had disciples, obeys the common lot of humanity, nothing is more frequent than for many of those followers to delude themselves into the belief that all is not lost—that the great heresiarch is still forthcoming—and that, like the sun in an eclipse, the reappearance of the superior influence will be attended with additional splendour. The poor misguided followers of Johanna Southcote could not be convinced that their prophetess was no more until, by the testimony of more senses than one, they were obliged to concede that mooted point. And so we have no doubt it is with some of the disciples—nay, with some of the supporters, of the Collegium Wakleyanum scheme—that darling project, which had so many of the most notorious spirits of the metropolis in the list of its supporters—

> Ambubaiarum collegia, pharmacopolæ,
> Mendici, mimæ, balatrones, hoc genus omne,

all, all now in tribulation and discomfiture at the calamity which has occurred—or which, as some would fain believe, is still impending, and might possibly, by some lucky conjecture, be averted. We cannot, nor will we, pretend to offer either sympathy or hope to these unhappy individuals: in us, it were more an act consistent with our duty to offer them wholesome advice, and awake them to a sense of their situation—to expostulate with them generally on the silly exposure of themselves which they have made; and when we suspect some of the said individuals to be more than ordinarily infatuated and obstinate, to reduce them to reason by a suitable severity. We will not mince the matter with them, and we are sure they will like us the better for our sincerity. How then stands the case? A great *fanfaronnade* is sounded: a college is said to be formed: and we are anxious to know its component parts. We take up the list which is thrust into our hands, and what names do we find set down as constituting the *senatus academicus?* A band of persons chiefly effete and broken-down teachers—all of them men of disappointed hopes—dissatisfied with their present condition—and leagued together under the leadership of a man of desperate fortunes. They meet together in private to concoct schemes of they know not what—they have every thing to expect from a change—they have nothing to lose—and their primary object is to secure as many associates as they can. They propose subscriptions, eleemosynary funds, fellowships, doctorships, hospitals, and professors' chairs. Promises are cheap—they promise every thing: they talk much and big about all manner of supposed attractions, purity of principle, (!) reform of abuses, a new constitution, and all that sort of thing. They call public meetings, contrive on the first occasion to gather a handful of people together, and to wheedle a certain member of parliament into the chair; make speeches for the mystification of themselves and the public, and try every expedient to attract the notice of the press. But all will not do: the public are not to be so readily gulled. Not all the desperate efforts and exertions of the party, their Crown-and-Anchor buffooneries, their heaps of annonymous and mock epistles, their announcements of fictitious correspondents, and their trumped-up replies—not all these combined could save the abortive bantling from premature starvation. It is gone—sunk—no more—and peace be to its unhappy manes!

We shall now in some sort (by way of consolation) examine more particularly into the causes which led to this sad event. These, we imagine, were principally two—desperate mismanagement, and the parts being committed to the most inappropriate performers. Of the scenes which were enacted at the Crown and Anchor we have had occasion formerly to speak; they were of the serio-comic kind, though with somewhat more drollery intermixed than was seemly. The clowns acted more than was set down for them, and the thing became "caviare to the general"—it pleased not the million. The second representation drew no house, and a third was not even attempted; so that the theatrical speculation failed completely. Other expedients were now devised. The hebdomadal organ of the company was set to work, and anonymous letters, and rumours of letters, of the most ridiculous description, were attempted to be played off periodically. We have a few of these precious documents lying before us, and shall present some part of them to our readers as a treat. The writers are supposed to make certain inquiries at the president, or founder (or whatever be the title he rejoice in) of the college; and he, as Sir Oracle, replies most sapiently. One would suppose, however, that some of the questions were *really* sent by some wag who wished to have a little fun with the founder. The first comes (probably it does) from the emerald isle:—

> "Are fellows and members of the College of Physicians, and of the College of Surgeons, in Ireland, eligible [to fellowships in the Collegium Wakleyanum]?" Hear the important reply:—"Undoubtedly"!

Now really this is too cruel of the letter-manufacturer, to make his first joke turn upon the query, as it were, of a fellow of the King and Queen's College of Physicians in Ireland! If he goes on in this way, he will surely be the death of us. The querist wished to become an *honorary fellow*, we presume, of the Collegium Wakleyanum. Capital!

Let us have another:—

> "Will the production of diplomas be adequate proof of the eligibility of a candidate for admission to a fellowship [in the Col. Wakl.]?"
>
> "Certainly not; it will invariably be necessary that documentary evidence should be supported by the *collateral testimony of respectable personal reference*. How otherwise can the candidate be satisfactorily identified with his testimonials?"

The answer is perfectly proper. What is more probable than that the candidates for the Wakleyan Fellowships would come forward with diplomas not their own—begged, borrowed, or stolen—*quocunque modo*, to attain the summit of their ambition—to get a Wakleyan fellowship? There ought to be no reasonable objection, then, one should think, on the part of any *gentlemanly* candidate, to bring along with the diplomas "the collateral testimony of respectable personal reference." It would certainly have been a good check on the aspirants to Wakleyan honours, to be obliged to produce *a respectable reference*; though we rather suspect that this regulation, had the thing gone on, would have been to be applied with much latitude of indulgence.

But there was a harder test—and a dreadful penalty besides—attached to the fraudulent attainment of the said honours in other circumstances.

> "What proofs will be required to establish the fact of having been in practice before August 1815? Will the production of account books suffice [to satisfy the mind of the adjudicator of the fellowship]?"
>
> The reply is most serious. "*Two* respectable references to professional gentlemen will be necessary, or *one* reference *with the production of the books*. It should, however, be remembered that *diplomas obtained by means of misrepresentation* [of which sort a large number may be calculated upon] *will be forfeited* on discovery of the fraud, and the families of such persons will not be allowed to derive any benefit from the ELEEMOSYNARY FUND."

That, we must say, was, in our opinion, a very wholesome law: the families of the *convicted* rogues obtaining fellowships in the Collegium Wakleyanum, should have no claim upon the eleemosynary fund; enough for the rogues if they be lucky enough to escape detection.

"Are chemists and druggists, who do not practise either medicine or surgery, but who, nevertheless, are members of the College of surgeons, eligible candidates [for fellowships in the Collegium Wakleyanum Medico-Chirurgico-Apothecarium]?" The answer to this query is rather verbose. The President is of opinion, if we understand him aright, that the title of "Doctor" ought to be bestowed with some little discretion; at all events, that mere druggists should not rashly aspire to that distinguished honour. But with what latitude this restriction was to be understood, might be gathered from another question in another letter. We quote from memory:—

"Is a young man who has passed Apothecaries' Hall, after serving his apprenticeship, admissible to a fellowship in the Collegium Wakleyanum?" Certainly; upon complying with the other conditions, of producing his testimonials and giving a respectable reference.

The reply to another query cannot be too extensively circulated:—

"How much is the money?"

"Three guineas: and the sooner it is forwarded to the Committee, the better. The subscription to the eleemosynary fund is not yet fixed, but that need not prevent any gentleman from contributing at once to the support of *this charitable institution.*"

One more question and answer, and we must close our specimens:—

"Is it likely that the sanction of parliament will be obtained for the Collegium Wakleyanum?"

"*A doubt cannot be entertained on the subject.*"

How truly like an oracle! The reader will not fail to observe the beautiful ambiguity which resides in this brief, simple-looking answer. The oracular response which Pyrrhus received, touching his warfare with the Romans, was not more encouraging than this, nor more remarkable surely for its result.

We now turn to make a remark or two on the personages—the *dramatis personæ*—engaged in this notable scheme. The prime mover and acting manager is so well known to the public—we have described him so often—that to dwell more particularly on his merits on the present occasion must be totally unnecessary.

We may more usefully direct our attention to some of the other performers, who are perhaps less known to fame. By the way though, we should take some credit to ourselves for the sagacity with which we marked out all the appointments about the Collegium Wakleyanum, at a time when the only clue we had to the formation of such a programme was the problem:—Given such and such objects to be attained, state who are likely to be the agents. We solved it correctly; even down to the Old Bailey reporter, whom we assigned to the post of secretary, it turns out that we made no essential mistake. There were, indeed, one or two additions to the list, such as we gave it; but these were not contemplated in the original project, and their accession to the scheme was rather the result of circumstances, than of any reasonable calculation on the subject. And how have the names of these senators extraordinary affected the list of the Wakleyan agents? For any good purpose not in the slightest degree. One of them, an ancient ex-teacher of anatomy (and all his life no more than a teacher of that single bare branch of medicine), should have been prevented by his friends and guardians from disgracing himself thus in his latter days. The aged courser should have been spared this ignominy;

and, as the poet recommends, be suffered to finish his career in retirement and seclusion,

> ——— "ne
> Peccet ad extremum ridendus et ilia ducat."

But a certain other individual of this class seems to have joined the Wakleyan establishment out of pure wantonness, (it can be accounted for in no other way;) one who, without adding any thing to the respectability of the body with which he became connected, has materially contributed to set himself forth in his due dimensions before the profession and the public. This man had a College of his own to mind—a College, too, which wants minding, but which *he* seems, with an instinctive sagacity, to have taken measures to eschew. Is it possible that that establishment, amid its other incongruities and incomprehensible proceedings, will continue to harbour in one of its chairs a man who has sunk himself, and by implication the University, to so considerable an extent? We warn, and we call upon the heads of that establishment from which he derives whatever reputation he enjoys, to insist upon his return to the allegiance from which he has so ungratefully swerved.

A few of the other survivors and *confrères*, whom we had destined for a little serious conversation, must wait for a more convenient opportunity. For the present we shall content ourselves with what we have said.

HOTEL DIEU, PARIS.

This great hospital, the centre of so many associations connected with our profession — the school in which so many illustrious men have studied, and become the teachers of successive generations—will soon cease to exist. We understand that the state of the building, which is very old, renders its demolition necessary, while the confined nature of the situation in which it is placed will prevent its reconstruction on the same site. The patients are to be moved to the *Hôtel des Invalides*.

COLLEGIUM WAKLEYANUM.

The inquest to which we alluded in our last turned out as we suspected it would. The jury, upon coming to take a view of the body—to proceed, as the law directs, *super visum corporis*—were astonished to find that they could see nothing: not a vestige of the body was to be seen—it was evaporated, vanished no man knew whither. The Coroner of course did not detain the jury, but discharged them after a suitable address, in the course of which he mentioned that in all his experience he had never met with any thing so awful, nor even approaching it, with the exception of a dreadful case of spontaneous combustion on which it was his melancholy duty to sit some years ago. A solemn feeling of seriousness was impressed upon every brow as the court broke up.

THE EMIGRATION OF THE SWALLOW.

The swallow is found an inhabitant of the tropical regions of the globe, visits the northern climates during the warm months of the year, and is regarded as the harbinger of summer; emigrating, however, with their young, to a more congenial climate on the approach of winter. It is then that they are met with at sea, perching, in an exhausted condition, on the rigging and decks of ships. Being interrupted by adverse winds, they waver in their course, spent with famine and fatigue, until the ship affords them a temporary resting-place; from which, refreshed by a few hours' rest, they renew their flight. They are said to arrive in Africa about the commencement of October, having performed their fatiguing journey in the space of seven days. During a passage from England to New South Wales on the 27th of September, 1828, several of these birds alighted in an exhausted condition on the rigging and deck of the ship, and were readily captured: we were at that time in latitude 13° 40′ north, and longitude 23° 20′ west; they were the hirundo rustica of Linnæus.

During a voyage from India to England on the 2d of December, 1822, I captured one of these little feathery emigrants in latitude 2° 18′ north, longitude 23° 11′ west.

I am not aware whether they emigrate from one tropical country to another, but during a passage from Manilla to Singapore, through the China sea, on the 20th of October, 1830, for several days a number of these birds were flying about the ship; and I am inclined to suppose derived at that period sustenance from the flies which infested the ship, as soon afterwards but few of those insects remained. In the evening they would roost on the rigging, and some even took refuge for the night in the chief officer's cabin, which opened upon the deck, and were again set at liberty in the morning. They became after a few days exhausted; several were found dead; and others became so tame from fatigue and hunger as to be easily caught, and would afterwards remain perched on the hand, without making any effort to regain their liberty. These were also of the species hirundo rustica of Linnæus, which is widely distributed over the globe. We first observed them in latitude 15° 29′ north, and longitude 117° 40′ east; and we lost them, most having perished, in latitude 13° 30′ north, longitude 110° 45′ east, having been about the ship for thirteen days.

The question that arises is, whether in the latter instances they were emigrating, or had been blown off the land by the westerly gales we had experienced since leaving Manilla Bay. It may be mentioned, that as we had at the same time numerous other land birds about the ship, the latter supposition is not improbable.—*From Mr. G. Bennett's MS. Journal.*

NAGA-PUSING OF MALACCA, &c.

At Singapore I observed some filaments from a plant which was described as growing abundantly at Malacca. These filaments display much irritability on the application of warmth, and will twist and turn about for a long period afterwards. They are named *Naga-pusing* by the Malays, which signifies "*to turn or twist round.*" Each filament is covered with a very minute pubescence. They appear to be the awns probably of a species of anthistiria. They are used by the natives, in form of decoction, as an external remedy in eruptive diseases.—*From Mr. G. Bennett's MS. Journal, November 7th,* 1830.

ON FAILURES IN LITHOTOMY.

[Concluded from page 704.]

The four following cases are not failures, though approaching that way; and they are, perhaps, sufficiently interesting in some other points to be recorded. The two first will further show how prone the mucous lining of the bladder is to inflame from rough sounding, though accustomed, with impunity, to bear the irritation of the stone. They also furnish examples of a sacculated stone left in the bladder, of hæmorrhage during and after operation, and of part of the bladder contracted round the stone.

Case IX.—*Peritonitis, excited by long-continued attempts to find the Stone with the Staff before Operation. Two Stones; one sacculated,—a piece left in that situation safely.*

Garrot Ward, Infirmary.—I performed lithotomy, February 19, 1817, upon — Holliday, of Nailsworth, a man seventy-three years of age, but of good constitution. There was considerable difficulty in feeling the stone with the staff, and attempts were indiscreetly made by many surgeons who were present, for a considerable time,—so that a fear was expressed that injury might be done to the bladder.

There was nothing remarkable observed in the operation, excepting that besides a large stone, the bladder also contained a smaller one encysted. I felt it projecting immediately above the pubes, where it was evidently firmly fixed. A long pair of dressing forceps broke off the point in the attempt to extract it from its bed, and the remainder was left behind, which the nail of the fore-finger could plainly distinguish to be on a level with the sides of the cyst and the bladder.

No further attempts were made to dislodge this stone. The severity of the sounding, (which led to this decision,) was not forgotten. It was not desirable to add more irritation to what had been already endured, and the stone was permitted to remain undisturbed in its habitation.

In the night, after the operation, he had severe pain just above the pubes, where was likewise some swelling; his skin was rather hot, the tongue somewhat furred. Pulse hard, and about 100. Thirty leeches were ordered to this part of the abdomen, and, when they ceased to bleed, cold rags were to be constantly applied.

In the evening, the pain was much diminished, and the swelling gone. No stools. Sulphate of magnesia every other hour, until the bowels should act. On the third morning, it was reported that he had vomited twice in the night: the belly was swollen more than before, very tender to the touch, and painful. Tongue furred, and pulse 111. He had taken two ounces of the sulphate, and had only one stool. Eighteen ounces of blood were taken from the arm, twelve leeches were applied to the abdomen. A purgative clyster was given; and when the leeches had ceased to bleed, a blister was directed, to cover the whole of the lower portion of the abdomen. He was placed in the warm bath after the leeches dropped off; the bleeding was considerable.

In the evening, he had had ten stools; his belly was much less swollen, he had been vomiting twice, the pain and tenderness much diminished. His pulse, 78, was reduced to 45. Tongue still furred, brown, but not dry. A clyster, with a little infusion of senna and sixty drops of laudanum, was ordered. On the following morning (the sixth from the operation) the swelling and pain of the belly were nearly gone; he had been sick once, had had five stools, pulse 73, slept four hours.

On the fifth day, in the evening, his pulse had risen to 88, and struck sharp and forcibly against the finger; his tongue was dry, and he was more restless; he had had one stool. Seventeen ounces of blood were taken from his arm, twelve leeches were applied to the epigastric region, where, he confessed, upon a close cross examination, that he felt some pain and tenderness. I say cross examined, because it was clear he wished not to be bled any more. He was desired to take sulphate of magnesia every hour until he was purged.

On the sixth morning his pulse was softer, though nearly as frequent; his pain, he declared, was gone; he had had twelve stools; his belly was no where tender; tongue more moist.

For the first time, this old man's countenance began to wear rather a haggard look, but his eye was still bright, he spoke firmly, and with confidence that he should recover. He was right. Strength was regained slowly, and he was ultimately discharged cured.

The foregoing case may be interesting, from the extent of the means successfully employed to remove the peritonitis in so aged a person, and from a sacculated stone being left in the bladder with safety.

In four days he lost thirty-five ounces of blood, had fifty-four leeches, and was freely purged;—moreover, he endured great pain. But here was a calm, courageous, and somewhat indifferent state of feeling, and if danger was apprehended, little was cared about the result. The patient had not the irritable quickness, the varying and wearing hopes and fears of the sensitive mind, which sees danger in every change, and suffers death many times before its real approach. Had he possessed the latter character, his chance of recovery would have been infinitely less; because the nervous power, which governs the frame, would have been exhausted by its own activity, and these vital actions which depend upon it would, in their turn, have ceased to be continued.

It is well, on this account, for the young practitioner to measure the moral, as well as the physical character of his patients, before he undertakes very decisive means. For as they are both mixed up together, to form what is called the constitution, this last can never be philosophically known without its component parts being somewhat made out; nor would the practitioner know how much it would bear, were he in ignorance of its true character.

I should not have dared to have taken such bold liberties with this old man's constitution, after it had endured a severe operation, had he possessed this quick, high temperament, sensibility, nervous irritability, or whatever it may be denominated, and which,—however it may be compounded of matter or of mind, and in whatever proportions,—feels every thing, yields to every thing, leads the body into all possible mischief, and, which is rarely remedied, though frequently increased, by bleeding or other lowering means.

I have chosen to consider the peritonitis as the effect of the long-continued and severe sounding previous to the operation, rather than of the latter itself, because there were no circumstances in it which indicated much violence or suffering.

Holliday was well when I last heard of him, and of course the sacculated stone had been in no way injurious. It is probable that a calculus, thus closely confined in a sac of the bladder, does little harm there; and when above the pelvis, it is not even likely to congregate more sabulous matter about it. If all this be true—if its close imprisonment admits not of that dreadful rolling of a loose stone in the bottom of the bladder, which irritates, inflames, and thickens the organ, and wears out the patient with pain,—why is it thought necessary to ferret out the sacculated one with so much anxiety and positive mischievous violence?

CASE X.—*Cystitis from Sounding—Hemorrhage during the Operation—Bladder full of Stone. From the case-book of Mr. R. Fletcher, junior.*

"Charles Pride, aged 50, was admitted into the Gloucester Infirmary, with the usual symptoms of stone in the bladder; and, upon being sounded, a stone was discovered. His bladder was so exceedingly irritable, that each time after sounding, which was twice done, he complained of great pain in the lower part of his belly, with tenderness of it, and vomited repeatedly, and his urine deposited an unusual quantity of mucus: so far did this go, that both times he was obliged to be freely leeched, put in the warm bath, and purged. On account of this great inflammatory irritability of bladder, Mr. F. did not think it right to encourage him with much hope from an operation; but, at the patient's own request, he performed it. After cutting into the bladder, Mr. F. discovered with his finger, that there were two very large stones in it. There appeared to be no room for the forceps, and, from the size of the stones, it was deemed necessary to enlarge the opening in the bladder more than is usually done. Luckily, both the stones broke under the forceps, and were extracted by means of that instrument and the scoop, in somewhat less than half an hour. Their weight was about six ounces.

One other circumstance may be noted in this operation; the transverse perineal artery bled so profusely, that, from the size of the stream, it might have been mistaken for the pudic itself. It must have bled nearly a quart during the operation. It was tied, but, being in the track of the operation, the ligature soon slipped off, and the operation was continued without regarding it. From this cause, and the length of the operation, the man became low and cold, and required some brandy. During that evening he was comfortable. Early next morning he began to vomit high-coloured green matter, but was in no pain, except what was occasioned by straining to vomit, in the lower part of his belly and wound. His bowels not being open, he was ordered a powder of Pulv. Rhei, gr. viij. and Pulv. Zingib. gr. v. with an effervescing draught. On seeing him in the afternoon, it appeared that he had thrown up the powder, and could keep nothing on his stomach; another was ordered, but that he also rejected. An enema of Magnes. Sulph. and gruel was then administered, but it returned without any fæces; several others were given him, but with the same want of effect. Pulse this day about 120, and feeble; tongue furred and white.

In the evening he complained of slight pain in the bottom of his belly, with slight tenderness. Twenty leeches to be applied to the part. He was very much depressed, and said he felt convinced he should not live till morning. In the morning it was discovered that the nurse had not applied the leeches, in consequence of the patient expressing a dislike to them. He had vomited greatly during the night. The leeches were applied directly, but did not draw much blood. A croton-oil pill was then given him, his bowels not having acted; this also he threw up. Pulse uncommonly quick, and very feeble; tongue brown. In the afternoon his pulse was imperceptible: Mr. F. ordered brandy to be given in small and repeated doses. In the evening his pulse gradually rose, and in the course of the night he had stools. He recovered.

Mr. F. considers the brandy to have saved him, by maintaining the powers of life until the bowels acted."

But for the brandy, given freely, he must have gone; so low was he brought in the first instance by the profuse hæmorrhage, and the long-continued teazing operation of extracting such a large mass of broken stones, and afterwards by the constant vomiting which succeeded the operation. But the hæmorrhage might have saved him from the dangers of inflamed bladder and peritoneum, for the former was remarkably prone to inflame from the mere irritation of gentle sounding.

CASE XI.—*Second Operation for Stone on the foregoing Patient. A portion of the Bladder contracted around the Stone above the Pubes.*

Samuel Pride returned to the hospital, after his former discharge and recovery from the foregoing operation, with more stone formed in his bladder. He was repeatedly sounded, and a calculus demonstrated to be there; but no cystitis followed this preliminary operation, as was the case before the first operation of lithotomy, nor was the stomach disturbed by it, which was remarkably the case on the former occasion.

The operation was now performed a second time, and no difficulties experienced in the early stage of it. The muscles and prostate were freely divided, and the stone was just touched with the tip of the fore-finger, deep, but very high, and above the pubes. The discovery of its situation was not made very readily, and then the forceps could not dislodge it, for it was evidently grasped or enclosed by some fold of the bladder. A little suspension of all earnest efforts was allowed, with a hope that the contraction might cease, employing the time in very gentle efforts of the forceps, to catch the stone between their blades, but in vain,—they constantly slipped over a small portion of its surface. The contraction around the stone did not relax,—the stone retained its situation.

Determined to use no violence, and to cease all farther annoyance of the bladder with the forceps, they were thrown aside. With the tip of the fore-finger of my left hand as a guide, the handle of the scoop was conducted to the lodging of the stone. By greatly depressing the handle, the extreme point of it was inserted over the calculus, and with a few gentle efforts—exactly after the sweeping or curved manner of the vectis working under the pubes upon the occiput of the child, in delivery—the stone was turned out of its enclosed or sacculated position, and fell to the bottom of the bladder, whence it was easily removed on a second introduction of the forceps.

The bladder was well cleared by the syringe in both operations. Discharged cured.

Where, as in the foregoing case, a stone lodged above the pubes does not come readily with the forceps, straight or crooked, the attempts with them should be discontinued, and some other contrivance adopted to dislodge the calculus, with the least possible violence to the bladder. Any slightly curved instrument, with a rounded edge and point, and of sufficient length, which could be passed over the stone and beyond it, would probably have the power of throwing it down to the bottom of the bladder, when the forceps might be safely resumed.

At this time, sixteen months after the last operation, Pride has again symptoms of a stone.

CASE XII. — *Hæmorrhage during and after Operation.*

Infirmary, April 14. 1818. In operating upon a healthy young man, the stone broke under the first gripe of the forceps. Half an hour was occupied in extracting the pieces, which was done as gently as possible, sometimes using the forceps, and then the scoop. During the whole period a considerable hæmorrhage was going on, and when the removal of the stone was accomplished, I looked after the vessel. It lay deep under the pubes, and certainly was of considerable size, as it bled rather profusely. Though it was apparently too deep for the perineal artery, yet it could not be the pudic itself, for the blood gushed from a point directly under the arch of the pubes, and of course far from the ischium.

A large rectum bougie covered with oiled lint, held by an assistant, suppressed the bleeding. Seven hours after the operation, a rush of water forced out the instrument, which, it appears, was held improperly and slightly; the hæmorrhage returned, and he lost, in a few minutes, about half a pint of blood. The instrument was replaced. It was removed on the third day, and nothing particular occurred until Sunday morning early, (sixth day from the operation,) when a profuse hæmorrhage took place, by which the patient lost perhaps three pints of blood: it ran through the bed, and the sheets and bedding were soaked in it. The instrument was again replaced, and well supported in its position, but was removed ultimately on Monday, the following day, the man complaining of exquisite soreness in the part. No farther return of the hæmorrhage followed, and he was discharged cured.

CASE XIII.—*Example of an Irritable bladder, ascertained by the effects of sounding upon it; increased by rather more violence in operating than should have been allowed;—the result,—abscesses in all possible directions. Case drawn by the Hospital pupil, C. P.*

" Charles Matthews, aged twelve years, admitted May 23d, 1822, has been subject for many years to pains across the lower part of his abdomen, at the end of the penis, before and immediately after making water, which will sometimes stop suddenly. There is a thick deposit of mucus in the bottom of the vessel into which he voids his urine, which is often tinged with blood. On the twenty-fifth he passed a small stone about the size of a pea, which he threw away without having shewn it to any person. (Mr. F. endeavoured to sound him on the day of his admission, but found some impediment.) The stone, he says, was of a brownish colour. For two days after this he continued tolerably free from pain, though not entirely; his water was also clearer, and he seldom experienced a sudden stopping of the stream whilst making it. The symptoms gradually returned; the water, too, deposited a thick sediment as before.

June 4th.—Mr. F. to-day passed a small sound, and distinctly (as did several of the bystanders) felt a stone. No violence was used. During the evening of this day he was in considerable pain, attended with some tenderness of the belly.

5th.—This morning the sediment was considerable at the bottom of the pot, and streaked with blood. The boy appears of a very irritable habit generally, like his bladder; it was with the utmost difficulty that the sound could be passed, on account of his violent struggles. Mr. F. expresses himself unwilling to undertake the operation, in consequence of the great irritability of the bladder, and the bloody discharge continued since the sounding.

7th.—He has continued much the same, suffering greatly at times. He thinks he makes water at least twenty times during the twenty-four hours.

July 3d.—The sounding has been again repeated, and the same severe symptoms have followed. The performance of the operation has been again deferred in consequence.

15th.—The patient having lost the additional symptoms from the sounding, the operation was this day performed, after a declaration from Mr. F. that it was not a promising case.

The transversalis perinæi was tied, in consequence of rather more than usual hæmorrhage. The opening into the bladder was made by the gorget, but upon grasping the stone with the forceps, it was found that the opening was scarcely large enough to admit of its passage, but some violence, though little, was exerted, when this difficulty was overcome, and the stone extracted. It was of considerable size, of a soft, white, gritty nature, similar to sand-stone, and possessed a volatile effluvia. Cap. Tinct. Opii, g. xxx. 12 A.M.—tolerably free from pain, except a slight smarting at the wound. Pulse rather full. 3 P.M.—patient asleep. 5 P.M.—said he was quite free from pain. The urine has passed freely through the wound. Pulse rather frequent.

16th, 10 A.M.—Slept at intervals of about half an hour during the night; awakened by the passage of the water through the wound.

17th.—Pulse increased in size and frequency; skin hot and dry; tongue slightly brown; mouth parched; slight tenderness upon pressure on the lower part of the abdomen; relieved by fomentations;—cap. Ol. Ricin. ½oz.—repeated during the evening, the first dose not having the desired effect. Free from pain all night; pulse slower; still thirsty.

18th.—Is in much pain; there is tenderness about the abdomen; pulse quick; tongue very white; skin hot and dry; has had three stools, produced by a dose of castor oil, during the day. Ordered a warm bath. In the afternoon the pulse was slower, and the tenderness and pain about the abdomen relieved.

19th.—Free from pain; full and quick pulse; tongue cleaner.

21st.—Clay-coloured stools; other symptoms as yesterday.

24th.—Rather better; appetite improved.

25th.—Passed some water through the natural canal for the first time, to-day.

August 2d.—He has rigors; his appetite is gone.

5th.—In a very weak state, and rather feverish; wound discharges very freely, and somewhat suddenly.

12th.—Wound discharges profusely; Mr. F. said an abscess had burst; loses flesh very fast.

19th.—Better; upon pressure of the belly matter flows out largely through the wound, and apparently through the anus.

July 20th, 1823.—Several collections of matter have taken place, and burst externally in several places in the region of the bladder, above the pubes, previously to September 31, 1822. He has had, through the winter, a slight, short cough; no expectoration accompanied it. His appetite and general health were for a time bad, but are now much improved, and the discharge from the wound, made by the operation, very small; though the urine has passed through the external wounds occasionally, above the pubes, where the abscesses burst, in three points, and where it causes a slight smarting pain.—Discharged cured."

The foregoing cases may assist in warning us generally against violence, and particularly from a rough treatment of the bladder, whether practised in the preliminary act of sounding for a stone, or in the operation of extracting it—that a gentle method is especially necessary in young children, as they bear not severe injuries with impunity—that an over-anxiety to possess a fragment of a stone, or an entire one sacculated, either temporarily or permanently, leads to irreparable mischief—that above the pubes a stone may reside with safety, and without inconvenience, in a sacculated or motionless state;—and, finally, to avoid the imminent dangers of a large stone, in the lateral operation, the surgeon should make it an invariable rule to ascertain its size before operation, that a judgment might be formed as to the necessity of a high one above the pubes, or the median section below their arch.

The danger from hæmorrhage can hardly be very great, since a fatal case was never known to happen in the whole course of my observation. One, however, whose experience has been far greater than mine, says that he has known patients lost repeatedly from that cause. Whenever it does happen that hæmorrhage from lithotomy destroys life—a fact to be granted upon the report of such undeniable authority—this unfortunate occurrence must arise from the knife or gorget acting too near the ramus ischii, an error which a surgeon of cool tact and experience would hardly allow himself to commit. A movement of the handle of the staff, towards the left groin, will carry the curve of the instrument, with the prostate, somewhat further from the ramus of the left ischium; and now if the gorget, (by far the safest instrument, for it cuts with precision,) is impelled forwards, with its edge inclined obliquely outwards, and downwards, the pudic artery itself is as safe as its neighbour on the opposite side the road.

Should the size of the calculus be not so very great as to forbid the operation below the arch of the pubes, (a circumstance itself of very rare occurrence,) and all the necessary circumstances and rules are observed by an experienced lithotomist—especially those concerning gentleness in execution—cutting for the stone, in cases proper for the operation, is probably as safe to the patient, as a common amputation of the lower extremity.

With regard to the modern practice of destroying the calculus, of boring, splitting, and hammering it to pieces in its soft and tender residence—time only can discover the real value of the measure. On a first view—it must be admitted that first views are frequently erroneous—and on reading some of the foregoing cases, the reader may probably conclude, that if it were possible to select another spot, he would not choose the human bladder as a desirable situation for the operation of stone-breaking. They shew that the mere irritation of a sound—and of the closed forceps, in searching for a stone in the bladder, is sufficient to excite fatal inflammation in that organ—and, in cases where no evidence existed of previous disease in the bladder or prostate gland, which would have predisposed those parts to fatal disorganization, under ordinary operating circumstances. Of course the work of death was effected by the irritation of the instruments employed in lithotomy. Will the lithontritic instruments, under similar circumstances, prove less irritating? Three cases out of seven, in one gentleman's practice, perished from inflammation of the bladder excited by them. Picked cases, paraded even before our best surgeons, do not furnish, either in kind or degree, the evidence necessary to decide upon the merits of lithontrity, and how far this innovation is likely to supplant or supersede the ancient and successful practice of lithotomy. Great and unbiassed experience of the most unquestionable kind—faithful and honest narration of facts, pleasant or unpleasant—of successes and *failures*, fairly intermingled alike—with a love of truth—can only decide the question. Evidence too, selected or not selected, should be taken with great caution, when proceeding from interested sources. Lithontrity is chiefly in the hands of those who either get their bread by it, or wish to do so; and it is too much to expect that nature should be the first to cry out against the gratification of her first wants. There is enough of evidence, however, to shew that the new operation bids fair to become a valuable auxiliary, if not a principal, in the treatment of calculi in the bladder; but the proper persons to mark the boundary of its merits, are those who get their bread from other sources. Surgeons generally, and especially hospital surgeons, whose operating habits, and greater experience in calculous affections, especially fit them for the important office of umpires, should no longer hesitate; for it is unquestionably both a matter of surprise and regret, that, up to this moment, not one case of lithontrity has been performed by a British surgeon, in a British hospital.

POISONING WITH PRUSSIC ACID.

Spring Assizes, Leicester, April 2, 1829.

(Before Lord Chief Justice Best.)

The King v. Freeman for the Murder of Judith Buswell.

[Concluded from page 766.]

Mrs. Elizabeth Biggs, examined by Mr. Clinton.—Is wife of last witness. On 11th February she retired to bed about eleven o'clock; saw the deceased after she went into her bedroom; the deceased brought the child to her; heard her go up stairs; heard her door shut; had not observed any thing remarkable in her manner that day; was called up the next morning at seven o'clock by her husband. He went to let the work people in. He called loudly to the deceased; he then called her, and she got up directly. She went directly to the maid's room; the door was latched, but not fastened. On going into the room she saw that the young woman was dead; she was lying on the bed, with her head resting on the bed-board. Did not notice the bed-clothes particularly; she was covered to her breast. Did not notice the position of her hands, nor the position of the candlestick. She called out, and Mr. Biggs came into the room; no other person came. Mr. Biggs and herself went into Freer's room; Freer got up and came in. Was so much agitated at the time that she cannot recollect whether Freeman came into it or not. She returned to her own room. Catherine Beer came in half an hour or an hour afterwards; had not returned into the girl's room before she came. Deceased went about her work the day before, and had made preparations for the following day's work. She locked the doors, and placed the keys in their proper places; she had been brewing on the Tuesday. It was her business to light the fires, and she had made preparations for the purpose. Had no suspicion before this time of the deceased being pregnant; had not observed any young man keeping her company.

Cross-examined.—Sometimes goes from home; don't know whether any young man visited her during her absence—cannot say. Freer occupied one room, and Freeman another; the two rooms were separated by a partition; there was a door between Freer's room and prisoner's. Freeman, to go to his bed-room, would have to go through Freer's room; prisoner, to get from his room into deceased's room, would have to open three doors. When she went up stairs the door of the girl's room was latched, but not bolted. The usual way of shutting the door of the girl's bed-room made considerable noise; heard her shut it that night, at the time witness was in her own bed-room with the bed-room door open. The room occupied by herself and her husband was under the room in which Freer slept. The child was in the room with her: it was not well during the night. Has no recollection that she slept at all. The door which communicates with the girl's room into the passage opens with great difficulty. A person who wanted to come out of the inner room through Freer's room to open the door, would have great difficulty in doing it; it would occasion a great deal of noise opening it in the usual way. Could have heard the maid's room shut if it had been shut in the usual way, but not the other, when in her bed-room. Has not said that if either Freer or Freeman's room had been opened she must have heard it; has said that if the maid's room-door had been opened and shut in the usual way she must have heard it. No alteration has since been made in the doors. Deceased was brewing on the Tuesday, and went into the cellar on the Wednesday night to fill her barrels, on the night of her death; she did it in the course of the evening. Don't know how long she was in the cellar—perhaps five or ten minutes. The fish-sauce and drugs were not kept in the same cellar as the beer; there is no connexion between that cellar and the cellar where the brewing vessels were. The brewing vessels were kept in a different part of the cellar from where the drugs were kept. On Tuesday the brewing vessels were removed into another cellar. A person could get from the kitchen into the place where the drugs were; there was no lock to the door into it. Deceased complained slightly on Wednesday of being unwell. Does not recollect saying before the Inquest, if either of the doors had been opened during the night she must have heard it; cannot take upon herself to say she never said so; if she said so, she was mistaken.

Re-examined.—Her husband never left the bed-room from the time he came in at night till the next morning.

Catherine Beer examined.—Went to the last witness's house on the 12th February, at a little before eight; was sent for to attend to the deceased. Went there as soon as she was liberated from Mr. Price's. Got to Mr. Biggs's a little before eight. After being there a short time, went up stairs with Mrs. Biggs into the deceased's room; saw the deceased; she was quite dead; she was lying on the bed lengthways, with her head a little sideways against the head of the bed-board. The bedclothes were quite straight, they came up to her breast; her hands were under the clothes; she had only a chemise and a cap on; her day clothes were on the bed, quite straight. She turned the bed-clothes on one side, and found a bottle; it was a small phial bottle. Mrs. Biggs standing at the door when she found it, she gave it to Mrs. Biggs; there was a white piece of paper round it when it was found; there was a cork in the bottle. Mr. Paget saw the body before she went. She removed the chamber-pot, which stood in her way; there was a quantity of hair in the chamber-pot, as if the deceased had been combing her head; there were two combs on the dressing-table; there was a small flat candlestick in the chair by the bed-side, the candle was out of the socket, and lying on the flat part of the candlestick. She laid the body out, and went on the Monday following to wash the linen and examine the bed more particularly; between the two top blankets she found six napkins and an apron. Is a married woman, and has had experience; supposes deceased put them there in case any thing happened to her in the night. Deceased might have put them there for her use, in case of a miscarriage, or any mishap. She moved the chamber-pot, and put it under the dressing-table; did not take it out of the room.

By the Judge.—The napkins were between the two top blankets; they would have been of use in case of excessive flooding.

Cross-examined.—Cannot positively say what time it was when she went; saw Mr. and Mrs. Biggs when she got there. She came to do the work of the house for Mrs. Biggs; saw prisoner in the kitchen; he did not stay many minutes after she got there. Did not see Mr. Paget; he had gone; he did not come again while she was there.

Mr. Thomas Paget examined.—Is a surgeon in Leicester. Was called to Mr. Biggs's house on 12th February, at half-past seven in the morning; went into the deceased's room, and found her lying on her back, with her head resting on the back of the bed-board. She was dead. Judging from the rigidity of the muscles, she seemed to have been dead four or five hours. Did not stay many minutes: did not remove the bed-clothes; did not turn them down; told them to let the things remain as they were, and inform the coroner immediately. Returned about four o'clock in the afternoon; the room was in the same state as in the morning; the deceased was lying in the same form, with the exception of her left hand being nearer to her stomach. Proceeded, together with Mr. Wilkinson, another surgeon, to examine the body; found the body in a state of freshness; attributed her death to her stomach containing a great quantity of prussic acid; the contents of her stomach were strongly impregnated with prussic acid. She was between six and seven months gone with child; the child was recently dead.

Cross examined. — From experiments which witness has seen tried on animals, thinks it is *possible* she might have had the power of corking the bottle.

Re-examined.—The operation of prussic-acid is always calculated by seconds. Cannot tell how long half an ounce would be in its operation upon the human species; it would operate sooner upon brutes; has seen a small dog rendered insensible in ten seconds by the same quantity the deceased is supposed to have taken; has also given a cat half the quantity, which he conceives fully adequate to her frame; she was thirty seconds before she fell insensible. Another cat, to which a quarter of the quantity was given, fell insensible within twenty seconds. Mentioned this as shewing the irregularity of prussic acid in its operation; the two doses were out of the same bottle; shewing its effect on different constitutions. The last-mentioned cat was a stronger and larger cat than the one which took thirty seconds to be rendered insensible.

By the Judge.—The animal appeared conscious of what it was about till the moment it was seized with convulsions. Cats and dogs walk about the room for several seconds after having taken it, till they are seized with convulsions. Thinks deceased might have had knowledge to cork the bottle after she had taken the poison; thinks it is *possible*. Cannot form an idea as to that probability. Smelling the bottle will produce sickness and insensibility; in some people it will produce a sense of sickness, and in others insensibility. Its force would not be weakened by being kept in a dark place a fortnight; thinks not. Druggists are obliged to leave it and other poisons accessible to their apprentices, for making up of prescriptions. Some of the experiments spoken of were made with the prussic acid that remained in the bottle.

Edward E. Wilkinson —Is surgeon to the Leicester Infirmary. Was with the last witness when the body of the deceased was opened; concurs with him in his opinion as to the cause of her death. Is not able to form any opinion of the quantity of prussic acid taken into the stomach. Saw the chamber-pot; a small piece of leather in it; it appeared as if it had been tied round the top of the bottle. There was likewise a small string, which seemed to have been used for the purpose of tying the leather round the neck of the bottle. Was present when experiments were performed on the Wednesday.

Cross examined.—The piece of leather was found in the pot; did not perceive there was any sickness in the pot; it was past four in the afternoon when he saw it; should think smelling prussic acid would produce faintness. Has seen experiments tried this morning. If deceased took four drachms, should think she *could not* have power to cork the bottle. Saw a dog take four drachms; it died in eight seconds. From what remained in the bottle produced, should think four drachms were taken by the deceased. Never heard prussic acid being taken to procure abortion; it might be possible.

Dr. Freer examined.—Was present at the time Dr. Macauley made experiments, the day before yesterday, with the acid found in the bottle. Has been in practice, as a physician, twenty years. Prussic acid is the most violent poison that is known; thinks its immediate effects would be in proportion to the strength of the dose administered; by being exposed to the light it might lose some of its effect; saw experiments made with fresh acid and the acid found in the bottle; it varied very little in its effect from the fresh. Sixty drops is a drachm. Thinks that after three or four drachms had been received into the stomach, a person would not be able to do the most tifling act—certainly not. Thinks it *impossible* for a person, after having taken such a quantity, to put a cork into a bottle.

Cross-examined.—His judgment is chiefly from analogy. Saw some experiments tried upon dogs the day before yesterday; never saw any experiments tried before. There were seven experiments; lays most stress upon the sixth experiment; four drachms and a half were given to a dog, and its effect was almost instantaneous. In his opinion, three seconds had not elapsed before the dog was perfectly insensible; it was a sort of terrier—a small one. Should imagine its operation upon the human species to be equally as speedy as upon brutes. Its operation might vary according to the constitution. So large a dose was sufficient to detroy any constitution. Never attended any person supposed to have taken it; have seen it administered in a small dose as a medicine.

Mr. Paget recalled.—Has seen the opinions of five medical men—Drs. Paris, Conquest, Addison, Morgan, and Babbington, jun.

Dr. Freer recalled by the Judge.—A dog which had taken twenty drops, in five or six minutes recovered. The last experiment which was performed was giving this identical dog forty drops more; in twenty seconds he fell howling; in thirty-five seconds he became insensible, but still breathing; in one minute he was gasping; in a minute and a half worse; in two minutes he appeared at his last gasp; in three minutes and fifty seconds he again gasped; by five minutes he was dead. Experiment No. 6, four drachms and a half were given; in four or five seconds the dog was insensible and dead. No. 3 was a large dog, and double the size of the other; four drachms were given to him; half was lost in forcing the animal to swallow. He was a large setter dog. In thirty seconds he staggered; in forty seconds he fell; in one minute and a half dead. The immediate effect of the poison would depend upon the dose taken. If food was in the stomach it would certainly make a difference; if it was a small dose, it would retard the progress of the poison; if it was a large dose, it would make little or no opposition.

Mr. John Needham examined.—Is a practising surgeon in this town; was present at the experiments to which Dr. Freer has spoken, and took the minutes; concurs mainly with him in what he has said; each animal was in a greater or less degree affected in a few seconds; one fell in half a minute. The acid taken by the deceased produced insensibility equally at the same time as the fresh, but it did not occasion death so immediately. Deaths happened at forty seconds with the fresh, and at sixty-five with the other. The animals appeared in great distress, and endeavoured to eject. Thinks the acid was deteriorated by the light. The comparative effect was precisely the same; insensibility was produced nearly at the same time in both the dogs. Should think it was *most improbable, and next to an impossibility*, that the deceased could have had the power of corking the bottle after taking the prussic acid, and put her hands under the clothes. Comes to that conclusion from the experiments he has made.

Cross-examined. — One dog had forty drops and one twenty before they fell. Dogs require the same doses in other medicines as human beings do. Cannot say as to the effect of the poison. Saw one experiment made upon a horse. Six drachms were administered; it began to be affected in a minute; fell in two minutes; was perfectly insensible and dead. Its heart was beating. The heart beat many minutes after it ceased to breathe; it was full a minute before it was affected at all. Thinks the horse would take strong poisons without being affected; might take half an ounce of arsenic.

Mr. Thomas Macauley.—Is a surgeon in this town. Was present with Dr. Freer and others when the experiments were tried. Concurs in the opinion they have given as to those experiments. Received the bottle containing prussic acid, found in the bed, from Mr. Owston, previous to commencing experiments. Measured the quantity of the acid; there were more than three drachms, but less than four; about three drachms and a half; eight drachms is an ounce. Some of the experiments were made with the identical acid that remained in the bottle; there was a difference in the acid taken from the bottle and the fresh acid; it was very trifling. Has made previous experiments, without a view to this transaction. Without *denying the possibility of it, thinks it extremely improbable* that deceased could have corked the bottle; and, from what he has read of the opinions formed by eminent medical writers, thinks it *impossible* it could be done, the operation having been done in the dark. Has tried experiments on dogs and cats; its operation depends upon the dose; has given five, ten, and twenty drops; some have fallen directly, and some have walked after it for a short time. Don't know what quantity deceased took. Has seen dogs walk moderately well after a small dose; never saw one walk after a large one.

Elizabeth Neale.—Lives servant to Mr. Cox, a baker, in Leicester. On the 10th of February went to Mr. Biggs's shop for a bottle of cowslip wine; it was a little before ten o'clock. Saw prisoner just inside the door. Freer was on the other side of the counter; asked Freer for what she wanted in Freeman's hearing; he asked Freeman to fetch a bottle out of the cellar; he said he should have to go into the kitchen for a light; Freer said he would fetch a light for him; prisoner said he would try to find it in the dark; Freer then said he would get a light, and went for one. There was a gas-light in the shop. Freer went into the kitchen for a light; he was absent a minute or two. Freeman went round on the other side of the counter to a writing-desk; he stooped and reached out a small parcel wrapped in a light-coloured paper; it appeared as if he reached it from under the desk. It appeared a small parcel; the shape of it appeared like a bottle—quite a small bottle; the paper was screwed at both ends; he put it into his pocket. Freer was out during the time; when he came back he gave the light to the prisoner, and he went and fetched the wine.

Cross-examined.—Saw the prisoner take the paper parcel out of the drawer on the Tuesday night; lives near Mr. Biggs; cannot tell whether the prisoner was always there by himself; has gone in sometimes and found him alone. Prisoner took a small book out of the drawer at the same time that he took the bottle; mentioned the circumstance to her sister same night. Prisoner did not read the book; he just looked into it, and then put it into his pocket.

Re-examined.—It appeared a smallish book, with a dark cover; he put it into his pocket, as well as the parcel.

By the Judge.—Should not know the book again if she saw it.

Ann Buswell.—Is mother of the deceased. Lives at Bruntingthorp; remembers Thursday, the 12th February; saw prisoner on that day; he brought a note from Mr. Biggs between ten and eleven that morning; he said it was about very bad news—shocking indeed; desired him to read the note. (The note was read; it was respecting deceased's death, and stated that an inquest was to be held.) Prisoner told her that deceased went to bed about half-past eleven o'clock; that the watchman called the man in the house up about a shutter. Prisoner said he wished the body to be fetched away, to prevent a coroner's inquest; he said, if there was one, perhaps some one or other would wish to have her opened; he said to her, "You would not like that?" He said no more to her; she went down the garden, crying " her poor girl was dead."

Cross-examined.—Recollects the words perfectly. Was examined by the coroner; told him the words used by the prisoner; it was after the deceased was buried; the words are as fresh in her memory now as they were when she was examined. The words the prisoner told her was, " that he wished the body taken away, as it would prevent the inquest."

By the Judge.—Prisoner said, " I wish the body fetched away."

Dorothy Jaques.—Lives at Bruntingthorp; knows deceased's mother; saw her on 12th February; heard her go down the garden crying out that her poor girl was dead; went into the house; prisoner was in the house, standing against the door; said to him, " Is it Judith that is dead?" He said he did not know; it was her that was living at Leicester. He said, " do you think they could fetch the body away to-day?" She answered that she thought it impossible. He said, if it could be done he should think there would be no need of a coroner's inquest upon the body.

Cross-examined.—Did not know that deceased was called Mary in Mr. Biggs's family

Elizabeth Wallace.—Is sister of the deceased; lives at Bruntingthorp; saw prisoner on Thursday, the 12th February; it was near eleven in the morning, in her mother's house. Her mother came to tell her. Had some communication with prisoner when she went in. Her mother was not present; she was looking for her father. Prisoner asked her whether she was a sister of the deceased? She answered, yes. He then said, " No doubt she has gone to a happier world, poor girl.' He said she lay with her head on her hand, with a smiling countenance, as if she was asleep. She asked him if he was the first person that entered the room? He said no, there were three or four before him; he was in bed; Mr. Biggs went out, without coat, waistcoat, or hat, to fetch a surgeon, but she was quite cold. He said he did not think she was in bed till half-past eleven; that the man in the house was called up by the watch at twelve. She asked him if he heard any thing of deceased in the night? He said, no. She asked him if he was near her? He said, " Yes, only a wall parted us." He said he thought it would be the best way to fetch the body away, as it might prevent a great deal of trouble both here and there.

John Buswell.—Is father of the deceased; saw the prisoner at his house on the 12th February, in the morning; prisoner asked him if he thought he could fetch her away? he asked him if he would give him orders to have a coffin made? He said he could get one knocked up in Leicester in three or four hours.

The case for the prosecution having closed, the prisoner was called on for his defence, but he declined making any.

The following witnesses were then called in his behalf.

Thomas Freer.—Is shopman to Mr. Biggs; knows the prisoner; remembers the night before the young woman was found dead in her bed; she slept in a room next the landing-place; the door from the landing-place made a noise in opening and shutting; there was a door to the prisoner's room which he could shut or fasten as he pleased; took one candle for the purpose of lighting both to bed; candle was placed on a chair which stood in the door-way; if prisoner got into bed first, he shut the door and pushed it farther into the room; if I got into bed last, I put out the candle; if he went to bed last, he did. On the 11th February I got into bed last. I went to bed between half-past ten and eleven; I left the servant girl in the kitchen; don't know whether Mr. and Mrs. Biggs were up or gone to bed; was disturbed by the watchman rapping at the front door; I got up; he told me that the cotters of the windows in the next shop were loose. I asked him what o'clock it was; he told me it was twelve. Mr. Biggs had the shop next door then, and has it now. I went to bed again directly, and slept till I was called up about seven in the morning. I heard the workpeople rapping at the door. I heard Mr. Biggs call the girl. I did not get up directly; did not hear any noise that alarmed me. Mrs. Biggs came into my room and told me the servant girl was dead in her bed. As soon as Mrs. Biggs was gone out of the room, I got up and dressed myself. Prisoner also got up. Myself and prisoner went together into the girl's room; both came away together. The door of the girl's room made a noise in opening and shutting. Has been into the room since; observed the door shut easier; it was on the Friday evening, after I was liberated from prison. If the prisoner had got up in the night, he must have opened the door to have come into my room, and must have moved the chair; it stood so that he could not have come out of his room without moving it. If he had opened the door and passed through the room, and had opened the girl's room, I must have heard him. He did not come through; I heard no noise; I found the doors, when I got up in the morning, the same as I had left them. The prisoner had complained of the toothache; I supplied him with something for it. I got out of bed on two occasions, and gave him laudanum, which I had in a drawer; don't know what articles he made use of for the tooth-ache; I gave him laudanum. On the night in question the watchman called me; I spoke to him from the window; after that I heard the clock strike one, but I did not hear it strike two; the partition door was shut on that night; the prisoner put it to in the usual way; I am sure it was shut. I don't recollect telling Mr. Macauley the door was open; I said it was not fastened; I do not recollect saying it was open. I told Mr. Macauley it was put to. I supposed it was shut; did not tell him it was open—that it was put to; it was put to, and I would say shut. I believe it was not open. I will swear it was shut; the chair was put in the door-place of Freeman's room. Has seen how the prisoner and deceased lived together; did not form any opinion, from what he saw, that there was any thing particular between them. Believes that he never said that the young man was connected with her; sure he has not; does not recollect in any instance having said so.

(The short-hand writer was instructed to take the remainder of the examination.)

Q. Do you remember speaking with Freeman one evening about taking liberties with females, when something was said about the servant girl?—A. Yes; it was on that evening I had some conversation with him about his having some connexion with her. Q. Did you use the expression that it would be easy to come round the servant?—A. I said I thought it would be easy to come round her at first, but I refused to have any thing to do with her at all. Q. After that, had you any conversation with him about giving a drug to that young woman?—A. No. Q. Was not that some few days before the girl died? —A. No, but it was not very long? Q. Did he tell you at that time any thing about a drug producing any effect upon a woman? —A. No. Q. Nothing of the sort?—A. Nothing of the sort. Q. Did he not tell you that you were a fool, and that you knew nothing about it?—A. No. Q. Did he say that to you at no time?—A. No. Q. Now I have asked you whether you stated that you thought there was some truth in the account that Bankin had given, and whether the boy did not say that he had made use of the words, and that it was the truth?—A. Yes. Q. You said he denied that it was the truth? —A. Yes. Q. Pray did you always give the same account before the magistrate?—A. I cannot say that I did in the first instance, because I did not at first understand the lad whether he admitted it or not; but when I came to recollect, he did not admit it. Q. Will you tell me the words that he used?—A. He said, "No, Freer, it was not so." Q. But could that leave a doubt upon your mind whether he admitted it or not?—A. I did not catch it distinctly at first, but still it was what he said. Q. When did you first catch it distinctly?—A. I can scarcely tell; part was said in my hearing, and part was not; I was called, at the instant he was speaking, into the shop, to wait upon a customer. Q. How came you to recollect, then, that he said, "No, Freer, it was not so?" —A. I can scarcely tell; but he did say those words. Q. How can you tell that?—A. Because I have thought upon it since. Q. Where did the conversation take place?—A. It was in the back-room at the shop in King-Street. Q. Do you tell me the words again that he said?—A. "No, Freer, it was not so." Q. What had you been speaking of, that made him say that?—A. I said, I thought there was some truth in what he had said to Bankin. Q. What did he say to that?—A. "No, Freer, it was not so." Q. I thought you said he denied he had used those words to Bankin?—A. No. Q. He only denied, then, that it was not true?—A. Yes. Q. Now do you mean to swear, that when he used those words, "No, Freer, it was not so," that it left on your mind any doubt whether he meant to deny the truth of the statement or not?—A. Yes, I did think so. Q. You mean to swear that?—A. Yes.

By the Judge.—Did you ever suspect this young woman was in the family way?—A. I did a short time before the circumstance of her death. Q. How did she take the liberties of this young man before you? —A. She was displeased at them.

Mr. Thomas Cooke.—Is an architect [a plan is produced]; examined the girl's bed-room door; to shut the door so as to bolt it would require force; it would latch pretty easy. Examined Freer's door; it had a lock upon it, which appeared to have been on several years; the key would not turn; could not open and shut the door without making a noise.

Cross-examined.—The door might be opened by a person in the inside, but not without making some noise; opened it himself; it might be heard by a person listening not far off, but thinks it could not be heard in Mr. Biggs's room.

Reverend Jerome Dyke.—Lives at Burbage, where prisoner's father lives; knows prisoner; he is 17 next October; never heard any thing amiss of him; has seen him at church; he seemed attentive; he was at home last Christmas for a short time.

Reverend Samuel Allen.—Keeps a school at Hinckley; prisoner was two years and a half at his school; he quitted it on his going to Leicester; has been at Mr. Biggs's at different times; has known him six years as a pupil; he was of an amiable and humane disposition.

The evidence being closed on both sides, the Judge recapitulated to the Jury the various facts disclosed in the course of the trial; after which they deliberated for a few minutes, and returned a verdict of—*Not Guilty.*

NOTICE.

It is not convenient for us to comply with the requests contained in the letter addressed to us on the 9th instant.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

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LECTURES
ON
THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

BY DR. ELLIOTSON.

LECTURE III.—Oct. 7, 1831.

General Observations—Explanation of Terms.

I MENTIONED, gentlemen, in the last lecture, that the occurrence of death—even of premature death, and the occurrence of disease, whether leading to death or terminating favourably, were nothing more than circumstances in perfect unison with the operations of nature throughout the animal and vegetable kingdom; and, indeed, throughout inanimate nature also.

I stated that there was not only a definite period for every animal and vegetable to exist, but that the greater part were cut off before that period arrived; and not only so, but that those which were not cut off suffered more or less derangement from time to time during their existence;—that we were so dependent upon all the circumstances around us for our existence, and that these circumstances were so liable to change, to be in a state of deficiency or depravation, that it was scarcely possible to escape disease.

I mentioned that disease, perhaps, might be defined to be an aberration of function, or structure, or size, in any one or more organs, or an aberration in quality or quantity of any of the fluids, from the sound state; but that it was necessary that a certain degree of these unhealthy circumstances should exist for us to say that a person was labouring under disease; and therefore there is generally added to such a definition as I have just given, "rendering assistance desirable or necessary." I mentioned this as a definition of disease in general; and I stated that with respect to particular diseases we usually defined them, not from their ultimate nature, but merely from their appearance—from the symptoms—that is, from every thing that was observed in an unnatural condition in a sick person: the most evident, the most constant, and the chief of these are grouped together, and said to constitute a disease. We do not generally look at the nature of the affection, but merely at the concatenation of the most important, and as far as we can judge, indispensable symptoms. Yet, if the proximate cause is evident, we may put it into the definition, or name the disease from it. For instance, certain symptoms arise so clearly from inflammation of the bowels that we do not hesitate to name the disease inflammation of the bowels, or enteritis. But we seldom know the proximate cause, and thus are content with calling certain convulsions, the falling sickness, and a certain change of colour, jaundice or yellowness.

With respect to the cause of all these symptoms, that, I mentioned, is called the proximate cause, which, when present, makes the disease, which, when changed, causes a change in the disease, and the removal of which causes a removal of the disease. According to the words of writers, *præsens morbum facit, mutata mutat, sublata tollit.* I stated, that besides this cause of the disease, which is called the proximate, those causes which produce this indispensable state, whatever it may be, and which gives rise to the symptoms, are styled remote causes; and I mentioned that these are divided into two—the predisposing, those which give a liability to disease; and the exciting, those which immediately produce it.

I mentioned a few varieties in regard to diseases; that sometimes they affect merely function, sometimes they affect structure, sometimes they affect the solids, sometimes the fluids, sometimes both, sometimes they are general, sometimes local; that they are sometimes acute, at others chronic; that they may be continued, remittent, or intermittent; that they differ likewise in their fundamental nature; that they, of course, differ according to the cause which produces them, according to the texture which they affect, and likewise according to the organ to which that texture belongs.

I must now enter a little more minutely into the explanation of certain expressions which will be used throughout the course, because, unless they are understood, it will be impossible for me to speak intelligibly. The whole doctrine of disease together is called *general pathology*. Pathology is the science of disease—the *ægroti hominis scientia*—while physiology is the science of man in health. I of course mean human physiology. With regard to pathology, I stated in the last lecture that sometimes an attempt has been made to limit the term to signify diseased structure, but certainly pathology implies the symptoms of a disease, and every thing relating to disease, as well as the particular circumstance of change of structure.

General pathology is commonly said to embrace four particulars: First, general *nosology*—that is to say, an account of diseases in general, and an account of their chief differences. Secondly, an account of the symptoms of disease, which is called *semeiology*, or *symptomatology*. Thirdly, the causes of disease, which is called *ætiology*. And, fourthly, the treatment of disease, which is called *therapeia*.

With respect, however, to pathology, there is not only a general but a special pathology, which respects particular diseases; and if the epithet *methodical* is attached to *nosology*, an arrangement of diseases is meant—a classification of the various affections to which we are liable. I shall first speak of what is called *general nosology*, or the consideration of diseases in general. These may be local or general: when they are local, however, they seldom continue so without producing more or less general effects. I stated in the last lecture that some contend that all diseases are local—that they are seated in some particular spot or organ; but I mentioned my dissent from that doctrine, because in scurvy, for example, all the blood seems vitiated, and all the solids, without our being able to fix upon any particular spot. Diseases vary in their symptoms, and vary in their course, according to the texture which they affect. You know that the body consists of a certain number of textures—that the various parts of the frame may be resolved into a certain number of particular tissues, or textures, as they are called. Now some diseases affect certain textures, and others affect other textures; and the symptoms of diseases, and their course, are influenced very much by the texture which is thus affected. To take inflammation, for example, we shall find that the symptoms are very different when it affects a serous membrane, from what they are when it affects a mucous membrane. Again, the symptoms are very different when it affects the skin, or when it affects a bone. The symptoms of disease, too, and the course of diseases, vary exceedingly according to the organ. Diseases may not only attack a particular organ, but they may attack a particular texture in a particular organ, or they may attack all the textures of an organ. Now when they do attack any particular organ, the importance of that organ to the economy, the function which it performs in health, the whole set of relations of that organ, greatly influence the symptoms. For example, inflammation which may be of no consequence in the finger, may, though only to the same extent, be fatal when seated in the stomach; a little ulceration of not the slightest importance on the hand may produce death if it occur in the stomach; an affection in the brain, or in the heart, may be productive of very different results—a very different degree of danger from what it would be if seated in many other parts.

Diseases likewise vary exceedingly, accordingly as they are attended by excessive or diminished action. Some diseases are characterized by an inordinate violent action of the part; the natural state of the part seems augmented. Whatever is done in health is done ten times more in the disease, at least for a time, till the strength is exhausted, or the state gradually subsides into health. Other diseases are characterized from the beginning, or nearly so, by a great diminution of action; the natural functions of the part decline exceedingly, so that the depression is the character of the disease. If the part affected be one characterized by feeling, as the surface of the body, the feeling may become very intense; if it be one whose function is motion, motion may become very intense, and you may have convulsions as the consequence. On the other hand, you may have a diminution of feeling, perfect torpor, insensibility; and such an absence of all power of motion that the part is useless to the individual. Again, if the part be one which secretes, the secretion may be in excess; and if it cannot escape we have dropsy. The secretion, on the other hand, may be exceedingly diminished, so that in the case of the kidneys no urine can be formed, and in the case of the skin the surface shall be perfectly dry.

Some diseases, among structural affections, are attended by a great dilatation of the part—the part becomes much dilated, beyond its natural extent;—on the other hand, in some diseases, the parts become contracted and diminished. In some diseases the parts become exceedingly hardened, and, from being naturally soft, they become as firm as cartilage, and are sometimes converted into absolute bone. Again, very firm parts will occasionally become perfectly soft, and bone will become as pliable as muscle. I myself have seen the liver, to all appearance, become softened down to a pulp in the course of three days.

Again, diseased parts will become exceedingly full, distended with fluid; and this may be general or local—it may be local congestion or universal plethora of the body. In other instances, the parts become devoid of fluid and shrink, and this is observed also generally or locally. Alterations of texture likewise take place, and alterations in the quality of the fluids. The texture—to say nothing of induration, softening, hypertrophy, or atrophy—the texture of the parts sometimes becomes perfectly different from what it is observed in the state of health. So with respect to the fluids, they will frequently be formed of a character totally dissimilar from what they should be. The urine is sometimes any thing but genuine urine, and the bile is sometimes so pale that you can scarcely recognize it as bile; and so with respect to the blood itself—it is sometimes very little more than a reddish watery fluid.

These are important and essential differences in disease; there are, however, many other varieties, which are considered more or less accidental—that is to say, which do not affect the nature and course of the disease. For example, some diseases are hereditary, and this will not at all influence the nature of their course or duration; but some diseases may be hereditary in the particular individual, and other diseases are not hereditary, not transmitted from parent or ancestor to offspring or posterity, but still are born in the individual—seem to be implanted in him at his first formation; and then they are called only *connate* or *congenital*. A disease which is connate, or congenital, may be hereditary; but it is not necessarily so. All diseases which are not hereditary, nor connate or congenital, are said to be *acquired*.

Then diseases—to speak still of accidental varieties—are sometimes primary and sometimes secondary. Sometimes the disease which occurs is the first; but sometimes after one disease has taken place it is joined by another—that is to say, exists with another, or gives such a predisposition that another is produced; but it is of no consequence to the nature of a disease, whether it is primary or secondary.

Again, diseases are sometimes said to be *sporadic*, sometimes *endemic*, sometimes *epidemic*. A disease is said to be sporadic when it occurs from an accidental circumstance occurring to any particular individual—when it is a solitary accidental case. Hence, when a person, by exposure to cold, suffers inflammation of the lungs or intestines, he is said to labour under a sporadic disease. These are insulated cases, having no connexion with each other, not having any general prevailing cause. But if diseases occur from some general and temporary cause affecting a whole population, then they are said to be epidemic. Sporadic and epidemic, therefore, stand contrasted with each other; the former class of diseases being those which occur occasionally and accidentally, from some peculiar circumstance happening to the individual; epidemic are those which affect a large mass of persons from some general prevailing cause. Again, diseases are sometimes said to be endemic—that is to say, when they do not arise from any general influence at a particular time, pervading a population, but when they spring from certain causes fixed and inherent to a certain place—when certain places in which people reside send forth causes of disease which all persons who come to that spot are liable to, but which persons do not suffer unless they go there. This is the distinction between endemic and epidemic diseases—the former being confined to the inhabitants of a particular spot, and the latter being those which arise from a general cause, taking place only from time to time, and pervading, not a particular spot, but a considerable population.

Another difference in diseases, not at all affecting their nature, their course, or their treatment, is, that they may arise from a common cause or from a specific contagion. Some diseases are contagious and some are not, but the symptoms do not vary in their essential nature; so that we are continually very much in doubt as to whether a certain disease is contagious or not. There is nothing in the disease itself to make us say that it must necessarily be contagious, and the greatest disputes are carried on as to whether a particular affection is or is not contagious.

Diseases have likewise been distinguished according to the time of year at which they occur, so that we have vernal, summer, autumnal, and winter affections—*vernales*, *æstivi*, *autumnales*, *hyemales*. Other accidental differences of disease arise from their seat, whether they are internal or external—and then, again, as to whether they are fixed, or whether they wander. If they cease in one particular part, and appear in another, they are said to be *metastatic*, or *retrograde*, or *retrocedent*—if disease suddenly cease in one part, and another become affected, a metastasis, or change of place, is said to have occurred.

Then diseases differ according to their severity. Some are exceedingly severe, others are light—some are said to be mild, and others are said to be malignant. Those which are said to be of a malignant character, however mild they may be in appearance, are sure to baffle all the resources of our art, unless we can eradicate the affected part by surgical operation, which sometimes is, but sometimes is not, possible. However, the word malignant is sometimes employed simply to denote a certain degree of severity; it is a term of rather indefinite meaning. Small-pox is said sometimes to be mild, and sometimes malignant;—and, again, a sore which is of a cancerous nature, however trifling the pain it may be attended with at first, but which is sure to go on to ulceration, violent pain, and, at last, destruction of the part, is also said to be malignant.

Diseases vary likewise as to their duration. If they be exceedingly short, so as to terminate within four days, they are called by Latin authors *acutissimi;* but if they terminate within seven days, then they are called *peracuti.* We have no English words to signify the difference between these two durations. If they terminate within 14 days, they are called *acute;* if they run on to 20 days, then you will find authors calling them *sub acute;* and some are so nice in their terms, that if they last forty days they call them *acuti decidui;* and all that run on above that period are called *chronic.* It is sufficient, perhaps, to remember, that an acute disease is considered one which terminates within 14 days—a sub-acute one which terminates within 20; for though some authors make a still more nice distinction, and call a disease sub-acute which terminates within 20 days, and apply the term *decidui acuti* to those which extend to 40 days; yet if the disease extend to or above 40 days, we may consider it chronic, at least I fancy a patient would think we were justified in so denominating it.

Diseases, again, are considered as they are continued, or intermittent, or remittent. *Continued* diseases are generally understood to embrace two varieties as to course; namely, diseases which pursue one uniform course to the end, and are called *continuent*—and those which relax from time to time, and are called *remittent.* You find many persons speak of continued disease as distinct from remittent, and it is so common that we are justified in using the distinction; but some make a nicer distinction, and use the word continued to include *continuent,* that is to say, one which preserves the same tenor to the end, and *remittent,* one which, although it never ceases, yet relaxes from time to time. Diseases, however, are sometimes *intermittent;* they absolutely cease altogether at intervals, and then recur again. If intermittent diseases recur and disappear at regular intervals, then they are called *periodical.* A periodical disease is necessarily intermittent; but an intermittent disease is not necessarily periodical. Intermittent embraces the circumstances of simple intermission, no matter when or how; but it embraces also regular intermission—periodical disease. The presence of a disease which is intermittent, is called the *paroxysm* or *exacerbation,* these being considered synonymous terms, and the interval which occurs between them, is called the *intermission,* or, in case of febrile disease, the period of *apyrexia*—the period of the absence of pyrexia.

I fear being troublesome in mentioning the meaning of so many terms, but it is absolutely necessary that they should be understood before we proceed to speak of particular diseases; and I will now make a few more minute observations in regard to what I said of the causes of disease.

The remote causes I mentioned have been divided into two — the predisposing and exciting; those which are exciting, you will find are by some authors called *occasional* or *procatartic.* Generally, in England, we say predisposing or exciting, but in foreign books you will find a variety of other names employed.

In regard to the predisposing causes of disease, they depend upon a very great number of circumstances. Each particular age, for example, is predisposed to particular affections: the child is predisposed to one kind of disease, the youth to another, the adult to another, and the old man to a very different set. Climate, also, causes a great difference in the disposition to disease; the season of the year, likewise, has a great effect; the habit of the individual, as to temperance and sobriety, as to a natural or unnatural and artificial course of life, has a peculiar influence; all these circumstances cause a predisposition to different affections. In regard to some diseases there is not so much a predisposition required as a want of indisposition. For example, almost every one will take the small-pox, the measles, and the hooping-cough; so that we can hardly say that there is a predisposition to these diseases; the cases in which they do not occur are rare exceptions; and we are more justified in saying, when these diseases will not occur in spite of the presence of contagion, that the person is indisposed to them, than that those who catch them are predisposed to them—that is to say, by nature we are all more or less liable to them; and it would be almost an abuse of terms to say that we were predisposed to them; all that appears to be requisite is, that there should be no indisposition to them.

In regard to the exciting causes of disease, they have been generally classed in three orders—those which excite or stimulate, and those which depress or debilitate, and those which exert a physical or physico-chemical action. Heat, and excess in wine, are stimulating causes of disease; the depressing passions, and an excessive loss of blood, may be instanced as debilitating causes of disease; and these debilitating causes you will find, when you read some Italian authors, spoken of as *contra-stimulating* causes. Many causes, however, cannot be said to depress or stimulate solely; they exert a peculiar action, which is not to be thus explained—it is something we do not understand. For example, malaria, which produces ague, is a certain agent perfectly unknown to us, except so far as regards the situation in which it is produced. Malaria produces a set of symptoms of so peculiar a nature, that we cannot say it is a mere depressive or contra-stimulating agent—that is to say, though the first symptoms of ague produced by it are those of extreme debility, yet if we weaken the body to the same extent in various ways, by bleeding and other means, we do not produce that train of symptoms which are known to us as intermittent fever. Therefore, we have stimulating, depressing, and physical or chemical causes, which are sometimes called *stimulantes, contra-stimulantes,* and *irritantes.* We must not, in medicine, be very nice about the etymology of our words—that is to say, we must employ words of a certain meaning, so as to be clearly understood; but we shall find words signifying something very different from what their etymology would lead us to suppose. Nor is this remark confined to physic; it applies equally to anatomy; and the only object should be to become understood among ourselves—to know what ideas are meant to be conveyed when certain expressions are used.

An exciting cause may become a predisposing, and a predisposing an exciting. For instance, malaria excites ague. But a person who has been exposed to malaria may not have ague till he accidentally gets wet through, and then this excites the disease to which the malaria had only predisposed him. Again, if a predisposition continues to increase, disease will happen without any exciting cause. Plethora of the head may increase till a fit of apoplexy occurs, without stooping, overloading the stomach, or any common exciting cause of a fit taking place.

Among the most common causes of disease are aberration with regard to six particular circumstances, which have been very strangely called *non-naturals.* The air we breathe, the food and drink which we take the retentions and excretions, motion and rest, sleep and watching, and the various passions of the mind, which are all natural enough, are called by old writers *non-naturals*; and others, seeing the strangeness of this expression, have called them *things necessary to life*—that is to say, things which are necessary in order to the continuance of our existence. The great sources of disease are an aberration from that state of all these which conduces to health. For example, with respect to air; vitiation, I need not say, produces a variety of diseases—fevers, and many others. I need not say how much our health must depend upon the wholesomeness of our food and drink, and upon a proper quantity of them. I need not say that the body cannot bear more than a certain degree of exertion—that many diseases arise from fatigue; and that, on the other hand, excessive rest and indolence produce a collection of fluids in various parts of the body—stagnation, plethora, fatal dropsy, and various organic diseases; so with respect to sleep and rest, alternations are absolutely necessary; and we every day see persons who, for want of sleep, become the victims of fever, and gradually of the most destructive diseases. So with respect to the passions of the mind; there is scarcely a disease, with the exception of those that depend upon contagion, to which they do not give rise; nor is there any one part of the body, in fact, exempted from disease through the passions of the mind. No one can imagine what a vast number of diseases, not only functional but organic, arise simply from unpleasant passions of the mind—that is to say, from grief and deep-rooted sorrow, from violent anger, from regret and chagrin, and all those feelings which are of an unpleasant character. On the other hand it is also to be remembered, that passions of the most agreeable kind, excessive joy, for instance, may have almost as injurious an effect as those which are of an opposite character.

DR. PHILIP'S REPLY

DR. PROUT'S PAPER IN THE LAST NUMBER OF THE MEDICAL GAZETTE, ADDRESSED TO THE EDITOR.

To the Editor of the London Medical Gazette.

Cavendish Square, Oct. 17, 1831.

SIR,

I HAVE already said that I cannot stoop to Dr. Prout's language; to his epithets therefore I make no reply. I shall confine myself to those faults of the style, and substance of his paper, which interfere with the questions between us.

I expected that after so long an interval, his reply would have been in a different tone; but it is the same in its expressions, its inaccuracies, and its shifting grounds without acknowledgment.

As I send with this reply an essay, being an attempt to ascertain how far chemistry is applicable to the explanation of the phenomena of the living animal, and the cure of its diseases, I shall here only make some general observations in support of the foregoing statements, and adduce a few specimens of Dr. Prout's mode of replying, and a few of the facts to which he makes no reply. It is quite evident, that from discussion with an opponent who only shifts his ground on every new exposure of inaccuracy, nothing profitable can arise. The course I am about to pursue will, I hope, place the subject of our dispute in a clear point of view, which the observations of my antagonist serve only to perplex.

His language, indeed, is generally obscure, and sometimes such as makes it impossible to arrive at his meaning. The peculiarities of his style, as every reader must remark, whether he speaks of himself or others, whether in his lectures or his replies, every where, in short, except in his account of his chemical results, where there is no room for them, are the substitution of positive assertion for all proof and even illustration, and promised for present performance. It is remarkable that in his replies he constantly accuses me of a fondness for verbal disputes, while I have been obliged in almost all of mine to complain of his dwelling almost solely on this part of the subject, and not replying to the facts I had adduced, a complaint I am now again obliged to repeat, and shall presently illustrate by instances.

The truth is, that on the one hand the discussion is constantly encumbered by the extreme inaccuracy of Dr. Prout's expressions; and on the other, as he finds it more easy to quibble about words, instances of which I am just about to adduce, than facts, it is very difficult to obtain from him any reply respecting the latter, proofs of which I shall also adduce, for I have throughout the discussion made no unsupported assertions; while on the part of Dr. Prout all the assertions are of this kind, which consequently may be, and in fact almost always are, the mere expressions of an angry man.

I am now to adduce specimens of Dr. Prout's mode of replying. A few, I believe, the reader will think sufficient, and I have no wish unnecessarily to prolong this reply.

In the 40th page, at number 4, he observes, "the next point which I deem it necessary to notice is Dr. Philip's opinion on the nature of the nervous principle." "Does Dr. Prout (Dr. Prout here quotes my words) then consider the terms nervous and living principle as synonymous? principles that have not one quality in common?" Dr. Prout proceeds, "To which I answer that I *do* consider the nervous principle to be a living principle, and beg to ask who besides Dr. Philip does not?" I might, I believe, with great truth, reply, those who are better acquainted with the subject than Dr. Prout: but of this hereafter. Our present object is Dr. Prout's mode of replying to my question.

I accuse Dr. Prout of using the terms nervous and living principle as synonimous. To which he replies, that he *does* consider the nervous principle to be a living principle. It is hardly possible to suppose that he does not see that, whether the nervous principle be a living principle or not, it cannot be synonymous with the living principle usually so called, which pervades every part of the living body; whereas it is notorious that the nervous principle is confined to a certain set of organs. For what purpose, then, does he make this reply? Is it with the hope of persuading the careless reader that he has made a correct reply?

I must beg the reader's patience while I adduce one more instance of the inaccuracy, to say the least of it, of which I complain.

I have repeatedly called Dr. Prout's attention to his new term merorganization, and pointed out the inconsistencies attending its composition and employment. He at length gives his reply. It will at once elucidate the nature of this reply, to ask Dr. Prout why he labours in it to confound the terms different *degrees* and different *kinds* of organization,—for, according to his present *corrected version* of what he formerly said, these terms are used indifferently. He says, "Does the organization of the zoophyte not differ, *in kind or degree*, from that of the human being?" And in another part of the same page, "admitting the existence of different *degrees or kinds* of merorganization." In the original definition, we see nothing of difference of kind. The term expresses only difference of degree. He there says, "I have provisionally adopted the term *merorganized* (μερος, *pars* *vel partim*), meaning to imply by it that bodies, on passing into this state, become *partly, or to a certain extent, organized*."—Phil. Trans. for 1827, p. 375.

I objected to a word which implies different degrees of organization, and it is changed to different kinds of organization, to which the objection, of course, does not apply. He affects to think *kind* of the same import here as *degree*; for it is difficult to suppose that Dr. Prout really does not perceive that these terms are, as far as applies to our argument, of the most opposite significations, the one exposing him to the whole force of my censure, while the other as perfectly relieves him from it. I accused him of introducing a term to designate different degrees of organization, without being in possession of a single fact to prove that such degrees exist. He attempts to relieve himself from the censure, by pretending that by degree he means kind! That there are different kinds of organization, of course, no one can question. It would be difficult, I believe, in a grave discussion, to find a parallel instance of defence. Besides, Dr. Prout, in his hurry to escape the censure, forgets that this new meaning suits not the composition of his term. He forgets also, that such means could blind no man whose opinion was worth a moment's consideration. It was Dr. Prout's part either fairly to shew on what principle he spoke of different degrees of organization, or to confess his error.

So much for the meaning of his term; his attempt to vindicate his use of it replies to itself. I repeat, that if the animal merorganizes the food, it must, according to his original definition of the term, possess a disorganizing power, because all admit that the food is wholly organized when it enters the stomach; and Dr. Prout *says*, it is there rendered merorganized, and therefore, according to that definition, only "partly, or to a certain extent, organised*."

I shall conclude with a few of the instances in which he finds it convenient to make no reply; for it is not so easy to perplex the reader respecting facts as terms.

In the 4th page of my observations on Dr. Prout's Gulstonian lectures, I have stated, that in his summary of digestion he confines himself to the mere chemical part, overlooking all the other equally essential parts of that process. My words are, "although the gastric juice be healthy, and consequently capable of effecting the due chemical changes on the food, the functions of the stomach may, notwithstanding, be wholly deranged, and no proper chyme transmitted to the duodenum. If that portion of the food which has undergone the action of the gastric juice be not duly separated, and kept apart, from that which has not, and in this separated state duly carried forward towards the intestine—if an error in any one of these functions take place, the office of the stomach is just as certainly deranged as if the chemical part of the process had failed; yet, in Dr. Prout's observations on the office of the stomach, the chemical part of it alone is referred to, as if it were in this alone it consisted."

The present is the third reply I have written to Dr. Prout since the publication of the foregoing sentences, but I have been able to obtain no reply from him respecting the facts stated in it, and others which I have pressed on his attention.

I shall trouble the reader with but one other instance of this kind.

In the same page of my observations on Dr. Prout's Gulstonian lectures, the reader will find the following paragraph:—

"Prepared as the reader must now be for Dr. Prout's partiality to his favourite science, he can hardly be prepared for the following prediction in the 9th page of his lectures:—'I will venture to predict, that what the knowledge of anatomy at present is to the surgeon in conducting his operations, so will chemistry be to the physician, in directing him generally what to do, and what to shun.' Does not the surgeon guide his knife solely by a knowledge of anatomy—a knowledge of the parts and their relative position? Will it be asserted that, let our knowledge of chemistry be as perfect as we can suppose it, the physician could regulate his practice on chemical principles alone? Could chemistry give him any knowledge of even the simplest principles of medical treatment? Could it tell him, for example, that a copious discharge from the bowels will relieve the turgid vessels of the brain; or that that by the skin will allay the symptoms of fever? But Dr. Prout may say, it will assist in his choice of the means by which these effects are produced. Undoubtedly it will. Here chemistry is in its proper place; but of what use would the chemical knowledge here be, if the laws peculiar to the animal body had not, in the first instance, supplied the indication of cure?"

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* Dr. Prout says I am unacquainted with the refinements of modern chemistry, but, as is his custom, adduces no instance of my ignorance. The degree of my knowledge has nothing to do with the question between us. However great may be the improvement in chemical knowledge, no improvement can change the nature of the science. With regard to Dr. Prout's desultory observations on galvanism, the accompanying essay will show that, however applicable they may be to his chemical experiments, they are wholly inapplicable to the functions of the living animal; and independently of this consideration, it is ludicrous to attempt to disprove a simple fact by any train of reasoning. See page 76.

To this paragraph Dr. Prout, after having been repeatedly urged to reply to it, replies by the following passage, in the 44th page of the last number of the Medical Gazette—I say to the paragraph, for to the fact there is no reply:—

"Dr. Philip next attacks what I have said on the powers of chemistry, when sufficiently understood and rightly applied, of 'directing the physician generally what to do, and what to shun;' and after stating a variety of objections quite beside the point in question," (what are they!) "ends by observing, 'Dr. Prout may say that chemistry will assist in directing his choice of the means by which these effects are produced. Undoubtedly it will. Here chemistry is in its proper place.' But then (just as if I had laid down the rule, neglect the laws of the animal economy, and attend only to chemistry) he lapses into the old story, and exclaims, of what use would this chemical knowledge have been, if the laws peculiar to the animal economy had not, in the first place, supplied the indication of cure?"

The substance of this pretended reply may be stated in a few words. Dr. Prout's original position is, that what anatomy is to the operating surgeon, chemistry will be to the physician, in directing him generally what to do, and what to shun. When he is told that chemistry can never, in the nature of things, be to the physician what anatomy is to the operating surgeon, because it cannot direct him even in the simplest case of medical treatment; while by anatomy alone the operating surgeon is directed in all cases without exception; he neither attempts to prove his own position, nor to disprove his antagonist's, but greatly modifies the former, and simply *asserts* that the latter is quite beside the point.

Dr. Prout continues, "I observe in his last letter the doctor recurs again to this point, and places in juxta position the above observation, and one in which I have elsewhere said, that I do not know a single instance in which chemistry can be directly applied to the purposes of the animal economy, as if the two were in opposition to each other. But the fact is quite otherwise. The absurd idea, indeed, never entered into my head, that chemistry at any time will ever be capable of being directly so applied; but that it will be capable of doing all I have anticipated, in an indirect manner, I have not the least doubt." This is one of Dr. Prout's promises; but he does not say, nor can any human being conceive, how it is to be accomplished; and as to reconciling the two passages contrasted in my last reply, and to which he here alludes, this is an attempt he very prudently declines; and thus adds another to the many instances in which I in vain look for a reply.

Dr. Prout still accuses me of not having given his favourite science a place in my Inquiry into the Laws of the Vital Functions. I explained, in a former reply, that it is only in a certain department of physiology that chemistry has any place. Now my Inquiry does not include this department; because, as I have explained in that Treatise, it is an attempt to trace the general laws of the animal economy, not to descend into the particulars of its various processes; and when Dr. Prout says, that if I had availed myself of the aid of chemistry it would have rendered my positions more perfect, of which assertion, as usual, he gives neither proof nor illustration, he only shews himself unacquainted with the nature of the Inquiry; and, indeed, has, in several of the instances which have been adduced, betrayed a very imperfect knowledge of its subject. My object was to obtain and communicate a correct and comprehensive view of the great laws of the animal economy, respecting which there had been, among physiologists, so much discussion and so much contrariety of opinion—not to trace minutely the steps of any particular class of its phenomena, but to determine the great outline to be filled up by the more partial and minute labours of those who were to follow me. Accordingly, in one of the last papers which the Royal Society did me the honour to publish, I observed, "I am fully sensible of the vast extent of the subject, and that it is only the great outline which I have attempted to trace. If this has been accurately laid down, my object has been accomplished."—Phil. Trans. for 1829, p. 262.

I cannot conclude this correspondence without saying that I have reason to complain of Dr. Prout, in which, I think, every candid reader will agree with me. If I was in error, why not calmly point out the error? To substitute assertion for proof, and abuse for argument, might hurt his opponent's feelings, but could not in any other way promote his views.

Dr. Prout again refers to the origin of our discussion. I therefore again say, that it was with great reluctance, and not till after much hesitation, that I entered into it. Having devoted so much time and labour with a view to the improvement of physiology, was it not painful to be publicly told, by a person supposed to be well versed in the subject, that no advance had been made in it for 20 years, as nearly as possible the period of my labours? He says, indeed, this observation did not apply to me, because he includes physiology in metaphysics; but this he did not explain; and it was impossible, either for myself or any one else to know it. Besides, I have shewn that, now that he has given his explanation of metaphysics, it will not apply to the expressions used in his lectures. I venture to maintain, that he cannot find one man who will say that he could, from his lectures, infer the explanation he has now given.

As to the note in my first communication, to which he refers, there is nobody, except himself, who will not at once see, that what is there said is merely adduced as a proof of his inaccurate use of the term, and can admit of no other explanation. Two things, at all events, must be admitted, that if he did use the term in the new sense he now explains, he should have defined that sense; and it should have corresponded with the expressions used in his lectures, which I have proved it does not.

As I have reason to hope that this may be the last reply that I shall feel myself called on to make to Dr. Prout, it only remains, sir, that I should thank you for the attention with which you have honoured my communications. I hope the essay which accompanies this letter, and which presents a summary view of the functions of the more perfect living animal, in several respects different from any other with which I am acquainted, will, if it prove to be correct, in some degree compensate for the tediousness of this reply. Nothing but a desire to maintain the truth could have induced me thus long to persevere in such a discussion.

I am, sir,
Your obedient servant,
A. P. W. Philip.

A REVIEW
OF
THE FUNCTIONS OF THE LIVING ANIMAL BODY;

Being an attempt to ascertain how far the Science of Chemistry is applicable to the Explanation of its Phenomena, and the Cure of its Diseases.

By A. P. W. Philip, M.D. F.R.S. L. & E.

From the following view of the functions of the living animal body, founded on the statements in my first communication to the Medical Gazette, it will not be difficult to see how far chemistry is applicable to explain its phenomena, and assist in the cure of its diseases.

The functions of the living animal body may be divided, according to their objects, into two great classes; those which maintain its existence, and those which connect it with the external world. Corresponding to this division we have seen them divided, according to their nature, into the sensorial, on the one hand, and the nervous and muscular on the other. The sensorial alone are the source of those functions which connect the more perfect animal with the external world. The nervous and muscular alone are the direct means of maintaining its existence.

The difference in the nature of these sets of functions, is essential. There is a chasm between them which no labour can enable us to fill up; and the attempts which have been made with this view have only convinced us of their futility.

Thus different in their nature and their objects, in their working, if I may use the expression, they are intimately associated. As the life of the animal directly depends on the nervous and muscular functions, as far as we see, the sensorial functions cannot exist independently of them; and we certainly know that, in the living animal, the sensorial can neither operate, nor be operated on, except through the nervous and muscular functions; nor are the nervous and muscular independent of the sensorial functions, because the animal has not the means of existence within itself, and the nervous and muscular powers afford no means, I speak of the more perfect animals, of obtaining the necessary aids from without, except under the direction of the sensorial powers.

As the animal becomes imperfect, and approaches the nature of the vegetable, the sensorial power dwindles; and the lowest animals can extract their nutriment from air and water, which being generally diffused, are at hand, and consequently obtained without any sensible effort on the part of the animal. His life, therefore, although not independent of the external world, is, like that of the vegetable, independent of any act of volition.

As we rise in the scale of animals, the sensorial power becomes more perfect, and in the same proportion more essential to existence. From those animals which obtain food without any act of volition, we come to those who can only obtain it by such an act; but who still, without any act of this kind, obtain the influence of the air, yet more immediately necessary to their existence.

We arrive at length at the most perfect class, which can neither obtain food nor air, except by an act of the sensorium; for in them the sensorial power is as necessary for the inhalation of the air, as the ingestion of the food*. When sensation ceases, the more perfect animal as certainly ceases to breathe as he ceases to eat.

Such appears to be the constitution of our bodies as far as our existence depends on the external world, and therefore on the sensorial power. But with this world the sensorial power connects us in a manner far more extensive and infinitely more varied. It is the source of enjoyment, the end of our being.

Here, as in maintaining life, it is still through the nervous and muscular powers that the sensorium operates. Through the former it receives all its impressions, and transmits all its dictates. But the nervous, like the sensorial organs, are incapable of communicating any impression beyond the limits of the animal body itself. For this purpose the muscular system is necessary.

By its means our powers are rendered active with respect to the external world. As through the nervous system we are influenced by that world, through the muscular system we can influence it. By giving motion to our limbs, it enables us to impress the inanimate objects which surround us, as by giving language to our thoughts, to influence the sensorial powers of other beings like ourselves.

It is thus by the impressions we receive and communicate that the wide field of enjoyment is opened, and not to the individual alone, but through him to other beings, who resemble him, and who repay him in kind. Thus happiness accumulates, and with it knowledge, for the experience of each becomes the experience of all, and the savage is changed into the industrious mechanic, the learned academician, and the contemplative philosopher.

But our present object is not the relation which the individual bears to the world which surrounds him, but the powers on which his own existence and well-being depend; and I think it will be found on an inquiry into the nature of these powers, that we have sufficient data to determine how far they are peculiar to the living animal, or partake of the nature of those which operate in the inanimate world, and consequently how far we may indulge in a rational expectation of deriving assistance from other sciences, in tracing the laws of the science most interesting to us, that of our own bodies.

I had occasion, in my first communication, to point out the general nature of the powers of the living animal, and the relation they bear to each other, deduced not from the fanciful theories which have filled so many volumes, and added so little to our knowledge, but from an extensive and very laborious set of experiments, whose subjects are not such as bear a certain analogy to the functions of the living animal, but those functions themselves.

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* My paper on the Functions of the Nervous System, in the Philosophical Transactions for 1829.

From these experiments it appeared, that in the more perfect animal there are three distinct powers, each in their nature independent, though in their organs more or less directly dependent on the others, and more or less capable of being influenced by them.

Two of these powers, the sensorial and muscular, are evidently peculiar to the living animal. No other power bears such an analogy to them as can suggest a doubt on this subject; but the case is different with respect to the nervous power. Its effects bear too striking an analogy to many of the processes of inanimate nature not to suggest the idea, that the power which operates in both may be the same. Hence the best-informed physiologists have long looked to this power as that which probably forms the link which connects, if I may so speak, the animate and inanimate world.

In entering on the inquiry respecting the nature of the nervous power, I endeavoured, in the first place, by the aid of many experiments, to ascertain the line of distinction between the nervous and sensorial functions, that it might clearly appear what belonged to each power; for it is the sensorial power alone with the functions of which those of the nervous power can be confounded, the function of the muscular power being simple and well defined. In order to render the results the more certain, I endeavoured to ascertain this line by two different sets of experiments, conducted on different principles, the object of the one being to ascertain what functions remain after the sensorial power is withdrawn, and of the other, what functions fail on withdrawing the nervous power.

I was thus enabled accurately to ascertain what functions belong to the latter power*, and was, therefore, prepared for ascertaining experimentally, whether there be any power which operates in inanimate nature capable of all its functions; and in galvanism I found such a power.

It passes along the nerves in both directions. It impresses the sensorium; it excites the muscles; it effects the formation of the secreted fluids from the circulating blood; it preserves the structure of the various organs during the performance of their functions; and it causes an evolution of caloric from arterial blood: for these I had found to be the functions of the nervous power*, and as far as I myself and others well versed in such subjects could see, in the experiments referred to, it performed all these functions as perfectly as the nervous influence for which it had been substituted.

This seemed sufficiently to determine the point at issue, the nature of the nervous power. But it was a necessary inference, that if the nervous power be galvanism, it is not a power peculiar to the particular structure of the nerves, and ought therefore to be capable of existing elsewhere. After many vain attempts made with a view to cause the nervous power to leave the nerves, and give evidence of its presence elsewhere, this was at length effected by experiments, the accuracy of which has been ascertained by repetitions both in this country and on the continent.

This fact, in combination with the results of those experiments in which galvanism was found capable of all the functions of the nervous power, the most essential of which have also been repeated both at home and abroad, removes, as far as I am capable of judging, all doubt respecting the identity of the two powers. Thus the living animal body possesses the sensorial and muscular powers peculiar to itself, and the nervous power of which it partakes in common with inanimate nature.

In attempting to trace the laws of the two first, therefore, it is in vain to expect assistance from any other source. But if the view here taken of the nervous power be correct, it necessarily follows, that many analogies must exist between the effects of that power and the phenomena of inanimate nature, and that many of the inferences from the latter will be found more or less applicable to the former. The operations of the same power, however different the circumstances under which it operates, must always have much in common; and, in fact, we find that all the analogies which can be traced between the phenomena of the living animal and those of inanimate nature, belong to the phenomena of the nervous power.

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* My Inquiry into the Laws of the Vital Functions.

* See the statements in my first communication to the Editor of the Medical Gazette, the paper just referred to in the Philosophical Transactions for 1829, and the third edition of my Inquiry into the Laws of the Vital Functions.

Where shall we find in inanimate nature any thing analogous to the circulation of the blood, or any of the other effects of the muscular power, and still less can we look for any thing analogous to sensation, volition, or any of the other functions of the sensorial power. But when we turn to the phenomena of the nervous power, many analogies with certain phenomena of inanimate nature, force themselves on our attention; nay, there are none of its functions in which such analogy may not be traced; a circumstance from which alone the similarity of their origin might have been suggested.

The nervous power, in its impressions on the sensorium, in its excitement of the muscles, in all the chemical changes it effects in the formation of the secreted fluids, the preservation of the healthy structure of all parts of the body, and the maintenance of animal temperature, nay, in its very generation and mode of conveyance, bears a striking affinity to that world with which the living animal is in so many ways associated.

But such is the difference of the circumstances in which galvanism operates in the living animal and elsewhere, that, however striking the analogy, the results cannot be identical. We have seen it proved, that galvanism operating in the living animal under the same circumstances as the nervous power operates, produces the same effects; but it is only under such circumstances that its effects are, or, on any principle, can be expected to be the same; and any inference respecting the functions of the living animal from the phenomena of inanimate nature without regard to these circumstances, must necessarily be erroneous.

Of all the chemical powers with which we are acquainted, galvanism is the most energetic, and for this reason doubtless is employed by nature in certain functions of the living animal, where the chemical changes are at once the most rapid, the most varied, and the most complicated; but here, as in inanimate nature, its effects, as appears from what has just been said, are not confined to chemical changes; and whether chemical or not, they are influenced by, and some of them wholly, dependent on, its association with the more strictly vital powers.

Thus its chemical operations are such as no unassisted chemical agent can produce, and many of its other effects differ essentially from, and bear but a faint analogy to, any of those which belong to inanimate nature. It is the medium of the sympathy which exists in the different parts of the living animal body. It is the bond which connects them together, and thus forms them into a whole. It is through the nerves, for example, that an offensive body introduced into the trachea or the stomach excites the motions which expel it; that the obstructed skin excites the heart and bloodvessels by their increased action to remove the obstruction; and so on in a thousand other instances.

On carefully reviewing what has been said, we may form a pretty just estimate of the extent to which, and the modifications under which, chemistry is applicable to the functions of the living animal.

In the functions of the sensorial and muscular powers it has no place. Can it move a limb, or propel the blood, or perform the slightest act of the sensorium?

In part, and but part, of the nervous functions it has place. These, it will readily be admitted, are an important and extensive part of those functions; but even here its effects are essentially modified. The chemistry of the nervous power is not the chemistry of inanimate nature. We know that the changes are effected by the same chemical agent, because we have seen them all effected by that agent, but in the animal economy its effects are modified by the vital powers with which it is there associated.

The chemical effects of the nervous power are not only, in a manner analogous to its other effects, modified by the vital powers, but also in a way peculiar to themselves; which, even were the chemical laws of animate and inanimate nature the same, would greatly increase the difficulty of applying them to the functions of the living animal. In the processes of secretion and assimilation it is not only impossible for us to trace the steps by which the changes are effected, which compels us to admit the operation of certain causes peculiar to the living animal, the effects of which, consequently, we can never even hope to see exemplified in inanimate nature; but the results of the chemical processes of the living animal, unless the resulting substances be immediately removed from the sphere of action of the vital powers, are so constantly influenced by them, that, in a large proportion of instances, even were we acquainted with every chemical change which takes place, it would be impossible to say what change is effected at any particular moment of its complicated and ever varying processes; and this difficulty is still farther increased by our very attempts deranging the natural process, by necessarily introducing a set of causes which are foreign to it. If, for example, we wish to ascertain the changes effected on the blood by respiration, the very circumstance of removing it from the vessels, as I have found in repeated trials, unless our experiments be made while it is flowing from them, causes the most essential change in its chemical properties. Yet this is a favourable instance—we can obtain the blood in the state and at the moment in which we wish to examine it. In most of the assimilating and secreting processes, it is impossible for us to determine the moment at which any particular change takes place; and if we could determine it, by what means shall we, at that moment, submit the subjects of these processes to our tests?

When, to the whole of these circumstances, we add, as I pointed out at length in a late communication, that the chemical changes take place chiefly in the fluids of the body, whereas it is chiefly on the state of the solids that the great changes of the animal system depend, that of the fluids constantly varying as their state varies, we readily see how limited must be the application of mere chemical principles either to elucidate the functions of the animal body in health or restore them when they have deviated from it.

Such is the view of the subject which our knowledge of the animal economy, deduced from observations and experiments made on the healthy animal, affords; and this view is amply confirmed by all our experience of its diseases. Let us take our illustrations from those with which we are most familiar.

Has not the chemical composition of the bile been long known to us? Has this knowledge assisted us in correcting its derangements? If we could exactly ascertain the nature of its derangement in each particular case, and could find a chemical agent capable of correcting it, would it afford any permanent relief while the diseased action of the liver continues? On the other hand, if we look to the means which experience has taught us are most effectual in correcting diseased states of the bile, do we find any particular relation between these means and the morbid states of the bile corrected by them? They do not act by correcting the morbid states of this fluid, but by improving the function of the organ which secretes it.

The vitiated state of the bile already formed, no doubt occasions certain symptoms which aggravate the evil. In its passage, it irritates the surface of the alimentary canal, and as far as its morbid properties can be corrected, this irritation will be allayed; but being once discharged from the liver, it has no more to do with the morbid action on which its vitiated state depends, than any other equally irritating substance introduced into the same passages would have; and while it is being formed, as its vitiated state is the consequence, not the cause, of the disease which has deranged it, its correction, even at the moment of its secretion, were this possible, could not correct that disease.

Let us take another familiar instance, which is the more in point because the imperfect knowledge of our predecessors respecting the laws of the animal economy actually led them to practise on the mere principle of the chemical properties of the remedy. Putrid fever takes its name from the putrid state of the various secreted fluids; and such was their confined views of the animal economy, that they thought, if the putrescent state of the fluids could be corrected, nothing more would be required for the cure of the disease; thus mistaking a consequence of the disease, the necessary effect of the failing powers of life, for the disease itself.

The trial was fairly made, for at one time no physician would have been considered as doing his duty if he had, in such a case, omitted the use of antiseptics; and what has been the result? that any physician who should now place reliance on them, although the labours of the chemist have supplied us with such as are far more efficacious than any possessed by our predecessors, would only expose himself to the animadversions of his better-informed associates. It would be superfluous to pursue the subject farther, as I have already had occasion to observe the tendency of such views is to bring us back to the humoral pathology—a pathology equally narrow and mistaken, which could not for a moment have existed except in the very infancy of our science.

Are we, then, it may be said, to regard chemistry as wholly inefficient in tracing the laws of the living animal body, and enabling us to cure its diseases? Such a question could only have been suggested by the exaggerated claims which have been made in its favour. Its legitimate claims in these departments of knowledge, notwithstanding all the deductions which have been stated, are great. They are sufficient to satisfy the most ardent admirer of a science which has left so few branches of knowledge unindebted to it. Let us take a rapid view of these claims, and first as respects physiology.

Although chemistry cannot, and, for the reasons which have been mentioned, never will, enable us to trace all the changes of composition which take place in the living animal body, physiology is in many respects indebted to it. It can tell us many of the changes of composition which take place in the living animal, and all those which the living animal effects in the external bodies influenced by it. It can expain, for example, certain changes in the blood effected by respiration, although it cannot explain all these changes, and of all the changes effected by respiration on the air, it can give us a perfect knowledge. Thus we owe to chemistry all the knowledge we possess respecting one class of the effects of this important function.

Similar observations apply to the function of perspiration, which, like respiration, both influences the composition of, and, under different circumstances, variously impregnates, the air.

In like manner chemistry, although incapable alone of explaining any stage of the process of digestion, can determine many of the changes which the food undergoes in that process. Although we have reason to believe that the chyle, when taken from its vessels, is not in all respects the same as that which moves in them, chemistry can, by enabling us to compare its composition with that of the food, determine many of the changes effected in the latter.

To chemistry we also owe our knowledge of many of the secreted fluids. These are separated from the living powers by being discharged externally, or into the various cavities of the body, and are therefore no longer subject to their influence; and their composition throws much light on the nature of the general, if not on the detail of the particular, processes by which they are formed. It tends also to throw no small degree of light on many of the functions of the animal body, that chemistry has unfolded to us the changes which would take place in the substances of which it is composed, were they not counteracted by the vital powers of that body. Many of the changes which take place in disease are of this nature, for as these powers fail, the laws of inanimate nature begin to prevail over them, and, before the patient expires, the latter have often in a great degree established their dominion, producing symptoms which cannot be explained without a recurrence to those laws. This leads us to the second part of the subject.

If the science of the vital functions in a state of health has been indebted to chemistry, that of their diseased states has been infinitely more so; not only as a means of occasionally explaining a certain class of the phenomena which attend and modify these states, and thus occasionally suggesting means of relief; but in giving the most efficacious and convenient form to a large proportion of these means. Respecting the first part of the subject, we cannot have a better illustration than Dr. Prout has afforded in his investigations respecting urinary diseases. These diseases, I have already had occasion to observe, are particularly favourable to the labours of the chemist, because many of their most distressing symptoms depend on the state of the urine which is so long retained after its secretion, forming no longer any part of the living animal, and acting in the same way and being subject to the same changes as would be the case with respect to any similar fluid introduced from without.

In cases of other deranged secretions, and those of morbid extravasations and growths, we have derived much information from the labours of the chemist; and those acquainted with the progress of knowledge will not undervalue such information, because, in many instances, it has not yet led to practical results.

When we turn to the improvement of the medicinal part of our means of cure, we owe every thing to chemistry. On this subject it would be superfluous to make many observations: it is the peculiar province of the chemist. In the various pharmacopœias, and the works published with a view to illustrate their processes, we see how much we are indebted to him. To the improvement of this important branch of knowledge chemists of the highest character have lent their aid. To the French chemists we are particularly indebted, and not less to some of our own countrymen, who have devoted to it a large proportion of their time and labour. We have been supplied with many new means of relief, and many of our former means have been rendered both more congenial and more efficacious.

ASTHMA, &c.

The Case of Mr. George Hobson, of Great Marylebone Street, drawn out by himself.

Mr. Hobson, aged 60 years, a native of the North Riding of Yorkshire, where he resided till he was 21, was subject, from early youth, to violent attacks of asthma, which were almost regularly periodical, occurring once in six or eight weeks. They varied as little in their frequency as in their violence, and each fit continued six or seven days, during which he could never lie down nor breathe unless his legs were in a depending position, nor sleep except with his head on a table placed before him; after this, easy and copious expectoration came on, which, continuing for three or four days, completely relieved him from every embarrassment.* He continued thus dreadfully to suffer from the age of seven to twenty-one, when he came to London, and remained upwards of eight months, during which period he never experienced even difficulty of breathing; and though he had, from frequent attacks of cold, violent coughing and its other usual attendants, his asthma did not return. He revisited the north, but before he reached home was attacked with his old enemy, which continued for four months, without any complete interval of ease, and the paroxysms were more violent than formerly. He then determined upon returning to town, where, being of the medical profession, he settled, and has since remained, without ever being deprived of a night's rest from asthma, though he has had, in severe colds, more than ordinary difficulty in respiration. In three or four years after the asthma left him, he became subject to irregular action of the heart, and palpitation, attended with great flatulence and acidity of stomach, and he fancied himself relieved by alkaline medicines, and particularly by the carbonate of ammonia, from its cordial effects in large doses. The attacks were at first of short duration, but soon became more serious and frequent, and have continued without intermission for eleven or twelve hours, attended with a violent and indescribable pain along the whole course of the spinal column and under each scapula, and difficult and painful respiration; the whole of which deprived him, at the time, of the power of walking without support. He can only compare his feelings to what would be the result of an immense weight placed upon each shoulder, dragging them and the scapulæ down. These symptoms were attended and followed, for some time, with a sighing laborious respiration, much increased upon the slightest exertion, and a sense of numbness and pain extending down both arms, particularly the left. The digestive functions were at times a good deal disturbed; the secretions of the liver and the bowels much depraved; bowels regular; motions dark and offensive; appetite very good; sometimes, but not often, vomiting after meals; his general rest at night but short, and disturbed. The period during which these attacks were absent was very irregular; sometimes as many as four or five times in one week, at other times he had not one in two months. Whenever dyspepsia attended, the usual remedies have always failed to cure it. The affection of the heart, however, came on in the intervals, and just as much when the nutritive functions were well as when they were imperfect; and he was at all times pale, debilitated, and bloated, with œdema of the feet and legs, extending to the knees, and moved about with difficulty; and such was his state of health, that, for four years past, he has never left home without having his name and address in his hat or a card in his pocket, being doubtful if he would ever return alive. It was soon after his arrival in town, about thirty-four years ago, he became the subject of irregular action of the heart; this, at first, was but slightly noticed, but soon became of a more pressing and alarming nature; the seizure would come on without any sign or note of its approach, when either sitting, lying in bed, or in active exercise; four or five of the heart's pulsations could not be felt; the fifth would be an irregular bound against the parietes of the chest; there was no distinct or regular interval observable in the pulse — its rythma would be nearly continuous against the finger; there was an oppressive constriction across the chest, coldness of the extremities, numbness and pain in the arms, as before noticed; the face expressive of intense anxiety; nose and lips cold, and of a blue colour; great restlessness, and inability to remain in one situation; frequent windy evacuations, without any apparent relief. The only thing which appeared to afford ease was the employment of strong friction along the course of the spine. These phenomena lasted indifferently for two, six, or twelve hours (never so violent, however, when so long as the latter period), when, with a sudden and very forcible and painful impetus against the ribs, the heart would be righted, and its action, with that of the pulse, would at once become regular. From the long continuance of these attacks, and frequent recurrence of irregular action of the heart, he was led to believe the existence of some structural disease in or about the heart; in which he was confirmed by the opinion of medical friends, many of whom he consulted, and by his father having suffered in the same way and died very suddenly.

Dr. Wilson Philip was at this time (about a year ago) consulted; who afforded him great consolation by assuring him that there were good grounds to hope that, in a great measure, his fears were unfounded, and, as many of these appalling symptoms were evidently sympathetic, there was reason to hope that the others might be of the same nature. Dr. P. ordered him five grains of the blue pill for three alternate nights, with an aperient draught on the following morning. The alimentary canal was thus cleared, and Mr. H. by his advice, commenced taking half a grain of blue pill with three of Extr. Hyoscyamus, three times a-day, with stomachic draughts, and other occasional treatment; which he continued for many months, and which, he believes, has restored him. At first the relief was less perfect, and only occasional; by degrees the intervals of relief became more perfect, and of longer duration, and it is now four months since he has had any attack; while his looks, feelings, and general health, are in every way so much improved as to surprise those who have known him for the greater part of his life. His œdematous swelling and bloating have disappeared, and he has regained both the appearance and habits of health.

October 10, 1831.

OVARIAN DISEASE.

To the Editor of the London Medical Gazette.

Sir,

Mrs. Bottrell, æt. 24, of short stature and light complexion, married four years. Has had no children, nor abortions; until a year and a half ago enjoyed good health. At that time was attacked with inflammation in the left iliac region. In a few days it was so far subdued that nothing but a slight pain and uneasiness remained. In a few weeks the abdomen was observed to be enlarged, and continued to enlarge for four months, when fluctuation was discovered. The operation of tapping was resorted to, and was repeated four times previously to her being under my care. What remedial means had been employed, I am unacquainted with. After the second operation the abdomen was noticed to be still large, and on examination a tumor was detected in the pelvic region. I was called upon to perform tapping on February 24th, 1831. About four gallons of limpid fluid were removed. After the operation the lower part of the abdomen remained prominent, and the parietes were distended; it had an even surface, and conveyed to the touch a doughy feel; no tenderness or pain on pressure; the body was much emaciated; tongue furred; appetite bad;

bowels torpid, and secretions dark-coloured; urine pale for a few days after tapping, but became thick and albuminous; pulse 120, small and thready; skin dry and hot; sleep disturbed; shooting pains along the course of the large nerves of the thigh.

Purgatives, mercury, and the various diuretic medicines, were exhibited. The Tr. Iodinæ was given for a few weeks, commencing with five drops every four hours, and increased to five and twenty. The abdomen was rubbed twice a-day with an ointment composed of Ung. Hydrarg. and Camphor. Digitalis was given to diminish the frequency of the pulse which it did effectually. The general health became improved, but without any diminution of the tumor, and only slight retardation of the dropsical accumulation. The Ung. Hydriod. Potassæ was rubbed into the abdomen for the space of half an hour twice a-day, regularly. The circumference of the abdomen after tapping was found to have diminished three inches and a half in eight weeks from the commencement of the Iodine friction. Paracentesis abdominis was repeated every three weeks or a month. The fluid varied in colour each time; sometimes it was limpid and watery, at others thick and opaque, and containing in it flakes of lymph and pus. At one time about a quart of uncoagulated blood followed the withdrawal of the trocar. On the 5th of September the last operation was performed. Seven pints of fluid more than previously, were removed. The day following she was up, and would, injudiciously, be assisted into the yard; she sat there for half an hour, but on account of faintness was obliged to return. From that time complained of soreness in the abdomen, not tenderness on pressure, nor positive pain; she said it was an indescribable uneasiness. Peritonitis being suspected, the various antiphlogistic measures were promptly adopted, but without affording any relief. On the 19th she died*.

Ovarian disease is universally allowed to be one of those direful maladies which have hitherto baffled every attempt at their removal by medicine. Happily this disease is not of very frequent occurrence; and to this may be fairly attributed our ignorance of any true diagnostic marks by which it can be recognized in its early stage. On reviewing the preceding case, many circumstances present themselves importantly interesting. We observe that this disease occurred in a *young* woman, who, though married, had never borne children; that it commenced with inflammation in the left iliac region; that in *four months* the abdomen was filled with fluid; and that the tumor was at this time so diminutive, that its existence was not even suspected. But one of the most remarkable features was the rapidity of the growth of the tumor, it not being cognizable immediately after the first tapping, yet in six weeks being as large as a child's head.

Had the tumor been discovered when the size of an orange, or even *twice* that size, would extirpation of it have been practicable?

The question is answered by that philosophic physician, Dr. Blundell, in his Physiological Researches; where he has proved very satisfactorily the groundlessness of the fears entertained with respect to wounding the peritoneum, and has also shewn the facility with which diseased ovary, in its *early stage*, may be removed. *In this case it is obvious that extirpation could have been successful only in the very earliest period, in consequence of the early formation of the dropsical sac; yet I believe no one would have apprehended that such a structure did at the time exist.* Even at the period when I first saw the patient, and also during the whole of my attendance, I did not doubt that the dropsical fluid was contained in the peritoneal bag. Dissection revealed that it was not; but that it was contained in a new structure, the 'sac;' which sac must have been formed either prior to the enlargement of the substance of the ovary, or have increased in a much greater ratio, so as to have lined nearly the whole of the abdominal cavity—to have become filled with fluid, and to have existed so for some weeks before the tumor was discovered.

We observe the inefficacy of the most potent medicines in this disease; and it affords further corroboration of the veracity of the conclusion, that on such morbid structures medicine has no influence. We therefore arrive at the inference, that extirpation of diseased ovary, in *the early period of its growth*, is the only curative measure we are yet acquainted with.

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* We omit the postmortem examination, as the appearances were not unusual, and as we are much pressed for space.

Ought paracentesis abdominis to be performed in ovarian dropsy, when the fluid does not amount to more than *thirty-two* pints? What advantage can accrue from such early tapping? Will it prevent a future accumulation?

Should any urgent symptom arise, such as violent dyspnœa, then the operation of tapping might be demanded; but it does not appear that in this case any unpleasant symptom was manifested;—she was tapped because the abdomen was filled. It is clear that no permanent benefit can be obtained by early tapping; for, while the cyst remains, fluid will be again secreted, unless the sides of the cyst could be retained in coaptation, and inflammation induced, so as to obliterate the cavity; but it would be difficult to effect this, owing to a certain quantity of fluid which must necessarily remain and form a nidus.

With respect to the tumor there can be no doubt that it was a disease of the ovary; but under what genus it can be classified, it would be difficult to say. It is neither a scirrhous, nor an hydatid, nor yet a common encysted tumor; but a *combination* of the three structures. The cells could not all be the enlarged vesicles of De Graaf, because these are not more than sixteen or twenty in number; whereas some hundred cysts were discovered, many of which had all the characters of hydatids; it is probable that the 'vesicles' were implicated. The envelope of the tumor appears to have been the *tunica propria* of the ovary; and the 'sac' an extension of the upper border of that tunic, which had become separated from the contained parts.

These brief remarks may enable the reader to comprehend more clearly the nature of the case.

I am, sir,
Your obedient servant,
W. Dobson, Surgeon.

9, Belgrave-Street South, Pimlico,
Oct. 5, 1831.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Observations on Cholera, as it appeared at Port-Glasgow, during the months of July and August 1831. *Illustrated by numerous Cases.* By John Marshall, M.D.

Dr. Marshall, of Port-Glasgow, is the gentleman who created a somewhat unnecessary alarm some months ago by reporting to government the occurrence of cholera in the town where he resides. It soon appeared, however, to use a vulgar expression, that he had "found a mare's nest," and some degree of ridicule, naturally enough, followed this discovery; in fact, we imagine from the contents of the brochure before us, that the Doctor's cases had become a standing joke in the north. Instead of letting the matter drop quietly and be forgotten, Dr. Marshall resolved to print the cases, "and shame the rogues." This proceeding, we think, was injudicious; for, in our opinion, he has thus condemned himself out of his mouth. Twenty-four cases of vomiting, purging, with cramps, &c. are related, such as every man occasionally meets with in the hot months: of these only one died, and he had been drinking some days before at an Irish wedding, and at the onset of his attack, before Dr. Marshall was called in, had had a mixture, containing six grains of tartar emetic. The Doctor does not admit that he had taken much of this, but adds, "Let us, for the sake of argument, allow that the whole was swallowed at one draught, could these six grains of emetic tartar have produced the morbid symptoms (namely, excessive depression, vomiting, purging, &c.) which I witnessed when called to him nineteen hours after?" The practitioner who can gravely ask such a question, must be wonderfully ignorant, or have his ideas strangely perverted by prejudice.

We shall quote one of Dr. Marshall's cases, selecting one which was severe, but the account of which is conveniently brief:—

"July 11, 1831.—James M'Lachlan, ætat. 25, labourer in a sugar-house, very stout made, enjoys excellent health, was seized during the course of last night with deadly sickness, vomiting, and purging of dark matters, which speedily changed to colourless fluids, excruciating pain in the bowels, spasms, by which the abdominal muscles seemed drawn in till they rested on the spine; excessive debility; surface of body cold; profuse cold perspirations. Reaction took place in eight hours after first attack; was completely established in twelve. The patient convalesced rapidly, and returned to his work on the 15th.

"Had sat with John Murray during the night of the 3d July; knows of no other cause for his illness; has not partaken of fruit, vegetables, or 'new potatoes,' during the present season."—*Ex uno disce omnes.*

Illustrations of Surgical Anatomy, with Explanatory References; founded on the work of M. Blandin. By John G. M. Burt, Surgeon to the City Dispensary, Extraordinary Member of the Royal Medical Society, &c. 4to. Edinburgh.

These illustrations of surgical anatomy are founded on the justly-esteemed work of M. Blandin, on whose plan, however, the editor has improved. In the fasciculus which lies before us there are two plates, the first representing the superficial and the second the deep-seated anatomy of the neck. They are beautifully executed, and if those which follow equal them in this respect, the work will prove highly creditable to Mr. Burt, and extremely useful to the practical surgeon as well as to the student of anatomy.

Pharmacopœia Medico-Chirurgica in usum Medicinam Facientium, à Roberto G. Holland, Chirurgo, Concinnata.

This volume contains a certain number of lists of medicines, tables of synonyms, formulæ, &c. perhaps as good, certainly no better, than many others previously published.

Reports of Medical Cases. By Dr. Bright. Price 9*l.* 9*s.*

[Continued from page 57.]

Inflammation of the Substance of the Brain.

The next subject which attracts our attention, in the regular order of the volume, is inflammation of the substance of the brain.

A healthy man, nearly sixty years of age, fell, and slightly scraped the side of the left temple. He found it very painful, used common remedies, and in a day or two had forgotten the accident, and returned to his usual avocations. Three weeks after he went upon a shooting excursion, took much exercise, got wet, eat heartily, and stept into a coach to return to town. During the journey pain in the head came on, and by the time he arrived at home he was labouring under a smart attack of fever, with pain spreading from the eyebrow towards the vertex, and occasionally down the back. He was bled generally and locally, and had appropriate remedies to allay the febrile disturbance. Next day was much the same, perspired freely, and became chilly on the least exposure; his manner was hurried; in fact, he was slightly delirious. There was puffiness above the eyebrow, and a tumor in the socket below the eye, from which last some discharge of pus took place at night, with great relief. On the following day, the symptoms continuing, he was seen by Mr. Brodie, who cut down upon the bone just above the superciliary ridge; it was rough; about "three drops" of pus came away, and the patient was "in heaven." He went on for some time without any very decided change, afterwards became rather childish, being, however, at intervals, more rational. For several days he got up regularly for a few hours, though his pulse was extremely weak. He was now seized with an erysipelatous attack of the face and head, which carried him off in a week.

"*Sectio Cadaveris.*—Some serous effusion in the cellular membrane of the scalp. The bone was denuded only to a small extent, about the size of a sixpenny piece, where the second opening was made; but the surrounding bone, more particularly between this opening and that which had been made upon the superciliary ridge, was unusually brittle, splintering in various directions under the saw and the chisel. The pericranium just round the denuded bone was thickened. The dura mater was not detached, but a deposit of organized membrane, nearly as thick as the dura mater, but more flocculent, had taken place between it and the bone in that part, over an extent of the size of a crown piece; and a mark remained upon the bone where the deposit had chiefly been, of an irregular form, with white specks as if from adventitious matter deposited in the pores of the bone. When the dura mater was raised,

> its internal surface shewed also a flocculent deposit, and the whole surface of the brain exhibited the arachnoid raised by serous effusion, so as to conceal the convolutions; the arachnoid was slightly opake; but on the edge of the anterior lobe of the left hemisphere, close to the falx, was an ulcer of the size of half a hazel-nut, oval, depressed, and containing a small quantity of turbid white fluid, of puriform character; and when this was removed with a sponge, a clear depression became visible, as if a piece of the brain had been cut out. The brain itself did not appear unhealthy, but the blood was rather fluid in the vessels divided in the cerebrum, giving bloody points. In the ventricles was at least double the ordinary quantity of serum, and that rather turbid. The choroid plexus was natural, nor was any other obvious deviation from health to be discovered."

Although the above be adduced as a case of ulceration on the surface of the brain, Dr. Bright is not disposed to argue that the chief symptoms depended upon this.

A man of colour, between fifty and sixty, was admitted at Guy's with severe dyspnœa, and dropsy. He had been in the hospital before, for the above symptoms, but had been sent away "on account of the noise and disturbance he made during the night." He was now in a drowsy state, capable of being roused, but not so as to give any satisfactory account of himself. He was not considered as being in his sound mind, and at night became noisy. About four days before his death he was observed to be sinking, but his pulse always had a peculiar sharpness. There were some appearances of slight paralysis.

> "*Sectio Cadaveris.*—On removing the dura mater, a thin layer of effused blood of several inches in extent—in some parts as thin as paper, in others in flat masses as thick as a shilling—was discovered on the posterior part of the left hemisphere, and on the anterior part of the right; this was not effused so as to be smeared over, or to discolour the arachnoid, but remained attached to the dura mater, and could be peeled off as if the thin clots of which it was composed were contained within a membrane; but whether this was only a fibrinous envelope of the clot, or an actual membrane, was doubtful. I inclined to the latter opinion. On examining the base of the brain, both of the anterior lobes were found to be eaten by superficial ulceration in three or four irregular patches of the size of a shilling, which were yellow and excavated to the depth of the eighth of an inch, though the yellow soft matter could scarcely be considered as true pus. The vessels of the base were not perfectly healthy, but in different parts contained cartilaginous deposits. On the surface of the medulla oblongata was a curious black carbonaceous deposit, following somewhat in the course of vessels, which, judging from some other cases, I believe to be the remains of a former effusion of blood under the arachnoid, or upon its surface.
>
> "The left lung was partially adherent to the pleura, containing much grey matter, natural in structure, but loaded with serous effusion. The bronchial tubes of the deepest red colour, and much dilated towards their extremities. The right lung much more generally adherent, and extensively hardened, from old and chronic inflammation; so that scarcely any part performed its functions; the upper lobe, in particular, was a hard, tough, grey mass, and the bronchi were still more diseased than in the left lung."

In this case there were sufficient signs of some derangement of the brain, but this was conceived to depend chiefly on the difficulty of transmitting the blood through the lungs.

Cases follow in which encysted abscesses were found in various parts of the brain; in some of them, from external injury, in others from spontaneous causes: but as these cases do not appear to us to present so much interest, either in a pathological or practical point of view, as most of the others, we shall pass on to

Cases illustrative of softening of the Brain;

that peculiar change of structure which has been of late years described by some of the most distinguished among the French pathologists, as well as by some among ourselves. Dr. Bright is of opinion that "softening" of the brain may exist in various forms, may originate in various causes, and may be marked by different symptoms. The symptoms, however, which generally first attract attention, are those of pressure: for the most part they come on suddenly, and they vary in their progress and duration. The pressure, however, seems to be but a secondary part of the diseased condition—a consequence of some change which has gone before it; while it throws but little light on the nature of the pathological condition.

The first case detailed (LXXXI.) is one in which there was sudden hemiplegia, all power of voluntary motion and all sensation being lost both in the upper and lower extremity. The face was only slightly affected, but the speech was exceedingly imperfect. Pulse 68, weak. This man had married a woman much younger than himself a few years before. He had also been subject to an open ulcer on the leg for several years, but which had healed up about six or eight months before. He then became subject to attacks of diarrhœa. He had never complained of his head. He was admitted August 5, and died on the 21st of the same month. Post-mortem examination discovered softening of the middle and posterior lobes of the left hemisphere of the cerebrum, with limpid effusion beneath the arachnoid and between the convolutions.

A man, aged 30, was admitted at Guy's Hospital, April 7th, with imperfect hemiplegia and coma. It appeared that he had complained of the left side of his head for three or four months, and just four weeks before admission he had been seized with very severe headache, which had continued ever since. The loss of power came on gradually. On the night between the 5th and 6th he became worse, and passed his fæces involuntarily, losing his speech at the same time. He lingered till the 21st. Softening was found of the anterior lobe of the left hemisphere. The arteries in the vicinity of the fissura Sylvii were indurated and cartilaginous, and the circle of Willis was studded with bony deposits.

A young man, aged 28, was admitted October 4, 1827, with partial paralysis, marked by slight deformity of the face, and feeble, unsteady action of the arms and legs. Had from infancy been afflicted with headaches, generally referred to the forehead, with occasional giddiness. The symptoms increased about three years ago, and about eighteen months since had a fit, in which he lost his senses for a few minutes. There was a very gradual, but decided change, from this time, indicative of chronic mischief in the brain. He died on the 12th of November. Great vascularity of the pia mater was found, with softening of the left hemisphere in two places.

A gentleman, aged 73, was suddenly reduced from comparative affluence to penury by the knavery of a partner, who absconded. On receiving the information, he fell down in an apoplectic seizure. After a time he recovered from this attack, and was able to write for one of the weekly journals, although his memory was defective. In ten months he had a second fit nearly like the first: after this he never was able to engage in any occupation, and about three weeks before the date of the report, he had a third fit, for which he was copiously bled, and had some aperient medicine. After this he was at times tolerably sensible, but in general lay in a comatose state, and appeared to have lost all power in the right hand as well as in both legs. He continued much the same from the 24th of June to the 3d of February following, when he became more conscious, and had more power over his limbs. This improvement lasted three or four days, when he sunk into a comatose state, and died on the 9th. On examination after death, considerable cerebral congestion was found, with partial softening, especially of the fornix and septum lucidum. There was a good deal of fluid about the brain. Dr. Bright observes of this case, that it was probably one of those in which no considerable vessel is ruptured, the apoplectic fit rather depending upon a general state of congestion, producing slight laceration of the fine vessels, chiefly at the union of the cineritious with the medullary matter.

A tall, athletic man, 40 years of age, was admitted at Guy's, under the care of the author, January 21, 1829. He had been long in the army—had received many wounds, and had "scars to shew." He laboured under great depression of body and mind; his left arm and leg were weakened; his gait unsteady; and he squinted. "His manner was peculiar, and his conversation quite incoherent; he stood for some minutes together in one position, as if lost in abstraction; when spoken to, he answered a question with some consideration apparently correctly, but then at once ran on to something else, without however saying much, but making some statement perfectly without foundation; as, for instance, saying that he was bled this morning, that the blood flowed well, and did him a great deal of good;—all which was most distant from the fact." His illness had been coming on about a year, and the first indication of it was his falling down in the street, and losing his recollection for a moment. This happened repeatedly, but was equally transient as at first. He had, however, constant pain in the forehead, and for the last six weeks had passed his urine, and sometimes even his fæces, in bed. He now had leeches applied, a seton was introduced into the neck, and he was purged. Afterwards he took a grain of sulphate of zinc three times a day, and the quantity gradually increased to gr. v. ter die. Carbonate of iron, sulphate of quina, and various other means, were tried without avail. July 10th he is thus described:—" His general manner and aspect quite unchanged; he stands about musing; and if you ask him how he finds himself, answers, after a few seconds of consideration, 'I am not quite dead;' says he has hurt his thigh, and that there are wounds in two places; but on examination there are only two old scars from wounds he had received in service; he complains of weakness of the whole left side, but then says he feels rather better."

He died on the 23d. The skull shewed a slight irregularity, with depression of half the size of a sixpence, with a minute point of bone running towards the brain: this was at the site of a wound. The chief morbid appearance was in the corpus striatum of the right side, which was quite flattened, and of a yellowish colour: it felt quite soft, and on making a section was found to be completely broken down, having a "filamentous and watery brown appearance."

The author, in commenting on the cases of which we have above given a brief outline, observes,—

" Thus we perceive that partial softening of the brain probably owes its origin to a variety of causes, and to a considerable range of morbid action; and we may therefore conclude, that however much the symptoms of the confirmed disorganization may approach in all cases, and may generally evince proof of pressure, and of interrupted nervous susceptibility and action, yet that the symptoms which mark its progress and approach will vary greatly; and this conclusion will be borne out by a comparison of the symptoms in the cases to which I have had occasion to refer. Although it is possible that inflammation may exist without being attended by very striking symptoms, yet I am inclined to make a threefold division of cases in which a softened condition of portions of the brain exists. First, where from obstructed circulation the part undergoes a change analogous to gangrene; and this is the more genuine form of the disease (Case LXXXI. LXXXII. LXXXIII.) Secondly, where from congestion the substance of the brain suffers a more or less complete laceration (Case XXIV. LXXXIV.); and under this division might be classed all those cases in which the disorganization is secondary, and to a certain degree mechanically dependent on the presence of tumors, or effused serous fluid, or apoplectic clots (Case LXV.) And thirdly, where softening is produced by inflammatory action (Case LXXVIII. LXXIX. LXXX.)

" It must however be remarked, that in these observations I speak only of partial softening with disorganization of the brain; not including those cases in which the whole substance of the brain is found unusually soft, a condition which often occurs when the body has been greatly debilitated and worn out by disease, but seems not to interfere with the structural integrity of the organ.

" Dr. Baillie, in his incomparable manual of morbid anatomy, by which the attention of modern physicians was chiefly drawn to the study of pathology, speaks of the softening of the brain as it is described more particularly by Dr. John Hunter, in the Gulstonian Lectures read before the College of Physicians in 1796; and he refers to its occurrence under two forms—in advanced age, and combined with apoplectic effusion; though, by a remark he afterwards makes when speaking of the effusion of blood into the brain, it is doubtful whether he considered it in the latter case a disease existing previously to the rupture of the vessels, or a consequence of the mischief. The fact appears to be, that it occurs in apoplexy under both of these circumstances, and cases illustrative of both will be found recorded even in the present volume.

" With regard to the treatment of cases in which we are induced to suspect that the softened disorganization is taking place, it will of course vary according to the cause; as long as we are obliged to infer excessive action we must deplete, and use counter irritation; but looking to the general condition of those who are the subjects of such disorganization, we should be inclined to prohibit active depleting remedies, as likely to diminish the powers of the system. Nor can we suppose that the mercurial action would produce any good effect; on the contrary, whatever is calculated to disturb the more healthy and natural actions, may be expected to do harm, and probably the careful avoidance of every thing which can over-excite either the body or the mind, with the employment of gentle tonic remedies, both as medicine and as diet, and even as occupation and amusement, will be most effectual in delaying the mischief, or in supporting the frame under its gradual decline. To what period life may be prolonged, when a considerable extent of the substance of the brain is diseased, we have not the means of asserting; but that large portions of the brain may be lost with apparent impunity has been proved in many instances; and in some of the foregoing cases there is reason to believe that the mischief had existed several months, attended, however, with great functional derangement. Whether healthy brain be ever regenerated is very doubtful; but as to the power of nature of repairing in some degree the injuries of the brain, there can be no doubt; and there is reason to believe, that parts of the substance which have been lacerated by blows, or by apoplexy, and thus rendered useless, are frequently absorbed; but how far the powers of the system are sufficient for such an operation, where, as in the cases of Kennedy or Murridge, spontaneous change has taken place, remains a matter of speculative opinion; at all events, one of the best objects we can propose in the treatment, is to give force and vigor to those natural actions by which such reparations are effected."

PRESSURE.

The division of this part of the subject adopted by our author, is, first, into those cases which illustrate the effects of mere vascular turgescence; 2dly, those in which the symptoms appear to have been in some degree, at least, the result of effused serum; 3dly, cases in which there has been rupture of vessel, with extravasated blood; and 4thly, those in which the pressure has resulted from some organic change.

Cases illustrative of the effects of Cerebral Pressure from Vascular Turgescence.

When this state comes on suddenly, it is productive of vertigo, loss of consciousness and voluntary power, and not unfrequently of convulsions; apoplexy from congestion, and certain forms of epilepsy passing imperceptibly into each other. When the congestion takes place more gradually as from the effect of narcotics, or habitual indulgence in fermented liquors, or arising in the progress of disease, it is indicated by increasing listlessness, drowsiness, and finally, by coma; temporary numbness, or loss of sensation, or convulsions, being experienced towards the termination of the disease.

The following case is a good illustration of the mingled attack of apoplexy and epilepsy:—

> "James Eames, aged 60, was admitted into Guy's Hospital under my care, September 10th, 1829, in a state of great feebleness, with occasional vertigo, from an attack of a mixed apoplectic and epileptic character, which he had experienced a few days previously, and for which he had been cupped; he was a stout-made man, with a bald head and short neck, exsanguine in his appearance, I believe from the frequent necessity of losing blood. I had seen him before, and was no stranger to his history. Six years previously, while he was walking, he was first seized with giddiness, and on the following day experienced a more severe attack, with loss of sight and of consciousness. He was twice bled from the arm, was freely cupped, had cold applications to the head, and gradually recovered, not having suffered during the whole attack any paralysis. Three years and a half afterwards he had a similar seizure, and again about a year after that; subsequently to which he was in the hospital, under the care of Dr. Cholmeley; and while still in the house about a year ago, he had a more severe attack, greatly resembling apoplexy, in which he remained perfectly senseless for several hours, but this was attended with spasmodic action of all the extremities. He was cupped, and recovered pretty speedily, but remained feeble for several weeks; and about six months afterwards had another similar attack. As he was free from the immediate severity of the fit at the time of his being received under my care, I had recourse to gentle purging, and afterwards to tonics, and on one occasion was obliged to cup him at the neck; under this plan of treatment he gained strength rapidly, and left the house in about a month."

In cases of this nature, which are by no means uncommon, the chief point to be decided is, the extent to which depletion ought to be carried, and Dr. Bright observes, that in most instances, in proportion as the epileptic character preponderates, the propriety of large depletions will become more questionable. Indeed, the abstraction of blood can only be regarded as a temporary means, calculated to remove present danger, which having been effected, keeping up regular action of the bowels, with tonic regimen, is best calculated to guard against relapse.

Next in succession we find a set of instructive cases, shewing the tendency of narcotic poisons to produce congestion in the brain. In the following case, which proved fatal, the state of the cerebral vessels was matter of demonstration:—

Poison by Opium—Great Vascular Congestion of the Brain.

"— Williams, a young woman, was brought to Guy's Hospital January 21st, 1825, in a state of profound coma, from the effects of opium, taken about two hours before her admission, with a view to self-destruction. The quantity she had swallowed was not exactly known. The stomach-pump was freely used, both to wash out the stomach and to administer stimulants; but sensibility never returned, nor did she ever so far recover as to be able to swallow, and she died about fifteen hours after admission.

"*Sectio Cadaveris.*—Vessels of the head unusually turgid throughout, and on the surface of the anterior part of the left hemisphere of the cerebrum, an ecchymosis was observed of a very light colour, about the size of a crown-piece—such as a very few drops of effused blood might have produced. The bloody points were very numerous in the cut surface of the brain: there was no serum collected in the ventricles. The stomach was quite healthy, except two or three spots of ecchymosis, which were ascribed to the tube of the stomach-pump; there was on the whole less vascularity than usual. In the large intestines were some appearances of inflammatory action. A small ovarian tumor, about the size of an orange, was found in the pelvis."

But all cases come not to this unhappy end, even where large quantities of laudanum have been swallowed, and Dr. Bright relates three instances in which "an ounce and a half," "something more than two drachms," and what was believed "to exceed an ounce," were taken; their baneful effects being warded off chiefly by the affusion of cold water upon the head, in the manner recommended by Mr. Wray some years ago, in a paper which he published in the Medical Repository.

MEDICAL GAZETTE.

Saturday, October 22, 1831.

"*Licet omnibus, licet etiam mihi, dignitatem Artis Medicæ tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.*"—Cicero.

USE AND ABUSE OF CERTAIN COLLEGE REGULATIONS.

We have been a good deal amused during the last week or two with witnessing the cool impudence and empty babble of a would-be director of study—an open instigator of cabals among the medical students of the metropolis. Amused we were, because we knew how impotent and harmless were all these unbidden efforts; nay, we should be quite satisfied with the amusement, did we not fear that some unlucky simpleton or other might be misled by the contrivances of the medico-political mountebank. As he "bides his time" with the instinct of an animal of mischief, he calculates about this period on the number of the inexperienced whom he may hope to delude; nor does he scruple to employ the most abject arts of insinuation to gain the ear of those who are about to enter upon their earliest course of medical study. To reason with him, or to reply to the sort of trash which he indites, were out of the question; it would first be necessary to know exactly what he aims at—what it is, in the name of every thing incomprehensible, that the man means to insist on? Is it the abolishing of all rule and government in the profession? or rather, is it not his notion that he may eventually be able to set up some fabric of his own upon the ruins of all that is respectable in our time-honoured establishments? The blockhead! the merest tyro could expose his folly and presumption.

Attending hospitals and lectures, and afterwards receiving certificates—

are not these the chief objects of the man's hostility? Has he not, in his very last effusion, held up the clinical instruction that could be afforded by a private country practitioner as equivalent to that which the student could obtain from our greatest hospitals? Let us have the language itself in which his notable scheme is described. His country master is surely a most accomplished character:—

"He furnishes his apprentices with *repeated demonstrations in anatomy and botany*, and reads to them during an hour or two daily, upon subjects connected with *the theory and practice of medicine*. With a view of making them acquainted with *pharmaceutical chemistry*, he experimentalizes with his pupils in his *snug little laboratory*; and with a wish to teach them diagnosis and prognosis, he causes *one of his apprentices daily* to accompany him *to the bed-side of the sick*; while, in order to render familiar to them those changes which the various organs of the body undergo from the progress of disease, he neglects no favourable opportunity of instituting *post-mortem examinations* (!) Thus tutored, how can these apprentices fail to acquire a knowledge of their profession?"

The question is really too silly to merit a reply. The personage (of whom more anon) who doles out these copious stores of medical tutorship, is, a little higher up, described as one who had just "walked the hospitals" in this metropolis, who had reluctantly acquired the "requisite quantity of recognized certificates," had happened to succeed in passing the "ordeal" of an examination for his diploma, and who had but lately "plunged into the active duties of his profession;" when, being "happy to avail himself of the assistance of one or two apprentices," he succeeds in swindling the parents of the hopeful youths out of 200 or 250 guineas for each, under the *false pretence* of completing their education for those sums. And this is the able gentleman whose tutorial care should supersede the acquisition of recognized certificates, and the attendance of London hospitals; this the *beau ideal* of the medical millenium which is to take place upon the abolition of the old system! Surely chaos must come again.

Hospitals! who is so weak as to endure for a moment the folly of him who would undervalue them? Hospitals! without which even the best appointed schools (if they can be said to be appointed at all without them) cannot be looked upon as half made up—can scarcely deserve the name of schools so long as the defect exists. We should be paying but a sorry compliment to the understanding of our readers were we to dwell upon the thrice-told tale of the great advantages of hospital attendance in the acquisition of a competent medical education; and we decline the task the more willingly as it has been so recently and so ably done in our pages by the accomplished professor of medicine in the University of London. But we cannot suppress our indignation at the bold effrontery of the man who is not ashamed to stand forth as a public adviser, and to advocate the possibility of their being dispensed with.

And is there a shadow more of feasibility about his notions regarding certificates? Questions may arise, as they have often arisen of late, touching certain differences as to the persons from whom certificates should be received; but we believe there never was a time when those documents were not demanded as a preliminary for an inaugural examination—as a rational, and in some sort a sufficient guarantee, that the individual presenting himself was not unworthy of being admitted to the test—a test which, without it would either have to be conducted with a painful minuteness, or be liable to frequent frustration, from the circumstances of disreputable persons offering themselves as candidates, and from the growing prevalence of the cramming system.

Time was, undoubtedly, when restrictions had to be imposed upon those who, without sufficient reason, assumed to themselves the right of bestowing certificates; when, in consequence of the great increase of persons educated to the profession, many additional schools were set up—places in which, for temporary purposes, inefficient plans of instruction were adopted—and where persons became teachers who were totally unprovided with the suitable apparatus, preparations, drawings, clinical wards, or subjects for dissection. This grew, at length, into an evil which could no longer be endured.

Nice distinctions had to be adopted with regard to the bodies or individuals to whom this important privilege of giving certificates was to be entrusted; and, from time to time, alterations had to be made, all tending to check the intrusion of unworthy characters, and to maintain the respectability of the profession. But we conscientiously believe, that the privilege is at this time fully as freely and judiciously communicated as it can be, with due protection to the interests of the medical republic; nor shall we take the word of any disturber who would prompt us to demand its more unrestricted bestowal —much less the abolition of the whole system.

The absurdity of the proposal is not a little ingenious. Abolish hospital attendance, and dispense with certificates! Upon what ground? Who proposes it? Why, one who has no connexion whatever with hospitals, because hospitals will have no connexion with him—and who has had no interest in the "certificate system," because *heretofore* he has never had a certificate to bestow; but one who has now a nostrum of his own to recommend, which is to cure all the diseases incident to medical education. Oh! the disinterestedness of this noble-minded projector—who has no other object than the establishing of a poor "cave of Adullam," A REFUGE FOR THE DESTITUTE, whither all who have nothing to lose, but every thing to gain, and are discontented with their present condition—particularly broken-down teachers of anatomy—may betake themselves, with the hope of comfort from the "eleemosynary fund!" Why should hospital tickets be purchased, or certificates of any sort obtained, when the price of the very cheapest of them were enough to buy a place in the "refuge?" And a place in the refuge makes a man a Doctor at once, and entitles him to the privilege of the fund. These must be allowed to be "advantages seldom offered to the public, and never equalled;" they are, moreover, seldom likely to be offered again—so now is the time to secure a bargain.

But what constitutes the most striking feature of the new establishment is, that it offers, like the cave of the Israelite, a most desirable haven to "every one that is in distress, and every one that is in debt;" and, we suppose, to every one that has got himself into the meshes of the law for the "good cause's" sake. And this, by the way, reminds us of a letter which we have just received, bearing on the subject in hand: we shall make no apology, but give it to the reader at full length.

To the Editor of the London Medical Gazette.

Taunton, Oct. 18, 1831.

Sir,—I must appeal to your generosity for setting me right before the public about a little matter in which I am concerned here, and an account of which I sent the Lancet last week; but what was my astonishment at finding my private and confidential communication laid before the readers of that journal in a garbled and strangely altered form!

I applied for advice in a bit of a law difficulty to the editor of the Lancet—knowing how clever he is in scrapes of this kind—but I did not want to have myself exposed, nor my name given to the public: judge, then, of my surprise and annoyance, at finding myself betrayed—I say *betrayed*—by the awkward management of that gentleman. Sir, my name is Cureall, or more properly Cur'l, as it is still pronounced here, and I live in this town, having a decent practice of from two to three hundred a year. There is no reason now why I should conceal any thing from you: I am exposed and disgraced—myself and my business, by the way in which I have been introduced into the pages of your contemporary: I am regularly blown up by having any thing to do with that magazine of scandal and absurdity; and I have no hope of recovery but by throwing myself on the public generosity through your kind interference.

You must know then, sir, that I am lineally descended from the celebrated Edmund Curl, of whom such honourable mention is made in Pope's works: this should be some guarantee for my claim to a character for learning. The name by some corruption, I suppose from the connexion of some of my later ancestors with the healing art, has descended to me as Cureall; and by that denomination many of your readers will, perhaps, recollect my father, who was sentenced to confinement in Ilchester jail for twelve months, for having been unsuccessful in an experiment with prussic acid, which he tried in the cause of science. From him I received the first rudiments of my general and medical education; and, in process of time, came to London, where, to do your contemporary justice, I found the hospitals the most useless places I ever stept into. I learned nothing; I walked them—that was all. None of the surgeons seemed to care any more for me than if they never saw me, or had never received my money. It was the same way at lectures. I attended all the prescribed courses at one of the great hospital schools, and, except when paying fo my tickets, never had the honour of enjoying the conversation of any of my preceptors, who in fact spoke only to those who were anxious for further information, and I did not need that. I would have been quite solitary, and perhaps have died of the vapours, had I not picked up with some despised pupils like myself, and with them contrived to spend the time pretty tolerably until the courses expired. Well, for all that, I was not deficient at the examination. I was always ready at gathering knowledge, and contrived to have enough to secure my diploma. What with the talk which I could always muster, on no matter what subject, and what with a little *cramming*, I passed. But what does it signify how I passed, when I am conscious that I know quite enough for any country practitioner? I was not going to be an hospital surgeon, or a metropolitan teacher; and so, in short, with that impression, I went home and began to practise.

It is not true that I bought a business; no, I got into notice by the credit I had of being all-accomplished, coming straight from the feet of the Gamaliels in London; not that I have got into any such mighty practice either, but my London education, and my easy account of it, stood my friend and got me a couple of apprentices, with whom I secured very respectable fees. Of course I said nothing, when taking the lads, about regulations of hospital attendance, and certificates from recognized schools. I always thought, with your contemporary, that they were odious and disgusting things; and, having equity on my side, I set my conscience at rest. The parents of my apprentices, I knew, would hear of these odious regulations some time or other, and would be readily convinced of their grossness; and, besides, they were very well able to bear the loss, or to go to law with the College, if they thought fit. I thought, too, that if they did, it would be no harm; and *I* should have the credit of being instrumental in effecting this most desirable consummation. I merely promised to make surgeons of them; and surgeons they are, I have no hesitation in saying, as good as myself. I taught them, it is true, more theory than practice—that was unavoidable; but I taught them the most useful practice: what is the use of capital operations I would be glad to know? I never had a capital operation to perform in my life, and perhaps never shall, but I have had a fair share of the useful minor surgery — much teeth extraction, phlebotomy, treatment of cut shins, sprains, and burns; and what more does a country practitioner require? "*My* snug little laboratory" —oh come, that is too good a joke! I may have mixed sulphuric acid with infusion of roses, or lemon juice with solution of soda, but as to direct experiments for learning or teaching chemistry, that is all — nonsense. Clinical instruction I certainly gave when I could, and I remember we once had a fine fever case for a post-mortem; but I never could venture upon a *subject*; I should have been taken up for a resurrection-man, I have no doubt, if I attempted it, and I always had a wholesome fear of the law of misdemeanors before my eyes.

Well, now came the time for it. My pupils' apprenticeships expired, and they were to be examined in London without more ado. I gave them their indentures, and a general certificate for what I had taught them, and desired them to demand an early examination at the College. With what anxiety did I await the first post after their arrival: it all came out at once: a knock at my door; the fathers of my pupils enter to insist upon an explanation of their sons' being refused examination. I explained the gross injustice of such an occurrence, and proved how they *could* not be refused. "But, zounds! sir, they *have* been refused," cried one of my furious visitors. It was of no use: they swore they would not be trifled with; and, sure enough, it was not long till I had notice of an action for *swindling* served upon me, and that action now pends.

Now, sir, I appeal to you, is not this a scandalous result of the London Hospital and Lecture-certificate system? Am I not a martyr to the "horrible and disgusting," "cold, unfeeling, and relentless" system pursued by the College? But, as I said before, I throw myself on your generosity, and leave my case entirely in your hands; though, to tell you the truth, I should not have troubled you, had not Wakley made so base a use of my confidence.

Your most obedient, humble servant,
PETER CURECALL, M.R.C.S.

However much we may be obliged to Mr. Curcall for the honour of his transferred confidence, we fear he will not find us of much use to him: we would recommend him though, by all means, to become reconciled to his old patron and adviser as soon as possible, and perhaps he may thus be enabled to exchange his "snug little laboratory" for a snug little professorship in the Refuge for the Destitute. In the meantime, the best thing we can do for him, we have done: *litera scripta manet:* what he has written, he has written; and his epistle carries its own commentary along with it.

FACTS REGARDING CHOLERA.

GREAT pains have been taken by Drs. Russel and Barry in tracing the history of cholera as it manifested itself at St. Petersburg and the adjacent places. Their inquiries have been chiefly directed to ascertaining its mode of propagation, with a view to determining the question of contagion; and although some of the facts are difficult to reconcile with any of our prevalent theories upon the subject, yet we must say that the great majority of them point to infection conveyed by persons, and not by goods or clothes, as the usual source of the contamination. The gentlemen alluded to seem to have exerted themselves to lay aside all preconceived opinions — marking the events passing under their own immediate observation, and in tracing the history of what they did not themselves witness—admitting only the testimony of those who had seen what they narrated; a caution which they were led to adopt in consequence of the colouring which they found to be often given to circumstances according to the views entertained by the narrator as to the nature of the disease itself. Of the many important facts which their last letters (received a few days ago) contain, we can only give a few, but we hope soon to see the whole laid before the public in a systematic shape. There is at St. Petersburg a city prison, under the medical charge of Dr. Bish, an intelligent physician, and previously to the appearance of the epidemic, an anti-contagionist. He, as well as all the other officers of the establishment, resides within the walls of the prison, by which all communication with the neighbourhood is prevented On cholera appearing at St. Petersburg, all intercourse between the gaol and the town was rigidly cut off, and no instance of the malady occurred for some time; but at length the wife of one of the prisoners, she being also a prisoner, was readmitted from one of the hospitals to which she had been sent to be cured of a syphilitic complaint, no one labouring under such being retained for treatment in the prison. On her return she passed through the apartment where her husband was: she spoke to, and saluted him, but proceeded in a moment to the part of the building appropriated to females. She had some diarrhœa when admitted, which on the following night proved to be cholera, and of which she died in twelve hours. Three women, who had been in the room with her, were next taken ill, and after them her husband, who also died. There were four hundred persons within the walls, and of these twenty-seven had cholera, which in fifteen proved fatal. No case occurred in the portion of the prison allotted to nobles, it being apart from the rest.

There is a German colony on the Neva, thirteen versts from St. Petersburg. The houses are detached with gardens, and the surrounding country highly cultivated, while the inhabitants are more cleanly, and fonder of the open air, than the Russians. Thither some persons fled on cholera appearing in the capital; one of these, a female, took the disease, and died of it; but it did not spread, no other instance of it having occurred, though her bed seems afterwards to have been used. Indeed, there are many instances in which the beds and clothes of those who have died, seem to have been made use of with impunity. Opposite this colony, on the other bank of the river, which is there about the breadth of the Thames at Blackfriars, are Russian villages; but they did not escape, though the locality was as salubrious as that of their German neighbours. It is a curious fact, that another German colony, on the road to Moscow, between Yshora and Colpina, escaped, although the disease raged at both the places just mentioned; but it was observed that no travellers ever stopped at the German village, because the others afforded much better accommodation.

At Cronstadt, as at Moscow, no case occurred among the military cadets (150 in number), all communication having been cut off between them and the rest of the fortress; but, on the other hand, several instances happened where persons had the disease, though no communication with any source of infection could be traced. Thus a man, confined by hæmoptisis on the third floor of a house, who had not been out of the room for many days, and who saw no one who had been exposed to the disease, took it notwithstanding. At the Foundling Hospital a good many children died of cholera, and several nurses had it; and it is a curious fact, that when any of these last, who were suckling, had the disease, so as to render it necessary for the infant to be given to another nurse, none of those who gave the breast in this way became affected with cholera, although, in many instances, the infant's clothes were not changed.

QUARANTINE.

We understand that most vigorous arrangements have been made for the purpose of excluding cholera from our shores, if this is be effected by quarantine. We hear that a considerable additional number of ships of war have, within the last week, been ordered upon this service.

DRS. RUSSEL AND BARRY.

The Russian Government has conferred the decoration of St. Anne, of the second order, on Drs. Russel and Barry, for their zealous and scientific investigations on the subject of cholera at St. Petersburgh. These gentlemen are supposed to be now at Hamburg, where the presence of the cholera will probably detain them for some time.

THE CHOLERA AT VIENNA.

The mortality from the cholera in this capital, during the first month of its visitation—that is say, from the 13th of August to the 13th of September, amounted to no more than one hundred deaths. But about the latter period, a heavy rain and storm having prevailed for the better part of three days, the disease broke out with the most alarming violence on the night of the 13th, and, in the course of four-and-twenty hours, swept away eighty victims; and this principally in the *city*. In one little street, not containing above ten or a dozen houses, six persons died on that night. The higher classes seem to have been peculiarly singled out for the ravage; and the Doctors Sidprowitsch, Gassner, and Roehrig, were among the first who fell. Since this alarming burst, however, the cholera has not gone on with proportional fury: it is comparatively tranquil, though the deaths are still by no means inconsiderable.

If the local peculiarities of Vienna be taken into account, it is not difficult to explain why the higher orders have particularly felt the severity of the disease, and also why the *city* has been its principal haunt. Vienna, the city, is small, compact, and surrounded by suburbs, from which it is separated by a wide glacis. The streets are very narrow, and the houses immoderately high. Yet it is here that the higher classes reside, though their first-floors are generally destitute of both air and light. The lower classes, meantime, enjoy the upper stories, as well as the more spacious and airy suburbs.

It should not escape observation, too, that Vienna is almost completely hemmed in with a chain of mountains; which, however, do not protect it from violent gales during the best part of the year, and especially about the equinoxes.

Rheumatism, in a severe form, is a constant resident at Vienna; and, every autumn, a malignant dysentery prevails, of which the mortality is considerable. The people, too, it may be remarked, particularly the higher orders, are fond of *good living*.

On the whole, it would seem to augur well for the future progress of the complaint, that in Vienna it has not been more severe; for few cities in Europe, perhaps, possess more of those elements which constitute the *fomites* of this direful malady.

EXTRACTS FROM JOURNALS,

Foreign and Domestic.

REPLACEMENT OF NOSES.

Dr. Barthelemy, in a paper in the *Journal Hebdomadaire*, on the re-union of parts entirely separated from the body, gives the following nose cases, which he says he has had upon the best authority. The first of them he obtained from a lady, who gave him *her honour* that it was true. A soldier, at Lyons, in 1815, had the end of his nose cut off in a duel by his adversary's sabre. He put the piece of nose in his pocket to keep it warm, and returned home to his lodgings. A surgeon was sent for, who put the disunited parts together again; and they *took* to admiration.

For the following case the veracity of Dr. Regnault, of the Military Hospital of Grand Caillon, is pledged. In the prison of Niort two men had a fight, in which one of them bit off a considerable portion of the other's nose. The unfortunate fellow who suffered the mutilation did nothing for four or five hours but deplore his loss: the piece of nose, however, he picked up, wrapped it in his handkerchief, and put it in his pocket. A fellow-prisoner suggested to him the propriety of applying to the surgeon of the jail, to see what he could do for him; he did so, and the medical officers having speedily warmed some alcohol, steeped the part in it, and then put the divided surfaces together. At the end of ten days they were solidly united.

ACTION OF VARIOUS MEDICINES ON THE HEALTHY BODY.

A set of experiments has recently been performed at Leipsig on the action of several important medicines on the healthy body. They were made on twenty-two physicians and medical students, between the ages of twenty-one and forty-five; two boys, and three females, one twelve, another eighteen, and the third forty-five years of age.

Nitrate of Potass.—Eight members of the association above mentioned took twice a-day doses of nitre, gradually increased from fifteen grains to a drachm, and the effect was excitement of the kidneys, intestinal canal, and skin. The sensation of coolness felt in the mouth, throat, and sometimes the stomach, on taking a solution of nitre, is soon followed by a state of re-action, accompanied by increase of the pulse. It is, therefore, not an antiphlogistic, as some physicians imagine. It ought to be prohibited in all inflammatory affections of the alimentary canal or genito-urinary system; neither is it a proper remedy in cutaneous diseases, except to produce determination to the kidneys or intestines. As a purgative it ought to be altogether abandoned, since we possess many safer and more certain medicines of this class. As a diuretic it may be admissible in some acute dropsies, but in general it is an ineligible diuretic, for other remedies of the same class are more powerful and safe, and do not disorder digestion, as nitre does, when long continued. The best effects are to be procured in inflammation of the brain or its membranes, or of the throat, or even of the chest. According to the experiments made with it on the females of the club, it is an emmenagogue.

Cherry-laurel water. — The article made use of was prepared by distilling three pounds of liquid from a mixture of six pounds of water, one ounce of rectified alcohol, and one pound of fresh laurel leaves chopped. Twelve members of the club took it in doses progressively increased from five to twenty-five, and finally to one hundred and twelve drops. Its whole action appeared to be concentrated on the brain, the symptoms being a sense of weight in the head, sleepiness, torpor of the intellectual functions, general lassitude and feebleness, retardation of the pulse, corresponding in degree to the headache; and this state was preceded by dull and pungent pain in the head, chiefly in the fore part, in the region of the optic nerves. The pathological cause of all these symptoms appeared to Professor Jörg to be turgescence or plethora of the blood-vessels of the head. It likewise produced unpleasant tickling in the larynx, cough, and an increased secretion of mucus, continuing sometimes for several days, and constituting a slight attack of bronchitis. Hence he infers, that cherry-laurel water is contra-indicated in inflammation of the brain and lungs, and in the first stage of scarlatina and puerperal fever. By its secondary action in lessening nervous sensibility and retarding the circulation, it may be useful in inflammation of the abdominal viscera, or organs of generation, or in excitement of these organs without inflammation. It is contra-indicated, he further conceives, in spasmodic affections where the cause is excitement of the brain, or turgescence of its vessels. M. Jörg has found that the cherry-laurel water, like the other forms of prussic acid, is rapidly decomposed, and likewise that its effects on different people are very different in degree. Bitter-almond water, though stronger in taste and odour than cherry-laurel water, is less active and more uncertain in its physiological effects. The effects of pure prussic acid are similar to those of the cherry-laurel water.

Valerian.—This was taken in infusion in the dose of from two to eight drachms in four ounces of water, or in powder in the dose of half a drachm or a whole drachm, or in the form of tincture in various doses. Sometimes no effect was manifested; sometimes it acted on the brain; sometimes on the alimentary canal. When the brain was acted on, excitement was first remarked, being indicated by serenity or disposition to liveliness; then slight congestion followed, as was shewn by the sense of weight in the head; but there was never prostration nor torpor, as after other medicines which have the same kind of action. When the alimentary canal was acted on, eructations were produced, with a sense of fulness in the stomach, flatulence, colic, and tenesmus; sometimes nausea, with a bitter taste. Valerian likewise quickens the circulation, though moderately. It was farther remarked that the action of half a drachm continued four hours, that of two drachms twelve hours, that the action on the stomach is most easily induced by the powder, that on the brain by the infusion, and that the disagreeable taste of the infusion passed away quickly, while that of the powder remained several hours. M. Jörg concludes from his experiments, that valerian may be useful in supporting the strength whenever inflammation is not present; but that it is contra-indicated whenever there is congestion in the head or abdominal viscera.

Serpentary.—Ten members of the club tried the effects of serpentary in the dose of from two to five scruples infused in four or eight ounces of water, and likewise in the form of powder in the dose of fifteen grains or one scruple. The result was that it excited the intestinal canal, produced determination to all the abdominal viscera, and occasioned flatulence. The usual symptoms were eructations, nausea, vomiting, pain in the stomach, colic, discharge of wind downwards, tenesmus. Sometimes, too, it produced a sense of heat, weight, and pain in the head, and at other times excitement of the circulation, or increased urinary secretion. Its effects lasted from eight hours with small doses, to twenty hours with large doses. It is therefore inferred to be a remedy which can only be useful when the practitioner wishes to rouse the vitality of the alimentary canal; hence in particular it may be useful in colliquative diarrhœa, not connected with inflammation, as it tends to diminish the secretion of mucus.

Arnica.—The Flower, in the dose of from two to thirty-six grains infused in from one drachm to six ounces of water, was taken by thirteen individuals. It produced much irritation in the whole alimentary canal from the mouth to the anus, particularly in the gullet, stomach, and small intestines, exciting rather increased contraction in these parts than increased secretion. It likewise excited the brain, probably through sympathy with the intestines; and a distinct acceleration of the pulse, increased perspiration, and various modifications of the urine were also observed among its secondary effects. Its action lasted from twenty-four to thirty-six hours. The conclusions are, that the flower of the arnica may be useful in general or local debility of the intestines, brain, or locomotive organs, but is contra-indicated when there exists irritation or inflammation of the intestines, or congestion in the brain. Externally, an infusion in eight parts of water caused heat and itchiness of the skin without redness; and the flowers themselves also produced redness in eight hours, without affecting the cuticle. Thus they appear to be useful rubefacients of the mildest kind. *The Root* affects the muscular coat of the alimentary canal and the brain, more powerfully than the flowers; and hence M. Jörg thinks it may be useful in chronic or colliquative diarrhœa, without inflammation, where the object is to strengthen the intestinal canal without augmenting its mucous secretion. Besides these effects, both the flower and root appeared to have a powerful effect in stimulating the absorbent vessels, and may be useful, as they have been conceived by some to be, in promoting the removal of indurations of various organs. In this respect they have some analogy to calomel in action; but they have a greater tendency to excite inflammation.

Camphor was sometimes taken in powder in the dose of half a grain, and from that to twelve grains—sometimes in the form of tincture in the dose of half a grain, and from that to four grains. The result was, that it stimulated directly the brain and alimentary canal, and indirectly the urinary organs, the skin, and organs of circulation. The effects on the brain were very different in degree on the different individuals—some experiencing much excitement, and being then thrown into profound sleep, while others remained free from any cerebral disturbance. It increased the heat of the body in all, and often produced sweating; it accelerated and strengthened the pulse, modified the quantity and composition of the urine, and excited the organs of generation. Camphor would thus appear to be contra-indicated in acute inflammatory fevers, and in irritation of the urinary organs, however contrary this principle may be to the usual therapeutic creed of physicians, and in every state of the cerebral system where congestion either exists or is apprehended. It is indicated in diseases of debility, and as it does not produce visceral congestion in the abdomen like the two preceding, it should be used in preference to them in tympanitis and colliquative diarrhœa, not connected with inflammation. The dose should be half a grain to one grain or upwards.

Castor in the dose of ℈s. produced in all who tried it disagreeable eructations, but no perceptible effect on any system of organs, and in small doses it had no effect of any kind. M. Jörg therefore concludes that it might be erased without detriment from the list of the materia medica.

Musk was taken by nine persons in doses varying from two to fifteen grains. It appeared by no means so diffusible a stimulant as is generally supposed. It powerfully excited the alimentary canal and the brain, producing eructations, sense of weight in the stomach, and dryness in the throat; also sense of weight in the head, and giddiness. The secondary effects on the brain were more prominent; the symptoms consisting of yawning, somnolency, depression, and finally deep, prolonged sleep. When the dose was large, trembling and even slight convulsions of the limbs occurred. It excited the circulation, rendering the pulse fuller and more frequent. It never produced its peculiar odour in the sweat, urine, or other secretions. His experiments led the writer to infer, that musk can only be used in debility of the brain and nervous system, when no congestion is present—but that it is not so appropriate a remedy in extreme nervous debility as is commonly supposed, since the stimulus it produces is followed by a corresponding collapse. Three or five grains are a sufficient dose; the effects cease in eight or twelve hours.

St. Ignatius Bean.—From half a grain to four grains of the powder, or from nine to ninety drops of a tincture made with eight ounces of alcohol and one ounce of the bruised seeds, produced salivation, nausea, weight and pain at the epigastrium, eructations, colic, constipation, or purging, and irritation of the anus. Its secondary effects were weight in the head, giddiness, pain and inflammation of the eyes, and finally much prostration and apathy, sometimes with increase of pulse, and a sense of creeping and pricking in the urethra. These effects disappeared and reappeared several times. Professor Jörg recommends it in weakness of the stomach and intestines, especially when attended with induration of the mesenteric glands; in diseases of a periodic character, as a powerful alterative; and in atony of the brain and weakness of sight. The dose should be half a grain of the powder once a-day; it may be gradually increased.

Assafœtida.—This, in doses varying from one to fifteen grains, stimulated the alimentary canal, particularly the gullet, stomach, and small intestines. Secondarily it produced cerebral congestion, indicated by dull pain and excitement of the circulation and respiration. It also produced strong excitement of the genito-urinary organs. Hence its use in indolence of the digestive organs he considers well-founded; but he is inclined to doubt the propriety of its almost universal use in hysteria and hypochondriasis, when depending on irritation of the uterine or abdominal organs. He thinks it is usually given in too large doses, and too often; from half a grain to one grain he generally found sufficient, and the effects often lasted till the second or third day.

Digitalis, in doses varying from half a grain to three grains, excited directly and powerfully the brain, the alimentary canal, and the genito-urinary organs. Its secondary effects were directed on the circulation, which it depressed. Its operation on the brain was indicated by intoxication, giddiness, dull headache, warmth of the face and dimness of sight; its operation on the alimentary canal by a sense of heat in the pharynx and gullet, colicy pains, and sometimes increase, sometimes diminution, of appetite and of the alvine discharges; its operation on the urinary organs was evidenced by a remarkable increase in the secretion of urine, observed in all but one, and attended sometimes by diminution, sometimes by increase of its colour; its operation on the genital organs by itching of the glans, and by erections, and in females by symptoms resembling those which precede menstruation. The consecutive effects were marked depression of the circulation, indicated by feebleness and smallness of the pulse; which, however, were always preceded by excitement.—From these results, the author concludes that digitalis is far from being an appropriate antiphlogistic remedy;—that it is contra-indicated in hooping-cough, acute hyprocephalus, and all dropsies connected with inflammation of the serous membranes—the very diseases, in fact, in which it is chiefly used; but that it is very useful whenever the object is to stimulate the urinary organs to increased secretion. He questions the propriety of its employment in diseases of the heart, as its depressing operation must prove hurtful in some and useless in others. The dose should be from a quarter of a grain to a grain of the powder, and when its action has commenced the dose should not be repeated oftener than once every twelve, twenty-four, or forty-eight hours, as its effects often continue thus long. In unnecessarily large doses he is inclined to think it may tend to induce an inflammatory state of the urinary organs, and consequently diminution, instead of increase of the secretion of urine.—*Archives Géneralės de Médecine.*

CAMPHORATED OIL IN CHOLERA.

In lieu of cajeput, when dear and scarce, M Champonnier, of Paris, recommends the medicinal use of a compound of olive oil and camphor. The proportion is twelve grains of camphor to the ounce of oil. The dose is a teaspoonful, in a warm drink, taken every hour if the symptoms are urgent, every second hour if not so severe. Tea, he thinks, is the best menstruum. The natives of India employ for the same purpose, and with good success, a warm infusion of the leaves of the *laurus camphora*, in sweet oil. M. Champonnier's design is to imitate them, and he adds, that he has found his compound a most valuable remedy, and a complete substitute for cajeput.

DIAGNOSIS OF HYDROCELE.

A very simple and ingenious method of ascertaining the transparency of the parts in obscure cases of hydrocele, has been hit upon by M. Segalas, and submitted to the Académie de Médecine. He merely applies to the scrotum one end of the eye-tube which is employed for exploring the urethra, and puts his eye to the other, upon which the existence of fluid, if it be there, becomes manifest. The result is explained by the isolated condition of the eye, the perpendicularity of the rays of light transmitted through the tube, and the pressure of the instrument against the scrotum. M. Roux, however, doubts the utility of this method in certain cases; for instance, where there is a cartilaginous thickening of the tunica vaginalis; and upon the occasion of discussing M. Segalas' contrivance before the Académie, the professor mentioned the curious fact of the extreme transparency of the scrotum in some examples of sarcocele. He was lately called in to a case of this kind, where the surgeon in attendance mistook the complaint for a simple hydrocele, misled by the transparency of the parts, and neglecting to take into account the specific gravity.—*Gazette des Hopitaux.*

[M. Roux's objection, in the meantime, proves nothing; and perhaps the only circumstance that would tend to depreciate the utility of the new method, is, that it enables us to see *too well*—to discover transparency where such a thing would be least expected. With the tube and a candle light may be perceived through the palm of the hand.]

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

BY

DR. ANTHONY TODD THOMSON.

LECTURE I.

October 12th, 1831.

General Observations on Clinical Instruction.

GENTLEMEN,—In commencing the series of discourses which I propose to deliver, on the nature and treatment of the diseases which are brought under your notice in this institution, a few remarks on their utility may not be unimportant. Such discourses are generally called *clinical lectures;* but, in the strict signification of the term, no discourses can be termed clinical except those which are delivered at the bed-side of the patient, and which have a reference to the immediate state of the disease which is there under the eye of the student. Were it admissible, in any situation, or under any circumstances, to disregard the feelings and to overlook the safety of the patient, for the benefit of the student, such a plan of instruction would be highly advantageous; but this is a sacrifice which humanity and justice equally forbid; and, therefore, the discourses to which the term clinical is applied, refer merely to cases actually existing, which have been recently seen by the student. Their intention is to explain the practice that has been adopted, and to make evident to the pupil the principles by which the practitioner treating them has been guided: in plain language, to display to the student the mode of applying and rendering practically useful the instructions which he is supposed to have previously acquired, in the various elementary branches of medical science, from attendance on lectures. Such is the nature of the instruction which you are to receive from me in this place: the real object of it is to make you acquainted with the phenomena of disease, and the effects of medicinal agents in counteracting these, or in restoring the healthy state of the system.

It has been supposed that dispensaries are little calculated to afford much information respecting diseases, from the absence of proper opportunities for tracing correctly the influence of medicines; and that it is in hospitals only, where the patients are completely under the control of the physician or the surgeon, that these objects can be fully attained. Far be it from me, gentlemen, to depreciate the utility of these institutions, which shelter, whilst they administer to the relief of the poor and friendless, whom disease has overpowered, or whom accident has rendered incapable of pursuing their ordinary avocations: they are the noblest monuments which can be erected to the glory of a nation, to those godlike feelings of benevolence and charity, which are the boast of England; and, assuredly, in no part of the world are their duties more ably or more humanely fulfilled. But to assert that it is in these institutions only that practical medicine can be taught, is assuming too much; for, although we must admit that, when the patient is completely under the control of the physician, the progress of disease can be most correctly and closely observed, symptoms traced to their sources, and the real influence of remedies ascertained, yet, it must be borne in mind, that this is not the state in which the practitioner is to combat diseases in private practice. Under such circumstances, therefore, the treatment of every case must be regarded simply as an experiment, successful or otherwise, which teaches the practice of medicine, when the patient is placed in a situation the most favourable for the fulfilment of the various indications contemplated by the physician. If you suppose, gentlemen, that the results which you witness in the hospital are to follow the administration of the same medicines prescribed for similar disease in private practice, you will soon find yourselves woefully mistaken. It is not disease only that you have then to combat: injurious localities, occupations, unwholesome food, extremes of temperature, habits, passions, prejudices, all array themselves against your judicious efforts, and the best directed skill, and too frequently render it difficult to decide, how much of the benefit following any line of treatment is to be conceded to nature, how much to be attributed to art. If these remarks be correct, it is not too presumptuous to assert, that whilst hospitals are essential in the education of a physician —under which term I comprehend every well-educated practitioner of medicine—in order to teach him the nature of diseases and the value of remedies, dispensaries are equally so, inasmuch as they display to the student the effects of the treatment of diseases, under those circumstances which are likely to influence his practice, and to modify powerfully the operation of medicines, in every step of his future career. In these institutions, he sees the patients in the situations which they occupy in society—surrounded by all the inconveniences which attend poverty—borne down by vexations and anxieties—and, because free from the restraints imposed in an hospital, perhaps still under the sway of obnoxious habits, from the indulgence of which their diseases may, in a great measure, have originated. To observe the influence of medicines on disease, under such circumstances, is of the utmost importance to the student: he acquires an additional art, invaluable in the practice of his profession,—that of overcoming the obstacles to the accomplishment of his object, which seldom or never present themselves in hospitals. The hospital physician resembles a pilot who steers his vessel into a safe harbour, amidst the shoals and hidden rocks which obstruct its entrance, with the position of each of which he is well acquainted; but who performs his duty in a calm, and under the most favourable aspect of the heavens; the dispensary physician, and the private practitioner, is the pilot performing the same duty amidst the violence of the hurricane; he has not only to look out for the breakers, but to baffle the conflict of the elements.

In making these remarks, gentlemen, it is far from my intention to lessen, in the smallest degree, the high appraisement which is usually bestowed upon hospital practice: the advantages to be derived by the student who witnesses it with the attention which it merits, are great and manifold: in no other place so well as an hospital can he attain an accurate knowledge of the advantages which remedial agents are capable of conferring; under no other circumstances, I would almost venture to aver, can he witness diseases unmasked and in their true characters; and, undeniably, in no other situation has he so favourable an opportunity of tracing their effects upon organization, and the changes produced by them on the tissues of the body. He not only enjoys the advantage of seeing much, but of investigating deeply; not only the benefit of having pointed out to him the complexities of disease, but also the probability of beholding them unravelled. To deny the utility of such establishments to the pupil would be folly; but, because hospitals hold forth such advantages, are we to refuse to Dispensaries those which they also are capable of affording? I am most anxious, indeed, to clear from your minds, gentlemen, the prejudices which have been so assiduously attempted to be impressed upon the student against the utility of Dispensary practice, by some hospital physicians and surgeons. Believe me, that, although it cannot be regarded as equivalent to that of an hospital, yet, to those who are disposed to observe attentively and to reflect on what they see—to reason, with the aid of their teachers, upon the phenomena of diseases, and to labour unremittingly in attempting to trace them to their causes; it holds out many advantages, not the least of which is the opportunity that it affords of studying exceptions to general rules, and, as I have already said, of anticipating those obstacles to success in the treatment of diseases which will invariably be encountered by them in every stage of their professional progress. You must perceive, therefore, gentlemen, that if the desire of acquiring a practical acquaintance with the phenomena of diseases, and of studying attentively the varied resources of medicine, be sincere, a Dispensary affords many opportunities of doing so; and of throwing, if not a strong, at least an indubitable light, upon the path of the student. Let me now endeavour to point out to you the manner in which these advantages are likely to be secured.

In the first place, be assured, that whatever may be your genius for observation, it will avail you little in the investigation of diseases if it be not based upon the foundation of an intimate knowledge with the elements of medicine—with anatomy, physiology, chemistry, materia medica, and those other branches of science which are justly regarded as valuable accessaries in a medical education. Without the aid of these, indeed, it is possible that an individual of great natural acuteness, and possessing a talent for observation, may attain to considerable expertness in discovering and managing diseases, within the beaten track of practice; but the moment any thing extraordinary occurs, the uneducated practitioner, like the fair-weather pilot, loses his control, and is at once confounded with the vulgar. I must therefore presume, gentlemen, that you are at least somewhat advanced in these studies before you enter as pupils of this Institution.

The diseases which present themselves to the attention of the student in a Dispensary, consequently those which you are likely to see in this place, are seldom of that acute description which requires the daily, I may almost say the hourly, attendance of the physician: but, if they are not those of the most formidable kind, they are those of most ordinary occurrence, and therefore they require to be well understood and readily recognized. The talent for observation in medicine, as in general matters, is strengthened by habit; but it must first be acquired; and the acquisition is not to be accomplished without the closest and most attentive examination, not only of those symptoms which obviously present themselves to the senses, but of those which lie deeper. "He who is incapable," says Zimmerman, "of observing the moral man, will never be able to know diseases." To accomplish this, however, the observations must not be made in a cursory and partial manner; it is not sufficient for the pupil merely to listen to the examination of the physician; he must employ his own eyes and exercise his own reasoning faculties; if he do not fully comprehend the aim of any query in the examination of a patient, he must not rest satisfied until it be explained; and in affording this satisfaction, I trust, gentlemen, you will never find me backward. In the same manner, I am desirous of seeing you inquisitive with respect to the object and the intention of remedies; and believe me, that the more closely you examine their nature and effects, the more confidence you will repose in those that you employ hereafter, and the more you will have reason to hope from their administration.

One of the most important qualifications of a good practitioner, and, consequently, one of those which ought to be most assiduously cultivated by the student, is such a love of sincerity as will lead to the observation of symptoms such as they present themselves, not such as we would suppose them to be. It is by accuracy in observing effects that we gradually acquire the knowledge of causes; and it is by reasoning upon these that we are led to the indications and the means of cure. In recognizing any disease, therefore, the description of which you may have previously read in books, or to the history of which you have listened in lectures, note down every exception presented by this individual case to the general appearance and progress of the symptoms, as detailed in the general history of the disease; for, as you will discover that all cases offer some exceptions to the general rule, you will find these particular histories of the greatest utility in your future practice.

Students are apt to fall into the common error, that the greatest sum of information is to be obtained where the greatest number of patients congregate; but crowded hospitals, or very well frequented dispensaries, rather embarrass the student than contribute to his instruction. He sees too much, and reflects too little; the rapid succession of objects does not permit him to fix his attention on any of them: in his endeavour to embrace the whole, he receives only an obscure impression of each; and, consequently, can neither understand nor retain the remembrance of those minute circumstances which have regulated the practice adopted; for "the understanding," as it has been justly remarked, "does not gallop so fast as the physician." It is these truths, gentlemen, which have decided me to confine my remarks, in the series of discourses which I am about to deliver to you, to some only of the cases which shall come before us, and to enter rather minutely into the history and the peculiarities of these, than to attempt a large field of research, or to indulge in discursive observation. The advantages of such a selection is acknowledged in the best clinical hospitals, both in this country and on the continent. In the Infirmary of Edinburgh, which contains nearly two hundred beds, the clinical wards to which the observation of the students is chiefly confined, usually contain only sixteen or eighteen patients; and more advantage is derived by carefully attending to this selection, than by daily walking, as it is emphatically termed, the largest hospitals of this metropolis. It is not the cursory observation of a great number of patients, but the attentive consideration of particular cases, which contributes to form the well-qualified practitioner.

Let me warn you also, gentlemen, against committing another mistake too frequent with the young aspirant to practice—I mean that of disregarding the most common phenomena of diseases, merely because they are common and obvious. It is from these chiefly that the student learns to generalize, and to establish his principles. Every disease consists of a series of symptoms following in a certain order. Extraordinary symptoms may occur, and ought, undoubtedly, not to be overlooked; but it is only by a previous and correct acquaintance with the common phenomena, that the deviations from them can be recognized, and the knowledge of them become practically useful. Every symptom in the concatenation of phenomena is of value in reasoning upon the disease; and the loss of the smallest link in the chain may disable us from pursuing the mazes of the obscure labyrinth in which the origin of malady is too frequently involved. It is only by such a scrupulous examination of the circumstances characterizing diseases that they can be familiarly recognized—that effects can be separated from causes—and that the influence of remedies can be accurately noted. The disease ought to be as readily determined as the face of a well-known friend; and when the examination is once fully accomplished, the information acquired is not confined to the occasion which called it forth, but is extended to the whole life of the physician. It is this which constitutes experience; and a close attention to which distinguishes one practitioner from another. But in noting, carefully, the ordinary phenomena of diseases, you must note also the diversity of circumstances which, even in the same city, and in the same locality, constantly contribute to vary them in different individuals. To illustrate this by an example: two men, living in the same house, are attacked with acute rheumatism; one of them is a single man, with no cares nor anxieties except on his own account; the other is burthened with a wife and a large family, borne down with the *res augusta domi*, depressed in spirits, and exhausted in frame. Although the symptoms of the disease may closely resemble one another in both cases, yet it would be highly indiscreet to treat them in the same manner. Depletion might be useful in the one, and, although freely employed, yet may be easily borne; in the other it would prove injurious, even in a moderate degree. To cure both of these patients, whilst the general plan must coincide, a different method must be adopted in some particulars. The physician must therefore possess not only a distinct idea of the disease, but he must endeavour to become acquainted with the state of the body in which it found the patient; and with the causes which may have contributed to disturb or injure the ordinary functions, and to predispose it to the attack under which it is labouring; for it must be recollected, that every symptom is not the effect of the immediate exciting cause of the disease, but that some symptoms are the consequences of previous deviations from ordinary health.

I might now, gentlemen, proceed to point out to you the method of investigating diseases; in what manner you are to recognize and to separate symptoms constituting the disease and inseparable from it, from those which are only of occasional occurrence; how you are to be guided in forming your diagnosis, to give the disease a name, and to arrange it with its fellows in a nosological sense; how to discriminate the signs which may enable you to predict the event, and acquire that tact which always obtains reputation to the practitioner—skill in prognosis; and, finally, on what grounds you are to found your claims for public confidence, in the performance of the important and responsible duties to which you have dedicated yourselves. But as these are to constitute the business of our frequent meetings in this place, they will, necessarily, form the subject matter of the discourses which I propose to deliver as we proceed in the performance of the task allotted to us, respectively, as teacher and as pupils. In urging you to improve the present opportunity of acquiring the habit of attention and close observation in the examination of diseases, which will make the retrospect of your time spent here an object of satisfaction and pleasure, it shall be my endeavour to make my instructions clear and perspicuous to the least informed amongst you; and whilst I trust you shall be stimulated by my anxiety to set you an example of diligence and attention, you will also be convinced, that the man who undertakes the practice of the profession of medicine, commences a course of study which can only terminate with his life.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

LONDON HOSPITAL.

Strangulated Scrotal Hernia, twice operated on. Death—Post-mortem Examination.

William King, aged 24, was brought into the hospital July 7th, about one o'clock, with a large strangulated scrotal hernia on the right side. It occurred while lifting a deal plank, about two hours ago, and has since rapidly increased in size. He has never been before subject to hernia. The tumor is very large and tense, and extremely painful; no impulse is communicated to it in coughing. He rejects every thing, his countenance expresses much anxiety, and he complains of pain in the abdomen. His bowels have not been opened since the occurrence of the hernia. The taxis was used without effect.

Ordered warm Bath, 100° Fahr.

While in the bath, further attempts were made to reduce the hernia, but without success. At two o'clock he was carried into the operating theatre. Mr. Luke made an incision, of about four inches, in the direction of the inguinal canal, and extending to the upper part of the tumor, and then carefully cut down to the neck of the sac, which the tendon of the obliquus externus was distinctly observed tightly girting, so that it was impossible to introduce a director between them; the stricture was therefore divided from without, by means of the point of a scalpel, and then, without opening the sac, the intestines were returned into the abdomen, on which the man immediately expressed himself relieved. The edges of the wound were approximated by two sutures; it was dressed, and a compress applied with a spica bandage.

4 p.m.—Sickness less; slight tenderness of the abdomen. Pulse 84.

V. S. ad ℥xvi.

8 p.m.—Magnes. Sulph. ʒii.
Aq. Menth. Piper. ℥j.
Syrupi Simp., ʒj.
Cap. 2dis horis donec alvus responderit.

10 p.m.—His bowels have been open twice; the last motion contained a small quantity of blood; skin hot; pulse soft; slight tenderness of abdomen.

Ordered forty leeches to the abdomen.
Cal. gr. i. c. Op. gr. $\frac{1}{8}$ 2dis horis.

8th.—He has slept during the morning. Abdomen slightly tender; pulse 90, and soft.

Rep. Mist. et Pil. tertiis horis.

9th.—He has passed a good night; bowels freely open; wound rather painful.

10th.—He has passed a restless night; much troubled with flatus; bowels not open; abdomen tender; tongue white.

Hirud. xx. abdom. Ol. Ricini, ℥ss.

11th.—No tenderness of the abdomen. The wound was dressed, and looked very well.

Vespere.—He complained of pain and tenderness of the skin on the right side of the abdomen, which was relieved by the application of twenty leeches. The Goulard lotion to be applied to the part.

12th.—Bowels freely open; tongue cleaner; skin less painful.

13th.—Has passed a restless night, and still complains of pain in his side.

Hirud. xx.

14th.—Pain in the side much relieved.

Porter, one pint daily.

The wound continued to heal very slowly, and a large sinus, nearly the length of a probe, was found to communicate with it, from which there was a great discharge of matter.

August 26th.—The sinus and wound nearly healed. About eleven o'clock at night he complained of being unwell. Four o'clock the next morning he began to vomit; immediately afterwards the hernia descended, his sickness continued; the abdomen and tumor became painful; the pulse small, and quick, and countenance anxious; skin bathed in perspiration. The taxis was used in a warm bath, but failed. About eight o'clock Mr. Luke made some further attempts at reduction, but without success.

Ordered Ol. Ricini, ℥j. and a freezing mixture to be kept applied to the swelling.

On the failure of this, the operation was again proposed and acceded to. An incision was made a little to the inside of the former, down to the neck of the sac, through parts thickened from the inflammation consequent upon the last operation; and attempts were made to reduce its contents without avail. It therefore became necessary to open the sac; on doing which, eight or ten inches of dark-coloured and collapsed intestine, in a state of engouement, were exposed. The stricture appeared to have been effectually divided, so that a finger could with the greatest ease be introduced into the abdomen. Much difficulty was experienced, however, in returning the intestine, and repeated failures preceded the last and successful effort, from the immense thickness of its coats not admitting of its easy passage through the freely divided stricture. It was at last returned by degrees, and the wound closed with sutures and dressed; pressure being made over the whole with a truss, to prevent reprotrusion.

[He continued to sink gradually, and died on the 7th of September.]

Examination of the Body, 2 P.M.—Upon opening the abdomen, the intestines appeared distended with flatus and were firmly agglutinated by lymph of a yellow colour; their peritoneal surface was very much inflamed; a portion of ileum of a dark colour was adherent to the internal ring; about an inch from the ring a contraction was observed in the ileum, which was thickened; the intestines beyond were not so much distended, or inflamed. The sac in the scrotum was inflamed and thickened, and at the bottom was observed the testicle, so that the opening to the tunica vaginalis had never closed, although no descent of intestine had taken place before the day that he was admitted. There was about a quart of pus in the cavity of the abdomen. The left testicle was situated just outside the external ring, and was small in size.

Fracture of the Skull—Trephined.

—— Perry, aged 73, was brought into the London Hospital, August 24th, about two o'clock in the afternoon, having fallen into a ship's hold, the depth of which was about twenty-five feet. There was a wound through the scalp and temporal muscle two inches in extent, situated above the anterior inferior angle of the parietal bone on the left side; the bone was denuded of its pericranium, and there was a fracture without depression, from the edges of which blood was oozing. There was also a wound through the scalp, in the same situation, on the other side; a slight hæmorrhage had taken place from the left ear. He was sensible; his pupils were rather contracted, and his pulse was quick, small, and irregular. His head was ordered to be shaved, cold lotion to be kept applied to it, and Cal. gr. ii. ter die sumend.

Vespere.—He had gradually become totally insensible, his respiration was laborious, and his pupils were more contracted—the left in a greater degree than the right. Some more bleeding had also taken place from the left ear.

V. S. ad ℥xviii.

25th. — He was completely insensible; pulse irregular and weak; pupils contracted, the left still more so than the right. As, from the situation of the fracture and the symptoms, effusion of blood upon the dura mater, from rupture of the spinous artery or one of its branches, was suspected, after a consultation it was determined to trephine. Mr. Hamilton enlarged the wound, and performed the operation over the seat of the fracture. Upon the removal of a circular piece of bone, the pulsation of the brain was observed, and a considerable quantity of coagulated blood, some of which was with difficulty detached from the dura mater with the handle of a scalpel. A large groove for the lodgment of a vessel was observed in the portion of bone which had been removed. Another piece of bone contiguous to, but above and rather behind the last, was then taken away, which was followed for a short time by some arterial bleeding. After the operation the man's pulse was fuller.

Ordered Cal. gr. v. quartis horis.

26th.—His mouth was slightly affected; he appeared more sensible, but was unable to speak: pulse regular, and soft.

Cal. gr. iii. octavis horis.

27th.—He appeared partly sensible, and could mutter a few words; he had passed a restless night.

28th.—His mouth was very sore; he understood a little when spoken to, and had a calm pulse.

Hirud. xii. capiti.

29th.—Slightly sensible; pulse 85, and soft; mouth very sore.

Rep. Hirud. nocte maneque. Poultice to the wound, which had an unhealthy aspect. Omit Cal.

30th.—Much the same as yesterday.

31st.—He became quite insensible last night, but was a little improved this morning; pulse irregular and weak.

September 1st.—Quite insensible; takes no nourishment; pulse weaker.

2d.—Died at eight o'clock this morning.

3 P.M. *Examination of the Body.*—The wound in the scalp was in an unhealthy state. Upon removing the calvarium, some dark-coloured blood was observed upon the dura mater on the right side, and a small quantity on the left. The dura mater did not appear inflamed; when removed, the surface of the brain underneath that part of the skull which had been trephined, was, to a considerable extent, in a sloughy state; the substance of the brain was rather more vascular than natural. The fracture extended, on the right side, from the anterior inferior angle of the parietal bone, in two directions; one through the squamous and petrous portions of the temporal bone, the other through the temporal portion of the sphenoid; then uniting and passing through the sella turcica to the anterior inferior angle of the left parietal bone.

WESTERN DISPENSARY,

23, CHARLES-STREET, PARLIAMENT-STREET, WESTMINSTER.

Typhus Fever successfully treated with Tartar Emetic in large doses.

ANN EDWARDS, æt. 30, was admitted under the care of Dr. Lilburn, on the 3d September, 1831, while labouring under typhus fever in a very severe form. The patient had been confined four months since, of her second child, who is still living. She complains of great pain in the head, with occasional delirium; the countenance is pale and collapsed; tongue black, and dry; the teeth covered with sordes; pulse at the wrist small, irregular, beating 136 times in a minute; respiration hurried; secretion of milk suppressed. About ten days since, she was labouring under diarrhœa, which ceased spontaneously. The bowels are at present irregular; urine scanty. Petechiæ have appeared on the neck, arms, and body. The patient lies supine, and appears perfectly insensible to every thing around her, unless powerfully excited. The head to be shaved, and kept constantly cool with cold water.

Applic. Hirudines, vi. temporibus.
℞ Sulph. Magnesiæ, ʒij. Aquæ ℥j. M. bis indies sumend.

September 4th.—Continues much the same. The bowels have been opened twice.

Cont. remedia.

5th.—Much the same. The cold water to the head to be continued, and the following medicine to be given, unless vomiting be produced.

℞ Antimonii Tartarizati, gr. j.
Syrupi Scillæ, ʒj. M. 4ta quaque horâ s.

6th.—The first dose of the medicine occasioned nausea, but it has been continued regularly, as directed. The tongue is less hard, but continues black; has had no alvine evacuation; urine still very scanty, and high-coloured; pulse 122; the skin is not quite so dry, but no sensible perspiration.

Cont. remedia.

7th.—Appears a little better; has less delirium, and the thirst is not quite so urgent; tongue beginning to get cleaner.

Pergat.

℞ Antimonii Tart. gr. j.
Syrupi Scillæ, ʒj. M. 2da quaque horâ s.

8th.—The bowels have acted twice; delirium and thirst abated; tongue moister; pulse 110.

Cont. aqua font. Capiti.
Cont. Mist. Ant. Tart. 2da quaque horâ.

9th.—The petechiæ are less livid; pulse soft and regular, 110; thirst lessened; has no vomiting; no alvine evacuation.

℞ Antim. Tart. gr. jss.
Syr. Scillæ, ʒj. M. 2dis horis s.

10th.—Slept well during the night, and is in every respect better this day; has had no return of delirium the last twenty-four hours. Pulse 100; the medicine produces nausea; she is able to sit up in bed.

Cont. Mist. Ant. Tart. co. 2dis horis. s.

12th.—The medicine occasioned vomiting last night, and was in consequence discontinued; she is better, and able to sit up in bed; one alvine discharge; pulse 100.

Cont. Aquâ capit. applicanda.

13th.—Continues improving.

Capt. Magnesiæ, gr. ij. ex aquâ bis indis.

23d.—Has continued to improve daily since the last report, and is now able to walk to the Dispensary with ease; appetite improving; tongue clean; pulse 76; bowels opened once or twice daily; urine natural in colour and quantity.

30th Sept. 1831.—Discharged cured.

NOTICE.

The name of the patient in the case of Aneurism inserted in our last, was different from that in the Clinical Observations upon it. This was a mistake on the part of the journal from which they were taken.

W. WILSON, Printer, 57, Skinner-Street, London.

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BEING A
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Medicine and the Collateral Sciences.

SATURDAY, OCTOBER 29, 1831.

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LECTURES
ON
THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

BY DR. ELLIOTSON.

LECTURE IV.

Recapitulation — Predisposing and Exciting Causes of Disease—Temperaments—Idiosyncrasy — Semeiology or Symptomatology — Diagnosis—Prognosis.

I FEAR, gentlemen, that the first three or four lectures will be considered by you exceedingly dry; but before entering upon the subject of any particular disease, it is quite necessary that you should understand the various terms which will be employed, and I therefore trust that you will not be tired of hearing the definitions which I feel myself under the necessity of giving.

I stated that pathology, or the doctrine of disease, was divided into two branches, general and special; the latter relating to any particular disease, and the former to diseases in general. The observations I am now making are preparatory to entering upon special pathology—the nature, history, causes, and treatment of any particular diseases. I stated that general pathology was usually said to consist of four parts;—one treating of diseases in general, their resemblances, and their differences; another of their symptoms; another of their causes, and a fourth of their treatment. I have made all the general observations which I think necessary respecting the first division of general pathology—the differences and similarities between various diseases.

I proceeded to consider in the last lecture the various causes of disease in general. I spoke both of those which predispose to disease, *causæ predisponentes*, or *proegumenæ*, and those which are called occasional, exciting, or *causæ procatarcticæ*. The exciting causes of disease have been arranged by many in three classes;—those which stimulate, those which depress, and those which act only physically or physico-chemically, independently of stimulating or depressing. I mentioned that the chief sources of the exciting causes of disease were derived, in the first place, from what appear to have been absurdly enough called "the *non-naturals*;" namely, the air we breathe, food and drink, the fluids proper to be retained in the body and those which are proper to be discharged, motion and rest, sleep and watching, and the passions of the mind.

With respect to the first of these non-naturals, the *air*, it may injure the body by its weight or levity, by the various electrical qualities which it may possess, by its temperature, by its dryness, and by its moisture. Various winds have various effects upon the human body of the most curious kind, according to the countries, or districts, or parts, over which they pass. With respect to temperature, it injures the body not only by its height or lowness, but also by its vicissitudes, and not only so, but accordingly as it is applied partially or generally to the body. A temperature that would do no harm if it were continued, would be productive of serious mischief if it suddenly succeeded an opposite temperature. A temperature which would do no harm if applied throughout the body, frequently does great harm if applied partially. Every body knows the danger of suddenly cooling, if the body be over heated and fatigued; and of a draught applied to any one part of the body. Included, perhaps, under the head of air, may be mentioned various effluvia and various odours, as they are transmitted through the air.

Upon *food and drink*, another source of disease, I need say but very little; they may be injurious either by their quantity or quality.

As to what are called *excreta* and *retenta* —to speak first of retenta: the great thing here alluded to by old authors is the blood, and it was supposed formerly that it might greatly injure the body mechanically by its excess, either absolute or relative, so that *plethora* formerly occupied a very prominent part among the causes of disease. You will find some curious distinctions made by old authors on the subject of plethora. They called it absolute or true, or *ad vires*, or *ad vasa*, when the quantity of blood was absolutely in excess; they called it *apparens* when the blood was in its natural quantity, but expanded. They fancied it might be expanded by heat, and in that way a temporary plethora be induced. Plethora, too, was divided again into a third variety—*relative*. When there was no actual increase of blood, when the blood was not expanded improperly by heat, but when the space in which it moved had become too small, this was called relative plethora, or *plethora ad spatium;* where the vessels, it was supposed, became contracted, and the blood was in excess to merely the contracted space. Plethora, however, certainly may be either *general* or *local*. A local plethora is what in modern language is called an inflammation or congestion, just as the case may be. When the blood is really in excess, I believe that it is generally of too rich a quality; that there is too little of its watery portion, and too much of the crassamentum. I need not say that the opposite of plethora is *inanition*. This is a state which is induced by excessive discharges, or from the want of food; so that the blood which was in the body is let out, or the blood which naturally escapes in the various excretions is not replaced. Anæmia, or bloodlessness, but more properly a watery state of the blood, will arise from an ill understood, I may say, not at all understood, state of the system, in which, although food is taken, nourishment is but very sparingly extracted from it. Cases of this description are seen continually: chlorosis is one of this kind. But the blood sometimes certainly is in fault with regard to its constituents, not merely as to whether it is too rich or too poor, but, as it would appear by some recent experiments of a friend of mine, Dr. Stevens, who is about to publish a work on Fever, and Dr. Clanny, by the saline ingredients erring in their quantity; so that the blood, in many cases of disease, is absolutely deficient in its saline particles. It is evident, however, that diseases of the solids themselves will sometimes cause these aberrations of the blood; so that the blood itself is not the cause of the disease. It may become an instrument of farther disease, though disease of the solids originally may have given rise to the aberration of the vital fluid.

With respect, however, to the excreta, you may easily conceive how excessive discharges will occasion various diseases. The excessive discharge may consist of blood, of bile, of secretion from the alimentary canal, fæces, of urine, of perspiration, of saliva—that, however, seldom does much harm—and of semen. These are the fluids which particularly escape from the body, and the escape of which may do harm. But with regard to the latter, the discharge of semen, although one must suppose that the simple excessive loss of the fluid must be injurious, yet it is very certain that the mode in which that fluid is lost exerts a very great influence upon the constitution.

In regard to *motion* and *rest*, I need not point out the ill effects of sluggishness on the one hand, nor of fatigue on the other. Nor with respect to *sleep and watching* need I make any remarks, for they come under the same head as motion and rest; sleep and watching being to the brain, what repose and motion are to the body.

With respect to *the passions of the mind*, I mentioned in the last lecture that they are a frightful source of disease; much more so than is commonly imagined. An immense number of cases of disease of the heart and disease within the abdomen, as well as of the brain itself, arise, I am certain, from unpleasant passions of the mind. However, it is not merely unpleasant passions that affect the body injuriously—*violent* emotions of the most pleasurable kind will sometimes operate in the same way. History relates many accounts of persons who have died from excessive joy.

Besides those circumstances, however, which I have just mentioned, the body is liable to injury from two very common sources, one of which is *poison* of various sorts, which poison may enter the system by the surface, may enter it by the breath through the medium of the lungs, or may enter it by being swallowed; and it is a very curious fact, that many which act offensively when admitted into the body one way, have no effect when admitted in another. I believe it is a fact, that the poison of hydrophobia, or the poison of a serpent, may be swallowed with the most perfect impunity.

In addition to poison, the body suffers much from *mechanical injuries*—mechanical injury, of course, is a cause that may affect almost any part of the body.

Among the *causes* of disease which chiefly act by giving a predisposition, we may mention that of age. All periods of life are subject to their own diseases, whether we divide them with Shakspeare into seven, or with those authors who mention five, or those who mention more. Every age is particularly liable to certain diseases: for example, you rarely see gout in an infant, nor is it common for old persons to have the symptoms of acute hydrocephalus. In short, every age seems marked out as particularly liable to be, if not destroyed, at least impaired, by particular diseases. The same is the case with respect to sex: each sex is particularly liable to its own affections. Hysteria, which occurs only now and then in the male, occurs far more frequently in the female, and even when there is reason to suppose an affection of the uterus. Of course there are certain diseases which can affect but one sex.

As to temperament, disease depends much upon it. We are all of some temperament, or some combination of temperaments, but excess of one temperament gives a great liability to disease. You know that the usual divisions of temperaments are four—the sanguineous, the melancholic, the phlegmatic, and the choleric. The sanguineous is marked by smoothness of skin, softness of hair, a quick pulse, a warm surface, great excitability of the whole frame, and generally a fair skin with florid complexion. The melancholic, on the other hand, is marked by a slow pulse, by a dark swarthy complexion, dark and strong hair, and less susceptibility of emotion—that is to say, of sudden violent emotion, although when certain emotions take place they are said to be more continued. The phlegmatic is marked by torpidity, paleness, and flabbiness. The choleric is characterized by reddish hair and great excitability. However, these divisions, although they were made by the ancients, are not so much attended to now. We certainly often see these temperaments strongly marked, but they are all infinitely combined.

An attempt has been made within these few years to divide temperaments differently, according to the activity of particular parts. For instance, some persons have the chief activity in the head; they have a large intellectual development of the head, as some people say, and that would be called perhaps the *cerebral* temperament. Others, again, have a large broad chest, with a considerable development of all the muscles, and this is called the *thoracic* or *muscular* temperament. And again, others are sluggish, prone to eating and drinking, with large bellies, and are said to have the *abdominal* temperament. Now it is rather an abuse of terms to call these temperaments, for I think the word *temperament* rather refers to the constitution and character of the whole frame together; yet there is no doubt that these divisions occur, for you see some persons all intellect, others all muscle and chest, and you see some portly, with their big bellies and lean pates, their heads suffering in proportion. In truth, Dr. Thomas entitles his work, ‘Physiologie des Temperaments ou *Constitutions*.

However, whether you choose to call these temperaments or not, it is well to know that these persons will suffer various diseases, according to the division under which they come, just as is the case with regard to those who are of a sanguineous, phlegmatic, choleric, or melancholic temperament. They will be particularly liable to diseases of the head if they be of the cerebral conformation; they will be subject to affections of the chest, not of a phthisical, but of an inflammatory kind, if they have a full thorax; and of course they will be subject to various hepatic, and other abdominal diseases, if the abdomen gain the sway. It is worth while to attend to this other division of temperament, although, perhaps, the term itself may be objectionable.

Besides original temperament, acquired peculiarities take place by a long residence in a particular climate: by long residence in a particular spot certain dispositions of the body are engendered, and a second nature, if I may so say, is produced. The various occupations of life have the same effect; the various trades have their various diseases. On this subject you cannot, I believe, read better works than Rammazini's and Thackrah's. Custom and habit, whether referable to trade or situation, or action, or any other circumstance, have also very great effect in producing or in preventing the cure of disease. For example, if a person be in the habit of dram-drinking, and has a sore, nothing is more common than to see that sore not heal, in spite of the best surgical treatment, till the person is allowed his dram. After a surgical operation a person has been known to sink through the surgeon not having been aware that the individual had indulged in some bad custom until it had become a bad habit. From the want of a patient being allowed a quantity of gin in the course of a day, he has died after an operation, whereas he otherwise might have done well. This has been strikingly shewn in other cases where the knowledge was happily attained; and a good allowance of gin after some severe accident has caused every thing to go on well, though the patient at first appeared sinking. Bad habits are, therefore, sometimes to be indulged, although they should certainly never be acquired.

Independently of what is called temperament, an individual may have a particular disposition not acquired; it may be born with him, and be hereditary. A peculiarity is sometimes so exceedingly singular, so totally different from what we observe in other people, that it is called *idiosyncracy*. It is nothing more than a peculiarity either original or acquired by habit, and sometimes it is only connate, and sometimes hereditary. As an instance of this, you will sometimes find a person who cannot eat a certain article of food. I recollect seeing a young woman who could eat the hardest salt beef, and digest it well; but if she took a raspberry, or currant, or any fruit, she was instantly thrown into the most violent spasms of the stomach, so that a stranger would have fancied her life in danger. These are called idiosyncracies, and it is of great use to know them, because some persons are peculiarly affected by certain remedies; and that which is a proper remedy for the disease may be improper for a particular patient.

So much, therefore, gentlemen, with respect to the causes of disease in general—to general *ætiology;* and I will now proceed to consider the third branch of general pathology—the symptoms of disease in general—which is called *semeiology* or *symptomatology.*

I believe I mentioned that *symptoms* are every thing which are observed in a patient out of the course of health; *singulæ quæ in ægro præter naturam observantur res.* The most evident, the most constant of these, are put together, and said to constitute the disease. You will recollect that the disease is not the cause of the symptoms, but that the disease is the collection of symptoms. I mentioned this, and illustrated it with respect to jaundice and to epilepsy, the outward signs of which are jaundice and dropsy, while the inward state is the proximate cause.

Now these symptoms may be *essential,* they may be what is called *pathognomonic,* and they may be *accidental.* The *essential* symptoms of disease are those which are necessary to make the disease—to constitute the idea of the disease. You cannot, for instance, suppose a patient to have pleurisy unless there be a degree of feverishness, with sharp pain of the side, increased upon respiration, together with a cough. These symptoms would be called the essential symptoms of the disease, yet no one of them would make the disease. A patient may have feverishness without pleurisy; or a sharp pain in his side without pleurisy, because it may be muscular or it may be neuralgic; or again, he may have cough without any pleurisy. These are then, you perceive, essential to the disease altogether, but no one of them constitutes it. If, however, a symptom be so important to the disease that this cannot be present without it, then it is called *pathognomonic*—a symptom without which you could have no conception of the existence of the disease. There are, however, but very few of these symptoms. In jaundice, the yellowness of the skin, of the sclerotica, and of the nails, is pathognomonic. Without making any farther inquiry, you may be sure that the patient labours under jaundice; but in general we make out the disease from the groups of essential symptoms, and from taking into consideration every circumstance of the case. Besides these symptoms, others are *accidental.* Many cases have symptoms which are merely accidental, and are not necessary at all to the disease: the disease has frequently existed without them.

You will find symptoms also spoken of as *positive* and *negative;* and it is of great use to attend to the latter. A negative symptom is perhaps, an improper expression: a symptom must, properly speaking, be positive, but, from the absence of certain circumstances, of certain symptoms, you make out frequently the nature of a disease. If there be two diseases with certain symptoms common to both, but one of them has a symptom which the other never has, then, if that symptom be absent, it enables you to make out the true nature of the case. Hence the absence of certain symptoms is frequently of as great importance to be attended to as the presence of others.

Now, from the presence of the symptoms chiefly, but not entirely, we make out the character of the disease; and the determination of the character, the name and nature of the disease, is called the *diagnosis.* To speak again of jaundice, from the yellowness of the skin, the whiteness of the fæces, and the dark colour of the urine, you make your diagnosis, and say the disease is jaundice, although, indeed, you may do so merely from the yellowness of the skin and the sclerotica. But you will frequently find your diagnosis is not easily made from the symptoms alone—you must look into the exciting and predisposing causes of the disease: you must ascertain what the patient is likely to have—what predisposition he has; you must also ascertain to what exciting cause he has been exposed; and then conceiving what it is likely would ensue, and observing what actually has ensued, as far as you can judge, you are enabled much better to form a conclusion as to the real character of the affection. You will find your diagnosis also materially assisted by attending to the history of the case; not merely observing the symptoms that present themselves to you, but inquiring carefully of the patient the whole story from the beginning. You will thus ascertain whether these circumstances confirm what you suppose to be the nature of the case. You will also find it of great importance, in making out the character of the disease, to learn the effects of any previous treatment that may have been adopted. If you learn that vigorous treatment has been properly employed, and has had no effect whatever, you would doubt very much as to the propriety of the view which had been taken with respect to the nature of the affection. In making a diagnosis all these points are to be scrupulously attended to.

It is in making an accurate, careful diagnosis, that the medical practitioner chiefly shines. When the nature of a disease is once ascertained, it is no difficult matter to treat it; other qualities of mind are then required—frequently only courage, or mere perseverance—but it is in making a diagnosis, in ascertaining the character of a disease, that the scientific practitioner outshines the inferior. Unfortunately this is a point not attended to as it ought to be. Many persons pride themselves in being good practitioners, because, without knowing what is the matter, they can say what will do good. It is an unscientific way of proceeding, and, if I could not practise better for making a good diagnosis, still I would be as particular on that point as possible, for the sake of observing a good general rule—for the sake of endeavouring to treat the patient better than I otherwise could, notwithstanding all my conceit—and for the purpose of being ready to meet any unexpected emergency that might arise. A medical man who will not take the trouble to establish a diagnosis, is just like a surgeon who will not condescend to learn the anatomy of hernia, but who says that he knows if he cuts this way and cuts that way, he shall liberate the entangled parts, and do the operation as well as the best. I have witnessed this: but, then, what are such men when a difficulty arises? They are lost in perplexity, and glad to apply to those who know better. It is impossible to be too minute in making a diagnosis.

You have not only to make a diagnosis as to the general character of the disease, but you have to ascertain what variety of the affection it is—whether it is one with strength or weakness—and to weigh the minutest circumstances respecting the case. This is, perhaps, an exercise of patience, but it is an exercise highly gratifying; especially when you find you have made a correct diagnosis.

The judgment you form as to what will happen in a case, is called *prognosis.* In order to make a very correct prognosis, it is best to make an accurate diagnosis; for if you do not know what the affection is, you will often be unable to tell how it is likely to terminate. For the sake of making a prognosis you must observe all the symptoms, and learn the history of the case, exactly as in making a diagnosis; but there is something more to be done than that—you must know the direct tendency of the disease, the course which the disease is always inclined to run, the course which it is likely to run in the particular individual, according to his age, according to the sex, according to his previous habits, according to all those circumstances which I have previously mentioned as to his individual peculiarities, and accordingly as you have it in your power to put into practice requisite curative means. In some cases, if we were allowed to practise our art freely, we should make a favourable prognosis; but our hands being tied, as they sometimes are, when others ill informed or inactive are called in over us, we must give an unfavourable prognosis, simply because the patient must be allowed to die. In making a prognosis there is frequently less skill required than in establishing a diagnosis; for some persons, with a sort of tact, will tell whether an individual is in danger or not without knowing what is the matter with him. Many persons who have no knowledge of medicine, or at least verylittle, have such strong perceptions, that, from the aspect of an individual, and from general observations of their own, they are enabled to give a very correct prognosis, either favourable or unfavourable. This I know to be the case. There are a great many intelligent officers who, from seeing their men fall around them in battle, sick in hospitals, have become, in one sense of the term, good doctors; and though they are not able to cure the disease, or tell what is the matter, they will be able to give a shrewd and correct guess as to how matters will turn out. But although some people, by nature, are well qualified for this, while others are not, yet the medical man must be taken as he is; and whether he does or does not possess natural sagacity to make a prognosis, it is his duty to make as careful a diagnosis as he can, in order that he may make as good a prognosis as possible. If his intellect be not of an acute description, yet, by knowing the case, and by the aid of science and minute observation, he will be able to form as good an opinion as the man who by nature is qualified for these things; and if with science and observation is combined native sagacity, then I need not say that his prognosis will be much superior to that of the individual who only trusts to his natural powers.

You therefore see what is meant by diagnosis and what by prognosis; and I cannot urge upon you too strongly the most careful attention in establishing the former. Whether you can cure the patient or not, you should make it a solemn rule to investigate the case to the utmost. It is only by this that we can profit ourselves, or that those who come after us can profit. If we be not careful we cannot advance science, and the next generation will be none the better for us.

With respect now to the symptoms of disease on which diagnosis and prognosis are in a great measure to be founded. The symptoms of disease may be divided, perhaps, in one point of view, into two kinds—those which are discoverable by the patient only, and those which are discoverable by by-standers also, or only.

Now there are some symptoms which a patient alone can ascertain—for example, his feelings. He it is that feels the pain, and it is a lucky circumstance that it is so; for if the doctor felt the pain as well as the patient, there are but few who could stand practising the profession. We know little about pain but from the patient's account; and in many cases in public practice we are deceived. We have, however, only done our duty in listening to the patient's account.

With respect to *pain*, it is of various kinds; it may be sharp, stabbing, darting, shooting, lancinating. These are all words used by patients. At other times the pain is not of this character, but dull, and then we call it an *ache*; and sometimes patients add the word pain, making it *aching-pain*. Sometimes you will have pain described as throbbing and pulsating; and you will hear patients also complain of pricking pain, as though a number of needles were being pressed against them. Now these things are all of great importance to be attended to, because they shew frequently the seat of the affection; and accordingly as a patient describes his pain, as sharp, or dull, or throbbing, or pricking, so you ascertain frequently whether he is telling the truth or not, because you see for other reasons a probability that, if he have pain at all, it is of a particular character, and you ascertain whether his account agrees with what you would, *a priori*, expect him to describe. You will sometimes hear pain spoken of as smarting, and a very high degree of this is broiling. In some affections of the abdomen you hear a patient complain of broiling pain; it appears to be a high degree of smarting, with a sense of burning.

Now if pain be increased by pressure, or by the application of any stimulants, then we say there is *tenderness*; but perhaps the word tenderness is more properly restricted to pain increased upon mechanical pressure. There are other feelings continually described by patients, as, for instance, a feeling of dead weight. This is particularly the case with respect to the head. You will sometimes hear persons complain of fulness, as if a part were excessively full; and now and then this sensation arises to such a pitch, that the part feels as if it would burst; and then we call it a feeling of tension. A feeling of tension appears to be a high degree of a feeling of fulness. You will now and then have patients complain of a sensation of pins and needles. It is very different from the sensation of pricking pain, as if needles were being run in; it is as if the pins and needles were rather blunt; it merely gives the lowest sensation of this kind, not amounting to pain. Pricking pain is of a very different character—for instance, that which people frequently complain of from inflammation of the urethra in gonorrhœa; they experience a pricking pain along the urethra; but the sensation experienced when the hands and feet are asleep is not called pain; patients say they feel as if pins and needles were there. You will frequently hear persons declare, that they have an indescribable uneasiness in a part, a sensation which nearly drives them mad; but they will not allow it to be pain. I have known this sensation occur in different parts of the body. I recollect one lady who had it in the neck, so that she was obliged to walk about the room followed by a servant rubbing her neck night and day. Unless this was done the sensation nearly deprived her of her senses, and her countenance betrayed the greatest distress. You will find other sensations spoken of by patients—a creeping, a crawling, or a feeling as if water were trickling down them. There is also a symptom which approaches nearly to pain, itching and tingling, which may be almost insupportable. Tingling is a high degree, I imagine, of what is called itching, united with a sense of pricking. In regard to other sensations, there is nausea felt in one part—the stomach; and now and then persons have an excessive sensibility of one particular sense—of the eye or the ear.

Now all these are symptoms which the patient alone can know any thing about. We all know something about them, because we are more or less ill at some part of our lives; but the medical attendant must take the patient's word for these sensations, at least he cannot ascertain the existence of them directly—he can only ascertain by other symptoms whether the patient is probably telling the truth.

Besides these, people have a feeling of great exhaustion, of great debility, and this is a point strictly to be attended to in forming the prognosis in an acute disease, for sometimes it is a fatal symptom. When a patient, for example, after inflammation of the bowels has no pain, but a feeble pulse, and complains that he is so weak that he feels as if he should die—that he never felt such weakness in his life—you may be almost sure that he will be dead before the next day. On the other hand, this feeling is very deceptive. Women are subject to a sinking sensation at the epigastrium; they tell you they feel as if they had no inside, and they are sure they are about to die, but the feeling is quite delusive. You might in the former case, after an acute disease, judge from seeing all, or the greater part of the symptoms decline, that the patient would soon get well, and that this feeling of weakness was nothing more than might be expected after severe indisposition; but you must take into account other symptoms, particularly the weakness of the pulse. But with respect to these women you find the pulse good, and they are able to walk about notwithstanding all the depressive feelings of which they complain. It is a very remarkable circumstance, and I shall find it necessary to draw your attention to it when speaking of diseases of the stomach, and of hysterical affections. Patients also alone can be conscious of drowsiness, want of appetite, inordinate appetite, and depraved appetite, although with respect to many of these things you can often by other circumstances ascertain whether they are telling the truth or not. It is to the patient, likewise, that you must trust for an account of the existence of tenesmus and of strangury—a frequent desire to go to stool, and a painful and frequent desire to make water. All these are instances of symptoms, of which you must receive an account from the patient himself.

There are some such symptoms, too, which are derived from other senses than those of general feelings, and these are referable to the ear. Patients will continually complain of the most violent beating pulsation in their head, and frequently, too, of a snapping and cracking there. Such symptoms undoubtedly do exist, but we can take no cognizance of them. With respect to sight, you must depend on the patient's account for the occurrence of flashes of light before the eyes; and for the symptom so common in many diseases—moats floating before the eyes, and for double vision or diplopia; and likewise for the symptoms of giddiness or vertigo. On the patient also you must depend for symptoms with regard to taste; some have a bitter nauseous taste in their mouth, of which you cannot judge. So with respect to smell; now and then persons have a most disgusting disagreeable smell, which no other person can notice. There was a case of hydrophobia at Guy's hospital, in which there was an instance of an accidental symptom; and that a symptom to be ascertained only by the patient—the patient experienced the most disgusting smell. You will find in cases of insanity that this occasionally happens. Now and then the smell which the patient perceives may be noticed by other persons, because it arises from some disease within the nose.

There are still other symptoms of which the patient alone can give an account—a defect in the senses, a loss of sight, a loss of taste, a loss of hearing; for all this you must depend much on his account. So with respect to many mental circumstances—the loss of many desires, and a feeling of strong desires; for all this you must rely upon the patient. You see, therefore, that in regard to symptoms, they may be divided into two classes, the first of which are evident to the patient alone, and the second those which are perceptible to the practitioner also or only.

At the next lecture I shall proceed to consider those which are of the latter description.

ON THE MEDICAL TREATMENT OF INSANITY.

Being one of the Croonian Lectures delivered before the College of Physicians, in May, 1831,

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Blood-letting—Mercury—Cold applications to the Head—Counter-irritation—Sedatives—(Acetate of Morphia—Belladonna, &c.)—Arsenic—Camphor—Purgatives—Diet.

THE application of medicine to mental disease must, of course, be directed, in the first instance, to the nature of the attack. It will become necessary carefully to inquire whether the brain is primarily or secondarily affected, and whether such affection is or is not connected with organic lesion. The physical causes must be carefully separated from the moral, and due weight assigned to each. If, on accurate inquiry, the patient has been exposed to causes sufficient to produce inflammation of the brain and redness of the countenance, injection of the conjunctiva and heat of skin very early in the disease point out increased vascular action, with increased power; antiphlogistic remedies, with blood-letting, principally from the jugular vein, are to be employed; and in such cases, and probably only in such cases, mercury may be employed, to affect the system; such cases will often recover. In the summer of 1827, I had occasion to visit a gentleman, æt. 30, whom I found in the following condition. He was lying on a sofa, in a state of great agitation, and complaining that he had committed some great crime, and was the object of a conspiracy. He complained of pain in the head, and would not permit me to feel the temperature of it. The pulse was quick and oppressed, and I recommended that he should lose blood. The first bleeding was borne very ill, but the second, to the amount of sixteen ounces, was attended with some relief. Cold was applied frequently to the head, and blood abstracted by cupping every day for several days, during which time he took calomel every four hours. As the mercury began to affect the mouth, a visible change in the mental malady came on; this gradually increased in the most favourable manner, and the patient entirely recovered, nor has he suffered the smallest relapse since that time. I had the advantage of the assistance of Mr. Tupper in this case. This case is only, however, an example of a few where the mental disease is going on in consequence of increased vascular action; in the great majority of cases the functions of the brain, in mental derangement, are increased in force, while the circulation is depressed, extremely quick and feeble, and the action of the heart giving way at the smallest abstraction of blood; and yet these are often attended with raving delirium, great increase of muscular force, and are, in fact, what are termed high cases. The consequence, I am informed, of such practice, is either the more frequent returns of the *high* stage, or the patient sinks into one approaching idiotcy.

"At the admission," says Pinel, "of any lunatic into the hospital, great care is taken to interrogate the relations on the subject of blood-letting; and the question is asked, whether it has been employed, and what was the result? The replies the most constantly made attest that the condition of the lunatic has constantly become worse immediately after bleeding." I think I ought not to omit a curious fact which occurred in the year 1813. Two young persons, of a similar age and temperament, arrived the same day; one of them had not been blooded, and her cure was effected in two months; a copious bleeding having been exhibited to the other, she was reduced to a kind of idiotcy, and did not recover the use of speech until near the fifth month; her complete re-establishment was not effected until nine months. We saw afterwards a singular example of a melancholic patient, who had been bled five times from the foot and three times from the jugular vein, and had fallen afterwards into such a state of stupor and debility that she passed several days without taking any nourishment." Dr. Haslam informs us, that it is only in the very early stages of the disease that blood letting is useful, and then he prefers cupping. It is also very remarkable that, according to his testimony, the appearance of the buffy coat on the blood, generally considered to justify such practice, has been found generally absent in cases of lunacy where venesection has been used. In more than two hundred patients, male and female, who were let blood by venesection, there were only six whose blood could be termed sizy. The experience of Pinel is very decided against this practice.

The experience of any individual, however zealous, must be extremely limited; and I do not venture, therefore, to detain the College with the result of my personal inquiries. I have endeavoured to obtain the result of medical practice on a large scale, and for this purpose selected Mr. Warburton's house, the White House on Bethnal Green, containing 400 patients, and where the skill of Mr. Beverley, the superintendant, and Mr. Phillips, the resident surgeon, embolden me to offer to the consideration of the College facts which have resulted from their extensive and assiduous observation, which I could never hope to have obtained from any private source. It is to these gentlemen I am indebted for the principal observations I am enabled to relate.

Messrs. Beverley and Philips state the following as the result of their observations on this subject:—

"The number of patients admitted with vascular excitement, requiring blood-letting, are very few indeed; we seldom or ever use the lancet in cases of excitement, if there is no evident effect upon the brain from increased arterial action, so as to lead us to fear an approaching attack of apoplexy or paralysis. The reason we do not use the lancet in cases without any such symptoms existing of disease going on in the brain, is that we have done so in several instances, and the result was not favourable; the patient became reduced from the loss of blood, and the excitement not abated, the powers of the constitution gave way, the tongue became typhoid, and the patient sank into a state of collapse and died."

As the result, then, of experience in cases of excitement, I presume that these arise from increased nervous energy, not depending on increased action of the heart and arteries, but on increased sensibility of the brain itself, and that blood-letting is not found useful.

Hence physicians have been under the necessity of seeking for means to allay the inordinately increased sensibility principally of course occurring in that division of insanity termed dementia. These remedies are various. I shall commence with cold, which may be administered in three ways—in the form of ice, in the shower-bath, and in a column of water, graduated according to the strength of the patient, and termed the "douche," a French word, for which I know of no corresponding one in our language. Of ice to the head, not only in order to diminish vascular excitement, but also to produce a really sedative effect, a diminution of intensity of sensibility of the brain itself, I can speak with some confidence. In the epidemic typhus fever which has recently raged among the lower classes in this town, and which has been of a marked and peculiar character, with scarcely any, if any, stage of excitement, the application of ice in the low maniacal delirium which accompanied it was attended with the happiest effects, and in more than one case appeared to be the principal means of cure; the patient sleeping quietly for hours under an ice cap, who, previously to the use of the remedy, had passed the night in low muttering delirium, or in the constant endeavour to leave his bed. This remedy appeared to me to be useful exactly in proportion as the delirium assumed the maniacal character. The torpor produced by extreme cold may convey a tolerably accurate idea of the modification which the extreme sensibility of the brain may receive from this remedy. The effect which pouring cold water on the head, in cases of affection of the brain with increased vascular action, produces, may be illustrated by the following very remarkable case, which I attended with my friend, Dr. Roupell, physician to the Seaman's Hospital. A young medical man was attacked with symptoms of inflammation of the brain, while pursuing with great ardour his professional studies; and being a medical man he was, of all other persons, the most difficult to treat. Notwithstanding the intense pain in his head, his quick and frequent pulse, he resisted remedies. Venesection he would not submit to, leeches he had an objection to, calomel he thought produced inflammation and ulceration of the mucous membrane of the intestinal canal;—it was proposed to pour cold water from a pitcher over his head; the consequence was diminution of pain, quiet sleep, and, in fact, so beneficial an effect that the patient himself frequently called for a repetition of the remedy. I saw this patient several times, and the impression on my mind was, that to this remedy, and to this alone, he owed his life in that attack.

The shower bath appears to have a very similar effect, and I am enabled to quote a case of its benefit in mania, from the testimony of the gentlemen to whom I have alluded.

"We have found, in some cases, the shower-bath of great service; but it appears to us, from the experience we have had of it, that it is more beneficial in cases of a very violent nature, with increased vascular excitement, as we have given it a trial in cases of various descriptions, and in some without the slightest benefit.

"There was a case admitted into this establishment, evidently a case that was completely cured by the use of it. A gentleman, aged 30, small, and light complexion, had been studying hard, and constantly confined to one room, was attacked with furious mania; thought he had found out perpetual motion, and that he could make the sun stand still. Pulse very quick, 120, and small; pupils contracted; imagined he could reach any thing he saw, and grasped at them; incessantly talking; tongue furred and dry. We ordered him to go into the shower-bath. His extreme violence put us on our guard, that we got six keepers to take him there. We persuaded him, with much difficulty, to go in, by saying it was only a sentry-box. On hearing that, he immediately went; the door was closed and secured; the shock was so unexpected that he screamed and held his breath for a short time after the shock was over; then gasped, and knocked the sides and the door into pieces, and stepped out; but was immediately secured, rubbed dry, and put to bed. He had a little refreshing sleep during the night. In the morning he vowed vengeance against the doctors for murdering him. We could not prevail upon him this day to go into the bath, which obliged us to confine and carry him there. He bore the shock better; was taken out, rubbed, and put to bed. Slept better; the tongue appeared cleaner, and he was not so violent. Bowels open; he begged to be released from confinement, which was complied with. He took a little exercise; we put him in the shower bath almost without any difficulty; sleep returned; gave him a dose of calomel and colocynth; more rational; he inquired what had been the matter; thought he had been asleep, and in the evening begged himself to go into the bath and have more medicine. From that time he became tranquil, took mild aperients, and was discharged well in a fortnight from the date of his admission."

"We have had several cases, nearly of the same nature, where the shower-bath in its results proved invaluable."

It is clear, therefore, from this statement, that it is to cases of dementia that it is applicable. I am not aware whether the douche is much employed in this country; in the houses where I have inquired, I believe not. Pinel speaks of it in high terms; and the power of graduation, so as not to give too violent a shock, allows of its employment in private.

The patient is put into the shower-bath, and I give the detail of the process in the words of the author himself, M. Pinel:—

"A happy combination of the douche with the bath adds greatly to its efficacy, and prevents even the smallest inconvenience which might arise from it. To each bath, and directly over the head of the maniac, is adapted a tube, so constructed as to let water fall three feet in height; the stream of water being proportioned to the end proposed, and graduated according to the symptoms, but in general the stream is very small. It is not until nearly the end of the bath, and during a few minutes, that the douche is administered; when the circulation of the blood has been drawn to the surface of the body, and is to be diminished towards the head by the cold produced. The douche is omitted as the disease begins to decline and during the convalescence, but is resumed at the approach of an accession of mania, or when it has already broken forth. If the appearance of excitement be very moderate, the practice is confined to letting, drop by drop, cold water fall upon the head, which determines a moderate degree of cold, both by the impression of the liquid and the evaporation which takes place."

The cold plunging bath, formerly so much recommended by Van Helmont, and in which the patient was half drowned, in the hope that this short interval between life and death would put an end to the chain of perverted ideas, and that, with new life, the patient would recover new senses, is, as it would appear, not now employed, at least in this country.

As the use of cold in various forms appears to be productive, under proper management, of the greatest advantage in cases of mania, a corresponding good effect has been said often to be observed in the employment of the warm bath in cases of melancholia. I was informed at Charenton, in France, and the Quakers' retreat, near York, that more marked advantage was derived from the employment of the warm bath in cases of melancholy than from any known remedy in diseases of the mind; and Mr. Tuke states this opinion before the Committee of the House of Commons:—"The warm bath (he says) is used more medicinally than the cold bath; and it has been observed that the warm bath has been found very beneficial, particularly in female cases." Dr. Guislain gives his very important testimony to the benefit of it, especially in cases where lunatics refuse nourishment. Like all other remedies, it is probably often of benefit, and occasionally fails. It appears likely to be more frequently advantageous when employed among foreigners, because from infancy warm baths are used, both as remedies and luxuries, and in either view may become more necessary than to persons who have never been in the habit of employing them.

Producing discharges from the scalp by means of blistering substances or escharotics, have been strongly recommended in chronic cases, both of mania and melancholy; and one of the greatest names which ever adorned the profession of physic has been brought forward in approbation of this practice. Dr. Jenner believed that he had cured cases of insanity by producing pustules on the scalp. A case was related to me, on good authority, where mania after continued fever was cured by this means twice repeated, the symptoms of mania entirely disappearing with the second crop of pustules. I have, of course, endeavoured to inquire whether such beneficial effects are at all frequent. "We have given this a trial, (say Messrs. Beverley and Philips) more particularly the tartar emetic ointment, and with, we thought, some advantage, in a few cases, but we found, from the constant restlessness of the patient, erysipelas came on from the friction against the pillow, so that we were obliged to abandon it. We afterwards tried it in several cases over the biceps humeri muscle with, I think, more benefit than on the head: these were cases of a very violent nature, stout robust habits, and who were, for the most part, subject to periodical attacks. The use of it had evidently the effect of making the paroxysms of much shorter duration. We have also used blisters to the inside of the thighs, and calves of the legs, with advantage." The employment of tartar emetic internally may be directed on two principles, either to diminish the action of the heart and arteries, where these are excited by keeping the patient in a constant state of nausea; secondly, as an emetic in melancholy, where full and repeated vomiting is of great service in producing a more rapid circulation in the vessels of the abdomen, and relieving the viscera, already gorged with blood. The former application of the remedy appears most applicable to mania; the second to monomania, or melancholy. "Tartar emetic, in doses of one or two grains, given internally every hour, is worth noticing in patients who are subject to violent paroxysms, particularly those who have increased vascular excitement, with great restlessness. The patient generally complains of nausea without vomiting, becomes languid and quiet, rests better, appetite improves, and in a few days is trusted out of confinement."

In addition to these means of diminishing sensibility and irritability, others have been sought for from the class of sedative medicine, and of course from the use of the greatest blessing ever accorded to mankind—opium. The opinions with regard to the use of opium in mania are very various, but may, I think, easily be explained. Where vascular excitement exists, together with increased sensibility of the brain, the restlessness is increased by the administration of opium; where perverted perception arises from disorder of the functions of the abdominal viscera, the constipating effect of this medicine, and the manner in which it acts to diminish all the secretions, obviously renders it hurtful; and it must be added, that in some constitutions it is poison. Hence it is far from generally useful even in the cases in which it is indicated; and as it is often the case that physicians neglect medicines (after a time) which do not entirely fulfil their expectations, so occasionally this medicine has been unduly estimated, and immediately afterwards, as improperly neglected. In some instances mistaken pathology has lent its aid to undervalue the medicine, and one is forced occasionally to listen to theoretic doubts about its efficacy in cases of great and urgent irritability from the prevalent opinion that all disease arises from inflammation, acute or subacute, and the corresponding error that opium necessarily occasions congestion in the brain. There is no question that where opium is not contra-indicated from peculiarity in the individual, or the presence of another disease to which it is ill-adapted, it is of all others the remedy to diminish the sensibility and irritability of the system, to make the wretched forget his grief, the ruined his poverty, and even the criminal the mental retribution of his wickedness: and in the same way it often removes from the imagination of the maniac his supposed iniquities. Still partly the real, and partly the theoretical objections to this remedy, have caused physicians to seek for some substitute from the class of sedatives which would exhaust that increased sensibility which magnified a hundred fold the objects presented to it.

It is now some years since the French chemists announced to the world that opium contained two substances on which its peculiar properties depended. Narcotine, believed to possess the stimulant, and an alkaline substance, united with a peculiar acid, existing in opium under the name of meconate of Morphia, the alkaline body, morphia, being believed to possess all the soothing effects of opium without its stimulant properties. The morphia was found to unite in preference with the acetic acid, and under this form of acetate of morphia to be more soluble in water. It is remarkable that the combination of opium with vinegar has been particularly approved of by the celebrated Van Swieten* for the cure of this disease, and that vinegar, for what reason I cannot discover, has been considered very efficacious in the treatment of mania. Since the discovery of the French chemists, morphia has been much employed; and if I may believe my own observation, and that of persons of very extensive experience in maniacal diseases, with most complete success. I have repeatedly administered it in disease where opium was ineffectual from the headache and vomiting which accompanied its use, with the most decided benefit. I subjoin the following case, which recently occurred, and which proves the benefit of this medicine:—

A gentleman was attacked, some weeks after a most severe cough and cold, with great difficulty of breathing, attended with the impossibility of lying down at night, together with intermitting pulse and swellings of the legs, the heart and great vessels having become enlarged and dilated after the inflammation. The principal inconvenience in this case arose from the want of sleep, the patient never having been able to lie down during twenty-two nights. Opium, of course, was resorted to, and in very large doses, but the distress produced was so great that the patient preferred his sleepless nights to the dreams and scanty rest produced by this means. The patient came to London, and I recommended him the acetate of morphia, in the dose of three-quarters of a grain. He slept well, and has repeated the medicine for several weeks without its having once failed of the most complete success.

But it is to the subject of mania that this applies, and I have it in my power to lay before the College the most convincing proof of the efficacy of this medicine, an efficacy which in my mind renders it invaluable, and which I should not venture to praise so strongly had it not been corroborated by the extensive experience of the gentleman alluded to.

"We have found the acetate of morphia useful, both in the excited and the low form of insanity. We have also found it useful in cases of fixed delusions, but not of any great standing, and more useful in the low than the excited form of the disease. Of five cases of melancholy, three got well; the remaining two are certainly improving under the use of this medicine. Of five cases of excitement, two were discharged cured; one remains much improved; two received no benefit. It is necessary to observe that we have used this medicine in several cases without taking notes, and the result was similar to the two cases mentioned, that is, without benefit. It appeared to us that morphia did not produce the same good effect in excited as in other cases, unless there was an occasional interval of reason. In the cases mentioned we have commenced with a fourth, and have not found it necessary to exceed half a grain. At present we have a patient taking half a grain dose every night with decided advantage, and we think the case very interesting, and proving the extraordinary effect of this medicine in cases of melancholy. A woman, of the age of 36, the mother of four children, was attacked with depression of spirits while pregnant of her last child. She did not feel the attack before she quickened, but immediately after she had a strong desire to destroy herself and children. This continued during pregnancy. After she was delivered she became worse, and attempted to commit suicide several times, and described her feelings, which is not common in such cases. She continued in this state, not fit to be trusted without a strict watch. She was sent here about two years ago; and what is extraordinary in her case is, that about noon all the feelings of desire of self-destruction left her. This occurred within the last three months, from which time they have remained the whole of the day. Various means were tried without effect. Our first idea, from the regularity of the attack was, to treat her as an intermittent, which failed. About a fortnight ago we gave her the morphia, beginning with a fourth of a grain, and gradually increasing it to half a grain; after taking the second dose, one-third of a grain. She slept all night; in the morning was cheerful, without feeling the propensity to destroy herself. The third day she had a return, which lasted until noon; the dose was then increased to half a grain. The fourth morning she had not any return, and continued well until the fifth day after the half-grain dose was given, when she had a return from five o'clock in the morning until nine, a paroxysm three hours shorter than any of the preceding. She is now free from any feeling to destroy herself.

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* Van Swieten says, that an accidental case first brought into notice the efficacy of this combination. A girl, who was maniacal, swallowed by mistake a scruple of opium mixed with vinegar: this mistake produced her cure.

"The following is a case of the excited kind, in which this remedy was employed with advantage.

"A. R. æt. 36, was admitted in February 1831, in a very high state of nervous excitement; she was a widow, and mother of four children. When admitted she was much excited, and constantly talking. Tongue dry; pulse very quick; skin moist. She was excited to such a degree that she tore the jacket and clothes to ribbons; refused her food; and would swallow nothing without force. She was ordered a pint of porter daily, with beef-tea and arrow-root. This diet was considered necessary, because if so much excitement continued without support she would fall into a state of collapse, and die. All our efforts being unavailing in giving her food, we determined to try the morphia. The first night it had not the least effect; she was noisy, screaming until morning; on the following day refused her food, and the excitement was unabated; we got the porter and arrow-root swallowed with some difficulty; the morphia was increased to half a grain; did not make any noise during the night, and appeared to be drowsy in the morning; but when she was spoken to answered in a very incoherent way, and the excitement continued; the porter and beef-tea was given with less trouble; the medicine repeated; slept well during the night; appeared, on questioning her in the morning, to have a slight return of reason, such as to inquire where she was; took her food better; tongue moist; pulse not so quick, and bowels open. Ordered two pints of porter, beef-tea, and arrow-root, as usual. Medicine repeated at night; slept very well; more rational; began to cry; took her food much better; drank the porter, and appeared to relish it; the medicine was repeated every night until 6th of March, when she appeared perfectly well; the morphia was discontinued; she employed herself, and was discharged 14th April cured."

The extract of the hyoscyamus niger, given in full doses, enjoys likewise on the continent of Europe a great reputation: it is the sedative which is preferred, and the fact of its being a mild purgative, has caused its employment by many practitioners in this country. It is believed to be less stimulating than opium; perhaps it is so; I am sure it is far less effectual as a sedative. The only English physician of great reputation who has spoken of its efficacy in diminishing the excitement of the brain in mania is Dr. Fothergill, who was of opinion he had cured puerperal insanity by this medicine; the dose was five grains, three or four times daily.

The most powerful of all the class of vegetable sedatives is undoubtedly the belladonna. It is not to be wondered at, therefore, that it has been recommended for diminishing the sensibility and irritability of the brain in mania: it is not an opiate, but diminishes pain. This is the best explanation I can give of its effect. I have used it extensively in St. George's Hospital; and, as far as my practice would permit, in private life, and often with the most marked good effect. The following is one of the cases in which its alleviating powers were most marked:—A general officer was attacked, about fourteen years ago, with the painful affection of the second branch of the fifth pair of nerves—tic doloureux. He underwent various remedies from the recommendation of the most celebrated physicians in London. The disease yielded to very large doses of belladonna, and it never reappeared for ten years. At the expiration of this time he had an apoplectic seizure, which left him paralytic of the right side, very imperfect condition of speech, and frequent convulsive and painful affections of the nerves, sometimes choking, and other times startings of the nerves of the affected limb, and occasionally the return of the tic doloureux. It is impossible to conceive more acute pain than is experienced by the patient at these times. He always (and I have witnessed at least ten such paroxysms) finds relief in the extract of belladonna. Half a grain is administered every four hours; and always by the time the third dose has had time to operate, the pains and spasmodic twitches disappear. It might be supposed that the pain terminated of itself, or that the spasmodic affection ceased naturally at this time; but distressed at the weak state in which the belladonna leaves him, the patient has once or twice refused to take it, and he has suffered intense pain much longer than he otherwise would have done, being obliged to resort to the remedy at last.

Greding speaks highly of this medicine in epilepsy; and in one or two cases in the hospital it has appeared to have rendered the intervals much longer, but I cannot lay claim to a single cure. It appears to me well worth the trial of physicians in mania, especially that which arises from moral causes, and is attended with pain and increased sensibility of the brain.

Various other substances from the class of sedatives have been adopted and relinquished alternately. The most powerful sedative with which I am acquainted—hydrocyanic acid, has not, I believe, been used in this country. The unhappy consequences which followed its exhibition in Paris would deter physicians from any but the most cautious experiments with this medicine. It is, however, in my opinion, from this class of medicines that the greatest improvement will be derived in the treatment of disease of the mind. It requires that practical physicians should turn their attention to it; first discarding the idea which is hostile to all improvement—either that mania is incurable, or that it always, or even most frequently, depends on organic disease of the brain.

There is another powerful medicine whose efficacy cannot be explained in our present state of science—arsenic. It appears to alter the sensibility and irritability of the brain, and has long been known as very efficacious in intermittent diseases. I have used it several times, with great benefit, in cases of chronic pain in the head, trembling and sleeplessness, nearly allied to mania. A woman, æt. 40, (servant to a lady in the Regent's Park,) was attacked with constant pain in the head, confusion, and giddiness, attended at times with loud noises, which she described like breaking stones; at other times images presented themselves to her mind, and her nights were entirely sleepless; pulse quick and weak. This condition continued for months, during which time I employed every means in my power for her relief; at length I resorted to the employment of arsenic. The patient took ten minims of the liquor arsenicalis twice daily, subsequently increased to the same quantity three times in the day. In about a week very great improvement had taken place; the pains had diminished, and the nights were tranquil. In a fortnight her ailments entirely left her.

The next was the case of John Graham, æt. 30, admitted into St. George's Hospital, under my care, November 24th, 1830. The report on admission was as follows:—Ill since June last with pain in the forehead, diverging to each side, very excruciating; and reported to feel as if the head was forced or burst open. The pains are worse at night, so as entirely to prevent sleep. The pupils are dilated and the conjunctivæ injected with blood; sense of smell increased in acuteness; bowels constipated; pulse 100, very weak; skin cold.

The bowels were ordered to be kept open, and sedative and antispasmodic medicines were employed without effect; on the 6th of December he was ordered five minims of liquor arsenicalis twice in the day.

11th.—The pains were reported greatly relieved.

13th.—The pupils contracted naturally; the pain and pressure in the head nearly gone, except when he lies down.

16th.—The medicine increased to five minims thrice daily: from this time the pains entirely disappeared. He remained in the house a fortnight, to see if any relapse took place, when he was dismissed free from ailment.

It has occurred to me to see several similar cases; but these, perhaps, are as good examples as I could select, of this peculiar effect in the medicine; and I cannot but think it might, in proper hands, be very beneficially employed in some maniacal cases. Although I am quite aware that this medicine is similarly employed by many physicians in practice, yet I cannot learn that it has hitherto been tried in any of the numerous similar, and more severe cases, which are found in lunatic asylums.

In female cases, and especially those kinds which approach nearest to hysteria, the fœtid gums and antispasmodic medicines have been principally used, and there can be no doubt that such means may tend very materially, by proper administration, towards a cure; but of all the diffusible stimulants which have been recommended, camphor has had a peculiar reputation. This was the remedy on which Dr. Perfect relied in the treatment of numerous cases of insanity. In 108 cases he conceives the patient derived essential benefit from it, and he administered it in the solid form. The following is one of these cases:—

Mrs. S. B., a married woman, became melancholic. Her complexion was pale, eyes red, tongue dry, pulse small, hard, and irregular. After bleeding and vomiting, Dr. Perfect ordered two scruples of camphor to be taken morning and evening. An eruption came out after this treatment all over the body. The catamenia returned; nitre was added to the camphor, and the patient recovered completely.

Purgative medicines have been from all antiquity greatly praised in the treatment, and above all the helleborus niger. In our present state of knowledge there are many purgative medicines far preferable to this, although it is consecrated by centuries of experience. Moderate, and frequently-repeated purging appear, as far as I can collect, to be very useful in monomania. One of the most advantageous discoveries in the modern art of medicine, has been the oil of croton, because there is scarcely any condition in which it cannot be administered, and very large doses of other medicines are required to fulfil the indication. I have repeatedly had occasion to administer this medicine in diseases of the brain, after blood-letting, in acute cases and in chronic cases, alternately with sedatives, and with the greatest possible good effect. Dr. Abercromby affords likewise his powerful testimony in favour of the derivative, as well as purgative, effects of this medicine. A drop may advantageously be administered in a little jelly, which covers the acrid taste; when made into pills it appears to lose partially its beneficial effects. Messrs. Beverley and Phillips have afforded me the following information on the curative treatment by purgatives:—" Purgatives are, we think, of the greatest importance. We have not tried long courses of drastic purgatives; we have tried courses of mild purgatives in melancholy with advantage. There appears almost always a want of energy in the constitution of melancholic patients; the circulation is languid; the absorbent system sluggish; there is a furred tongue and swelling of the legs,—in those cases we have found a course of laxative medicine, with gentle exercise, of great service. The exercise is generally enforced, as they will not comply without it, frequently requiring two assistants to take them by the arms, and oblige them to walk. The patient's health by this means improves, and the mind by other medicine, particularly the morphia, which we have given at this period with advantage."

The oleum terebinthinæ, a medicine combining purgative, anti-spasmodic, and stimulant properties, has been much employed in cases of hysteria, connected with epilepsy; it has been recommended by Dr. Latham, senior, Dr. Thomas Young, and Dr. Percival; and in the latter cases especially, by Dr. Pritchard, of Bristol. Nevertheless, it has never become an ordinary practice. I lately had the opportunity of seeing very decided effects from it, in a case—an out-patient of St. George's Hospital, æt. 15. The patient had never menstruated, and was in a state very nearly approaching to idiotcy, being unable to speak or take notice, except occasionally when struck with the appearance of some glittering ornament, when she expressed her surprise by remarkable and violent gestures. The pulse was quick and weak; great pain in the head; skin cold. She was ordered two drachms of ol. terebinth. and the same quantity of castor oil every morning, and in about a fortnight she recovered the use of her senses; but she remains in a weakly state of body, and the catamenia have not occurred. She is now able to take steel. I am inclined to think that great benefit may be derived from the employment of this medicine in similar cases, and such are not very uncommon.

The belief that cases of insanity always depend on organic causes, or a state of acute or subacute inflammation of the brain, has not only led to the erroneous treatment of many of these forms of disease, by venesection, but has suggested an antiphlogistic diet. We have, I think, the best evidence from the greatest experience in France and England, of the evil consequent on such practice. M. Pinel draws a terrible picture of the afflicting consequences of the short allowance on which the patients of the Bicêtre were placed in 1796; and it has been seen that a full diet, and even considerable quantities of stimulants, are often productive of the utmost advantage in what are termed high cases. In the cases arising from child-bed, or during nursing, Dr. Gooch has very clearly demonstrated that these occur either in consequence of, or during, an exhausting process, and are relieved by tonic and antispasmodic medicines and restorative diet.

I have now spoken cursorily of the principal means of cure, from medicine, which may be adapted to mental disease; any improvement in treatment, and consequently any collection of facts, to serve as guides to future observers, must obviously depend on the judgment exercised in discriminating the causes, both moral and physical. The small bleedings, for instance, which would be useful in mania depending on chronic disease of the heart, and steatomatous or bony depositions in the smaller arteries of the brain, would be a fatal error in those frequent cases connected with hysteria; and the large evacuations and antiphlogistic treatment imperatively called for in inflammation of the membranes of the brain, would change mania into furious madness, by increasing the already excited morbid sensibility of the brain and nervous system. I hope, little as I have been able to say upon these subjects, that it is enough to prove that it is to the educated physician, to the man who is engaged in the constant discharge of the duties of his profession, that such cases should be made a subject of deep importance; that all his experience should be brought to bear; that all his faculties and observation should be concentrated in improving this portion of the medical art; not by studying it exclusively, but in conjunction with the other diseases of the human body, and thus removing a great source of quackery and imposture. The educated physician is too often called upon to stand between the public and its prejudices: this must be done, not by deserting our colours, but by shewing that, with every desire to serve the public, neither rewards nor honours, nor fleeting popularity, can make us lend our countenance even to innocent imposture; and that that independence which is the distinguishing mark of an honourable profession, will support us through difficulties, and vindicate the integrity with which we practise that profession. If we carefully investigate disease, and neither resign that interesting and useful study to artful and designing persons, nor suffer ourselves to be overcome by the momentary prejudices of the world; all experience has shewn that medical practitioners so acting have received, and will continue to receive, the respect and homage of society.

CASE OF EPHIDROSIS IDIOPATHICA.

To the Editor of the London Medical Gazette.

Sir,

I know not whether the opinion is founded in correct observation, but I am disposed to believe that there is something very peculiar in the constitution of the present year—every variety of disease appears to present unusual phenomena. Fevers, if inflammatory, quickly assume the typhoid character; or from their commencement give strong evidence of their malignant tendency. The active stage of inflammatory diseases appears to pass as a shadow, and the disturbance which a mere attack of diarrhœa will produce in the vascular and nervous systems but ill accords with the usual and observed effects of that disease. How far these circumstances may be associated with the pestilential state of atmosphere, which at present is depopulating many parts of Europe, it may be difficult to decide, and it remains a point of no trifling moment for our consideration whether these phenomena are to be considered as precursors of future and more fatal mischief, or are to be regarded as modifications of the same calamity. With such impressions, it is to be supposed that every unusual occurrence will be adduced in support of the belief; nor can I think that the following case, which I offer for insertion in your journal, in any way contradicts the opinion.

A lady, whose mind and body had been harassed by unremitted attendance on a sick and valued relative until his death, through the day subsequent to that event was troubled with repeated attacks of cramp in the legs, and with general feelings of indisposition. Soon after going to bed she was seized with rigor. No perceptible hot stage followed this state of collapse; but a most profuse perspiration within the space of half an hour covered the whole body her back and limbs became painful, with pulsating pains in the head. I saw her for the first time (later than I intended) the evening subsequent to the attack. The pulse was ranging at 130; the countenance very anxious; the perspiration literally pouring from the face, the skin of which was exquisitely tender (and still remains so) from the constant use of the handkerchief; and it had been necessary to keep a napkin under the chin to receive the perspiration as it ran down the face; every part of her body was in the same state; she had had no sleep: there was great sense of weakness in the abdomen, with unusual and most distressing flatulency. I ordered the following medicines, directing that she should be kept moderately cool, and no warm fluids to be given.

℞ Confect. Arom. ʒss.
Tr. Hyos. ʒss.
Sp. Ether, Nit. ʒss.
Tr. Card. c. ʒj.
Aq. Puræ, ʒx. M. fiat haust. stat. sumend et post hor. quatuor repet.

Early the next morning I found her with the pulse reduced to 100 beats in the minute; the perspiration had considerably decreased since four o'clock in the morning, though up to which time it had continued with unabated violence; head still painful; great uneasiness in the back; the flatulent state of abdomen greatly relieved.

℞ Extract Conii, gr. xvj.
Tr. Cardam, comp. ℥ss.
Inf. Rosæ, ℥iij.
Aq. Cinn. ℥iiss. M. fiat. mist. cujus. capt. 4tam part. 4tis horis.

In the evening I found her with the skin in a natural state; pulse 80; no pain in the head or back, but excessively weak. I continued the sedative, and ordered a mild aperient for the morning. On my next visit I found she had passed a comfortable night; skin in a natural state; pulse regular; I continued the sedative every six hours. Two days subsequent to this visit she complained of nothing but debility.

I remain, sir,
Your obedient servant,
Henry George.

22, Lower Phillimore-Place,
Kensington, Oct. 21st.

CHOLERA TREATED WITH CAJEPUT OIL.

To the Editor of the London Medical Gazette.

Sir,

If you consider the following case of English cholera, which was clearly attributable to improper ingesta, and in which the oil of cajeput was administered externally and internally, with apparent benefit, sufficiently interesting for insertion in your journal, it is at your service.

I remain, yours, &c.
R. R. Robinson,
Surgeon.

13, Cooper's Row, Trinity Square,
October 24, 1831.

N. Brenan, æt. 60, a thin and feeble man, of bad constitution, and rather intemperate habits, who had been sometimes subject to mucous diarrhœa, was purged more than usual on the 13th September, notwithstanding which he eat pork sausages for dinner, and drank porter at 2 p. m. and at 11 p. m. took a pint of ale, and went to bed, as he thought well, about 12. He awoke, however, about two the following morning, with violent cramps in calves of both legs, vomited frequently, and was purged three times. The pain increasing, I was sent for between five and six: he was then suffering severely from cramps in legs; he was very languid, his countenance anxious; there was no pain in abdomen, where he could bear to be pressed; skin warm and perspiring, tongue coated, great thirst, pulse soft and regular, between 80 and 90.

Pulv. Rhei gr. xxv. Pulv. Cinnam. co. gr. v. statim. Applicentur frictiones cruribus.

12, noon.—Contrary to orders, he had taken port wine and water; had been constantly sick; the fluid vomited appeared to consist chiefly of mucous tinged by the wine; cramps have been frequent and severe, but have always been diminished in duration by friction.

Cal. gr. iij. Opii gr. j. statim.

6, p. m.—Constantly sick; no motion; cramps frequent and severe.

Ol. Cajeput. gtts. xxx. Tinct. Opii gtts. xv. statim ss. The same to be used as an embrocation.

10, p. m.—The cajeput immediately returned, and he has been constantly sick since; has vomited altogether since last report about ℥vj. of clay-coloured fluid, strongly impregnated with the oil; cramps, which before invariably followed each attempt to vomit, have occurred only five times since last report, and have not been so severe; thirst great, tongue much furred, skin warm and perspiring, pulse soft, 90; no motion.

Pulv. Opii gr. j.—Repetatur Linim.

Sept. 15th.—Slept well, but little vomiting; cramps occasionally on moving, not otherwise; less thirst; urine scanty, but high coloured; tongue coated; pulse soft, regular, 84.

Ol. Ricini ℥ss. statim.

16th.—Cramps gone; sick only once; two motions; tongue still coated; some thirst.

Cal. gr. j. Ext. Colocynth co. gr. v. o. n.
Ol. Ricin. ℥ss. p. mane.

18th.—Up, and convalescent; sickness and cramps gone; tongue clean; bowels open.

Inf. Gent. co. ℥iss. Mag. Sulph. ʒss. bis dic.

20th.—Recovered his strength rapidly, and has had no return.

Remarks.—The spasms appeared in this case to be chiefly relieved by the outward application of cajeput and laudanum as an embrocation, which I am not aware has been hitherto used in cholera.

OPIUM IN CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

It appears to me to be generally the practice to give opium in a very wrong form in cases of cholera; and as the public is at the present time under so great an apprehension of this dreadful disease coming to this country, I think it my duty to ask, whether it would not be much better to give it in its hard purified state, or to administer the acetate of morphia, for it is well known that light, according to the experiments of Vogel, has the power of decomposing the tincture, and I make no doubt that it is the common practice for the druggists to adulterate the powder. Should you think the above observations worthy of notice in your valuable Gazette, you will oblige me by inserting them.

I remain,
Your obedient humble servant,
John Greening, Surgeon.

Worcester, Bridge-Street,
Oct. 24th, 1831.

YELLOW GUM, OR GRASS TREE, OF NEW SOUTH WALES (XANTHORRHŒA.)

The Xanthorrhœa, or yellow gum tree, is found abundantly in New South Wales, where, among the colonists, it is named the grass tree, from its long, pendant, grassy foliage. It is a tree of curious growth, and I have seen some of the species of from eight to ten feet in height. From the centre of the foliage rises the straight flowering stem, which is frequently ten feet and upwards in length; it is round and very light, combining lightness and strength; it is used by the natives for making their spears, fish-gigs, &c. The scape terminates in a cylindrical spike; the flowers are white, small, and numerous; an albumen is secreted from them, which is greedily devoured by the natives, and numerous birds; it is said to be very agreeable to the taste. The capsules are triangular, and contain three flat, black seeds, in distinct cells.

It derives its name, "Yellow gum tree," from secreting a gum-resin of a bright yellow colour, and resembling in appearance the resin of the Stalagmitis Cambogioides, or gamboge tree. Externally the yellow gum has a dull appearance, but breaks with a shining bright yellow fracture, and is sometimes streaked internally with red. The resin is soluble in spirits of wine, but is insoluble in water; it is light and brittle, and is, in its natural state, without any fragrant smell; but by the action of fire it yields an aromatic odour, resembling the frankincense, for which it might be used as a substitute. It exudes spontaneously from the trunk, but only in very small globules; yet it may be melted into large masses. At the base of the tree are congeries of scales, between which the gum is found concreted in small pieces. One tree yields a considerable quantity of the gum.

This resin (more commonly known as the gum acaroides) is slightly bitter, pungent, and astringent, and has been used in dyspeptic, dysenteric, and other cases; but not, I believe, with such success as to cause it to be admitted into our materia medica.—*Mr. G. Bennett's MS. Journal.*

London, Sept. 27, 1831.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Essays on the Effects of Iodine in Scrofulous Diseases; including an Inquiry into the Mode of preparing Ioduretted Baths. Translated from the French of M. LUGOL, Physician to the Hôpital St. Louis, by W. B. O'SHAUGHNESSY, M.D. With an Appendix by the Translator.

This volume consists of a translation of three Essays published by M. Lugol, physician to the Hôpital St. Louis, of Paris; followed by an Appendix by Dr. O'Shaughnessy, containing some important additional information, which we shall lay before our readers in the sequel. Of the opinion entertained in France of these papers, some idea may be gathered from the circumstance of their having received the warm eulogies of the reporters appointed by the Académie Royale, as well as the unanimous approbation of the medical press—to say nothing of six thousand francs voted to the author by the Institute. The intelligent translator of M. Lugol's Essays attributes much of the success attending the use of iodine at the St. Louis, to the care, skill, and patience of the physician—to the period of treating the cases being unfettered by the limits as to time, which prevent investigations of this nature from being satisfactorily conducted in the hospitals of this country. The cases, indeed, are numerous and astonishing—so much so, that farther trials will be required before practitioners can become convinced that similar success, in scrofula especially, is to be hoped for among us.

M. Lugol seems to be a person of eccentric manners, and an enthusiast, affecting in his discourses the most preposterous forms of expression. Thus we are informed that he will sometimes personify his favourite medicine, and exclaim, "*O iode, iode, O mon fils! pourra-t-il se rencontrer jamais une langue capable de celebrer dignement tes vertus anti-scrofuleuses!*" And again, on pointing to a patient in whom the iodine had, in his estimation, gained a great triumph over the disease, "*Voici la bataille d'Austerlitz de l'iode!*"—

Our Gallic neighbours may, indeed, say of this high-flown style—

> Ce style figuré dont on fait vanité,
> Sort du bon caractère et de la vérité—

but to us it seems wretched bathos and downright nonsense. A knowledge of these circumstances leads us to trust more to the report of the commissioners than to the statements of M. Lugol, who, without intending to deceive, may yet mislead. Now the report is really very favourable, and the profession is indebted to Dr. O'Shaughnessy for having given, in so convenient a form, all that is valuable in the observations of the French practitioner.

The preparation of iodine which M. Lugol regarded until lately as most certain, was a solution in distilled water; observing, however, that the vehicle was never carried in quantity much beyond what was necessary in order to obtain a perfect solution; the dose being half a grain to begin with, gradually increased to two-thirds of a grain, and finally a grain, in twenty-four hours. This, it will be perceived, is greatly under the maximum doses of M. Coindet, who gave to the extent of three grains a-day. Indeed our author particularly objects to the tincture so much used by the Swiss physician, which, being administered in an aqueous vehicle, leads to the precipitation of the iodine in the solid form, and thus incurs great risk of serious injury to the stomach. A pound of water was the quantity employed by M. Lugol in making his solutions, and to these he generally added a few (twelve) grains of common salt, or, if we must use the more scientific language of our author, of chloruret of sodium. For external use he employed an ointment, having three degrees of strength: No. 1, containing, in two pounds of fresh lard, hydriodate of potass, ʒiv. and iodine, ʒiv. No. 2, hydriodate of potass, ℥v. and iodine, ℈xiv.; and No. 3, ℥v. and ℈xvi.* Occasionally, as a substitute for these ointments, was used a solution—viz. No. 1, gr. ii. of iodine, gr. iv. of hydrod. of potass, to lbj. of water; No. 2, gr. iii. and gr. vi. to lb. j.; and No. 3, gr. iv. with gr. viii. to lb. j. But upon these modes of exhibition, though much superior to what had been previously used, farther experience enabled M Lugol to improve, and he has latterly abandoned the simple aqueous solution for one made by dissolving the iodine in a solution of potass, thus:—

Ioduretted Mineral Water.

" ℞ No. 1.—Iodine, gr. ¾; Hydriodate of potass, gr. 1½; Distilled water, ℥viij.
No 2—Iodine, gr. i.; Hyd. of potass, gr. ij.; distilled water, ℥viii.
No. 3.—Iodine, gr. 1¼; Hyd. of potash, gr. ij. ½; distilled water, ℥viii."

This preparation is quite transparent, of a fine orange colour, and keeps for a considerable time; while it is so little disagreeable that children take it readily if mixed with sugar. This addition, however, it is worthy of remark, ought only to be made when it is just about to be administered, as a decomposition results if it be allowed to stand for some hours. Two thirds of No. 1, as above, are to be taken in the course of the day, to begin—and this ought to be continued for a fortnight; after which, the whole of the mixture may be taken daily.

The external effects of iodine, when applied to ulcers, were great increased local action, sense of pricking and smarting, which was often prolonged. In a few days followed an improvement in the aspect of the sores, with or without suppuration. The local action generally lessens in activity as the surfaces heal, but in rare cases it continues, however long it may be used, to produce undiminished excitation.

The internal effects of iodine vary: one of the best, though not one of the most common, is that of increasing the appetite to a great degree A more general consequence of its use is an increase in the urinary secretion: sometimes the diuresis is very considerable indeed. More than one person in three is purged by it, and in some this is attended with colic. In several cases it produced salivation. Some patients, particularly females, complained of it producing pain of the stomach; and when this happened, the inconvenience was removed by administering two or three ounces of the kina wine after the mineral water.

Such are the principal forms of exhibiting iodine, as adopted by M. Lugol;

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* No. 1, made up in smaller quantity, and with rather less iodine, may be prescribed thus:

Iodine, gr. xii.; Hydriod. of Potass, iv. scruples; fresh lard, ii. ounces. M.

and we shall conclude our notice of his part of the volume by quoting a portion of the Report upon the subject, by MM Serres, Magendie, and Dumeril.

"The author of the Memoir has carefully noticed the effects produced by the iodine on the animal economy. Applied externally, its local action has always been very sensible: it determines on the surfaces of the ulcers a feeling of smarting, accompanied with painful itchings. *This application to the diseased surfaces changes their appearance, and frequently produces as appreciable an effect as that determined by mercury on venereal ulcers.* Moreover, the mode of its action does not appear to be invariably the same: sometimes the iodine seems to melt down and resolve the tubercles, sometimes, on the contrary, it urges them on to rapid suppuration. At other times the painful sensation appears to diminish in proportion to the healing of the surfaces, an effect which is perhaps dependent on habit; nevertheless, some ulcers remain sensible while the curative process is not at all established.

"Internally administered, and always in small doses, and with the most prudent slowness, the ioduretted water constantly excites the appetite, and appears to increase the urinary and salivary secretions. Sometimes, but very rarely, it has become purgative to so considerable an extent that its use was necessarily suspended, at different intervals, from two to three days each. In other and still rarer cases, in which the solution of iodine appeared to occasion pain in the stomach, the wine of quinquina, given according to the directions of M. Coindet, in a dose of two or three ounces, put an end to the troublesome symptoms. M. Lugol, however, always declined, as much as possible, this association of remedies, in order to avoid complexity in the results of his treatment.

"Iodine, administered in this diluted form, has never caused emaciation nor produced the expectoration of blood or other accidents, which many have imputed to its action.

"From the contents of the first Memoir, it appears that M. Lugol has treated with iodine alone, in seventeen months, at the Hôpital St. Louis, 109 scrofulous patients, of which sixty-one were males and forty-eight females.

"That at the close of last year, thirty-nine (twenty-nine males, ten females) were still under treatment.

"That thirty (seventeen males, thirteen females) had quitted the hospital with marked improvement.

"That in four cases (two males, two females) the treatment was ineffectual.

"Finally, that thirty-six (thirteen males, twenty-three females) were discharged completely cured.

"The author concludes, from all the facts he has collected, and the researches he has conducted, that iodine deserves to be considered as the most efficacious remedy in scrofulous diseases, since it has constantly arrested their progress, or at least exercised a salutary action in the treatment of all tubercular tumors, even when it has not evidently accomplished their cure. He therefore believes that the introduction of this remedy into medicine is one of the most valuable acquisitions the healing art has made in modern times.

"We shall then confine ourselves to say, that after having made ourselves acquainted with the facts cited in the memoir, we have been enabled to confirm the evident action of the remedy; and that we believe M. Lugol to have effected a work of great utility by availing himself of the facilities afforded by his situation, in seeking for a remedy for a disease hitherto so deplorable and desperate. We consequently propose to the Academy to encourage this physician to persevere in the researches which he has hitherto pursued with so much zeal and sagacity."

An appendix by the translator contains some farther evidence in favour of iodine, from the practice of various other physicians and surgeons in France; but the part of it which appears to us most valuable is the author's own, namely, a clear and succinct account of the chemical properties of iodine and hydriodate of potass, with their adulterations, &c.

"*Adulterations, &c. of Iodine.* — From the high price at which iodine was sold on its first introduction into medicine, a great inducement was afforded for its adulteration, and accordingly, with a clumsiness and ignorance only equalled by their shameful cupidity, some druggists were in the habit of mixing the iodine with a liberal per-centage of charcoal, the protoxide of iron, plumbago, or the carburet of that metal.

"Although the price of the article has now *fallen* to *1s 6d.* the ounce, with *many* eminent and honourable wholesale houses, still, perhaps, from the difficulty of abandoning old habits, some dealers yet practise the admixture above-mentioned. The fraud may be at once discovered by heating ten grains (accurately weighed) of the suspected sample in a glass tube by a spirit lamp, and continuing the heat till no violet vapour is evolved; if any residuum remain, it indicates an adulteration, the extent of which may be ascertained by the weight of the fixed matter.

"Perhaps an easier process is by throwing the suspected specimen into strong alcohol in sufficient quantity. All the foreign ingredients above enumerated remain undissolved.

"By either of these methods the mixture of charcoal, the protoxide of iron, or the carburet of iron, may be discovered. I should not omit to add, that I have met with one specimen much more artfully and scientifically adulterated, and in which the fraudulent ingredients were, at the same time, soluble in alcohol, and volatilised by heat. I forbear, however, alluding to this mixture more minutely, because the fraud is comparatively so difficult of detection, that describing it would, perhaps, only lead to its more general practice. I should add, that in the last edition of Gray's Supplement to the Pharmacopœias, the editor states that iodine is sold at the absurdly extravagant price of 1*l.* 5*s.* per ounce."

"*Adulteration of Hydriodate of Potash.*—The falsifications of this salt are so extensive and peculiar that the practitioner, who is not aware of their existence and the mode of their detection, need never attempt to place a patient under Lugol's treatment with any reasonable prospect of success. Had the adulterations been contrived for the sole and express purpose of destroying any anti-scrofulous virtues which iodine may possess, the method could not have been more ingeniously devised.

"It was long known that hydriodate of potash was apt, during its preparation, according to the method of boiling with the alkali, to be mixed with a slight excess of potash, probably from the weakness of the spirit used to dissolve away the hydriodate from the iodate. Little attention, however, was paid to the subject until Mr. Pereira published a paper in the London Medical and Physical Journal, in which he described additional adulterations of much moment. A short time before Mr. Pereira's paper appeared, I had observed the same facts in Scotland, and demonstrated them to my pupils.

"From these conjoint observations it appears that much of the hydriodate of potash at present vended by druggists, &c. contains large quantities of the muriate of soda, carbonate of potash, traces of the sulphate of soda, and other salts in minute quantities. One specimen of which I made a quantitative analysis, contained 61 per cent. of the carbonates alone; and this examination was fully confirmed by Dr. Cristison, who also investigated the subject.

"The pernicious effects thus exerted on the therapeutical applications of iodine in the internal and external treatment of scrofula, goitre, syphilis, &c. may at once be understood by consulting the subjoined comparative statement of the chemical properties of the pure and impure solutions of this salt.

PURE.	IMPURE.
"1. Dissolves iodine in the cold, forming an active ioduretted dydriodate of potash for internal use.	"1. Does not dissolve iodine in the cold.
"2. When warmed with iodine and diluted with water forms an active bath, which excites powerful local action.	"2. When warmed with free iodine, converts it into the hydriodate of potash, a compound proved by Lugol to be nearly inert as a local or general bath.
"3. Affords by double decomposition pure iodurets of lead or mercury for internal or external use.	"3. Affords by double decomposition carbonates, chlorides, and sulphates of lead or mercury compounds either inert or opposed in their action to the iodurets of these metals.
Conclusion.	*Conclusion.*
When pure, a valuable medicinal and pharmaceutical agent.	"Impure, possessed of no medicinal or pharmaceutical value whatever.

"In order to detect the presence of the carbonates and muriates of soda, or potash, the best method is to add a solution of the nitrate of silver to a solution of the suspected hydriodate; if impure, a white precipitate of the carbonate, chloride, and ioduret of silver, subsides. Filter, and when dry project the precipitate into ammonia, which dissolves the chloride and carbonate of silver, and the ioduret remains; of this ioduret of silver 220 parts correspond to 137.42 of the hydriodate of potash; the amount of the adulteration can, therefore, be readily ascertained by comparing the weight of the hydriodate of potash corresponding to the ioduret of lead obtained, with that of the weight of the specimen before the experiment.

"As a trial test to detect these adulterations, a little acetate of lead should be added to a solution of the suspected specimen; if impure, a copious *white* precipitate is formed; if pure, the deposit is a fine *yellow* colour of a crystalline texture, and perfectly soluble in hot water, from which it crystallises on cooling in hexagonal scales of a golden splendour.

"I trust I have now said nearly enough to guard the public and the medical practitioner from the mortifying effects of this shameful practice. All M. Lugol's experience has been placed in vain before the community, unless this sophistication be diligently sought for, and its authors efficiently punished*.

"As it is more than probable that the management and preparation of iodurretted baths will fall into the hands of the proprietors of large bathing concerns, it is highly desirable that the patient should possess the means of ascertaining whether the baths are properly prepared—that is, whether they contain free iodine dissolved in pure hydriodate of potash. For this purpose the bather may procure a specimen of the water, and add to it, when cold, a cold solution of starch in excess: if free iodine be present, the blue colour will be produced, and the filtered colourless liquid will yield a *yellow* precipitate with the acetate of lead. As the patient's sensations will be his principal guide to the quantity of free iodine necessary to excite the cutaneous action of the remedy, I do not feel it necessary to describe here the mode of analysis by which the quantities of the respective ingredients in a given quantity of water may be ascertained. The analysis is, moreover, so complicated that I could scarcely hope to render it intelligible but to practised chemists, who, of course, do not require the information."

A Practical Guide to Operations on the Teeth. To which is prefixed a Historical Sketch of the Rise and Progress of Dental Surgery. By JAMES SNELL, Dentist, Member of the Royal College of Surgeons, &c. &c.

THE first fifty-five pages of this work are devoted to a history of the healing art, rather misplaced, in our humble opinion; but that is matter of taste. "It is idle (says our author) to inquire with whom, or in what country, medicine took its rise;" and we think it is idle to spend the fourth part of a thin volume, which professes to be a "practical guide to operations on the teeth," in discoursing about Confucius, Zoroaster, Hermes Trismegistus, and other, no doubt, very respectable personages, but who have heretofore been little heard of as dentists.

Those of our readers who practise in the department of surgery on which Mr. Snell's essay treats, will find some useful instructions on the mode of extracting teeth, and some strong objections to the plan lately so much recommended of excision, together with a description of some ingenious mechanical contrivances for facilitating the performance of the various operations. The only subjects of more general interest are referred to in the following short quotation on tooth-brushes and tooth-powder.

"The various opinions which are held relative to the shape and texture of brushes, would lead us to suppose that the matter was a much more important one than it really is. There are even patent tooth-brushes. A late author insists upon the necessity of hard brushes, and states that he has tried for some years to do himself all the injury he possibly could by using a hard brush, and finds that he can inflict none. I confess I am not yet acquainted with any necessity for using a hard brush, presuming upon its doing no injury:

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* "When the hydriodate of potash is found to be adulterated, Lugol's aqueous solutions (see formulæ in Part I.) should be used in the internal treatment."

the teeth do not require it. A brush too hard is as useless, from having no elasticity, as a very soft one, from its having no firmness. There is a medium between the two, which should be chosen. A brush for the anterior part of the teeth should have its bristles cut lower in the centre than at each end, so that when looked at sideways, the bristles should form the segment of a large circle, the external row inclining outwards a little. In texture it should be as elastic as is consistent with a proper degree of firmness, where the gums are not diseased and the teeth not loose; but when these defects exist, a softer brush should be used. Under any circumstances, however, the hairs should not be so soft as to loose their elasticity by use. The brush should be used as much as possible in a perpendicular direction, not as it regards the brush but the teeth. Brushing the gums until they bleed is recommended by a late author. I see no necessity for this rude method of phlebotomizing; notwithstanding I believe that the moderate use of the brush upon the gums is highly beneficial.

"As the grinding surfaces of the molar teeth are apt to retain matter in their indentations which might be injurious, a brush rather larger than that above-mentioned should be used for the purpose of cleaning them; the bristles should be much stronger, although by no means so strong as some which are sold for this purpose, as it is totally impossible that they can act but upon the projecting points of the teeth. The object is to have the bristles of a sufficient length, strength, and elasticity, that their points may insinuate themselves in the little indentations of the grinding surface. A hard brush, with short bristles, such as are sold for this purpose, is consequently altogether unsuitable. The back part of the teeth are best kept clean with a small square brush, with bristles rather long and elastic, and left highest in the centre, the corners of the square being cut off. The handle of the brush should be bent a little below where the bristles terminate. A flat brush also, with one row of bristles, or a pointed one, the bristles being inserted like a camel hair pencil, is useful for inserting between the teeth, to remove as soon as deposited the tartar which, without care, is very apt to accumulate in that situation. With these brushes, if well chosen as to their shape and texture, the teeth may be kept clean in every part."

"Bearing in mind that the use of a tooth-powder is to assist the action of the brush, in taking off any extraneous body which may adhere to the teeth, and for which purpose the brush alone is frequently found insufficient, those substances should be chosen which can be levigated finely, and which will not have a tendency to wear away the enamel. The testaceous powders, therefore, in combination with some alkaline, should be preferred. Charcoal is highly spoken of. There are, however, two strong objections to it. First, it cuts through hard substances rapidly, consequently will destroy the enamel; and secondly, it is a very dirty application to the mouth, and others much less so may be readily found. Where, however, it is used, that which is made from the areca nut or from the vine shrub is to be preferred. This, mixed with creta, may be used without injury, where there is a *penchant* for this substance. The best tooth-powders, however, in my opinion, are composed of such ingredients as the following formula:—

"℞ Pulv. Cretæ. Prep. ʒiij.
Saponis Hispanici, ʒj.
Pulv. Rad. Iris. Flor. 3ij.
Sodæ Carbon. 3j. Misce."

MEDICAL GAZETTE.

Saturday, October 29, 1831.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

CAN CHOLERA BE EXCLUDED OR CONFINED?

EVEN before the words we now write have come before our readers, facts may have given a practical answer to our question; but though we apprehend not so speedy a solution, our deliberate opinion is, that protection by exclusion, though within the range of possibility, and therefore to be attempted, is no longer within the pale of probable events. The destructive malady has now traversed nearly an eighth part of the earth's surface—it has gone on heedless of man's contrivances—it has baffled every human precaution—it has come even from the banks of the Ganges, and approaches the *divisos orbe Britannos*; and now that it is nearly upon our shores, it scarcely remains for us to say, "Hitherto shalt thou come, and no further!"

We must confess we have long since lost all confidence in the continental system of quarantine and cordons—so far as they might be supposed to protect the nations of Europe against the invader. Russia tried them amply, and to its cost; and if any care was neglected, Prussia guarded cautiously against such neglect. Another powerful nation—Austria, hoping to profit by the experience of both, followed their example in an amended form. Yet what has been the result? Why, Berlin and Vienna—which are even still struggling with the monster—were after all unable to protect themselves from his fatal ingress. Nay, the cordons of Vienna nearly occasioned a civil war; and at Berlin, where they were only maintained by the rigour of strong despotism, the King of Prussia was soon convinced of his mistake, and hastened to repeal those "sanitary laws" which experience taught him were so fruitless, so expensive, so ruinous to commerce, and so tyrannical. In his proclamation (dated from Charlottenburgh, Sept. 6th, 1831) he complains that, "the Asiatic cholera had penetrated into his dominions in spite of measures the most rigorous, precautions the most active, and vigilance the most sustained, which had all proved useless and unsuccessful in averting or even checking its progress." He adds, "and now, since the disease is in our kingdom, and the eyes of government have been opened by experience, I have ordered the rules, hitherto prevailing, to undergo a complete revision, and a conscientious examination as to all the circumstances worthy of being entertained, in order to arrive at a proper modification of those rules, partly from an acquaintance with the most approved modes of treating the disease, and partly from the exigencies of the occasion. The rigorous measures of isolation by cordons, established on the frontiers, and in the interior of the country, have hitherto acted *unfavourably* on the industrious habits of my people, and threaten, if they be maintained much longer, to destroy the comforts of numerous families, and in short to become *more ruinous to the country than the malady itself*." The Board of Health, at Berlin, was accordingly enjoined to publish forthwith such changes in the "rules" as their experience had suggested to be beneficial; while, meantime, the military cordons were almost everywhere discontinued, and such other restrictions as originated in the (so called) "sanitary laws," were considerably modified.

It is very remarkable, that at the very time the King of Prussia was taking these measures, dictated by his sad experience, and lopping off the heavy expense of cordons, the French minister was procuring a grant of a million from the Chambers to try all the same experiments over again; he was urging even then the salutary effects of *cordons sanitaires*, and insisting that sufficient precaution had not been taken wherever the disease had got admission. He took Prussia for his example. "The Russians, Poles, Hungarians, and Wallachians," said M. d'Argout, "have fallen victims to their own negligence—they have shewn the most culpable contempt for the sanitary measures prescribed by the contagionists; while the *Prussians*, with persevering and most praiseworthy rigour, have followed up the ordinances of their government—they have spared no sacrifice—they have omitted no precaution—they have resisted the invasion of the disease—and behold their *most signal success!*" Even then, as we have said, the disease was settled in Berlin, and the king was abolishing the cordons. But the knowledge of this fact came too late, and the million was already voted.

To do the French justice, however, all their attention is not now turned to frontier regulations; they seem to be rather intent upon the best method of grappling with the foe when he gets in amongst them: and they have profited considerably by the example of their continental neighbours.

And shall not we, then, weigh well the consequences before we adopt the strict enforcement of similar regulations? Shall we not consider cautiously the certain injury to trade, and the extremely doubtful advantages to the public health? For our own parts *we* have spoken; keep it out, if our fleets on the broad seas can accomplish this; but once among us, we have little faith in any attempt to lock it up, while the whole economy of society must be destroyed in the very attempt.

But if we cannot shut out this fierce intruder, it remains to be considered what we are to do with him when amongst us. Can we hold him in durance? Can we make him our prisoner, and strangle him in his dungeon? Alas! we fear not. He has never yet been subdued in that way. He becomes more bloated and venomous by confinement; he thrives on the thick foul vapours of the lazar-house; he is the monster "that doth make the meat it feeds on." We should beware, then, how we attempt to isolate the disease. Experience has shewn that it has never yet been shut up, that it has not come forth from its confinement with increased severity; whilst, on the other hand, it is enfeebled and it languishes in the pure air of heaven. It will not be restrained; it cannot be extinguished; it is in progress; and we will be wise in suffering it, as far as may be, to "pass peaceably." It will abide with us, we doubt not, in its progress; and it is to the event of that visitation that our attention should be principally turned. We repeat that we can repose no faith in any measures taken for the exclusion of cholera, except the forlorn hope afforded by our quarantine; and we have no confidence in any means of arresting it, if once introduced, except on those which have for their immediate object the abolition of every thing like a *nidus* for its reception. It is in this respect, we think, that the labours of the Board of Health will be most usefully put in requisition. They cannot announce a specific remedy for the disease, because none exists, or at least has been discovered. It has become the fashion to censure this body, for no other reason apparently than that they have not afforded to the public "satisfactory information," when there was nothing satisfactory to give. Periodicals and journals may, without impropriety, publish every fact or suggestion as it occurs, for the public will estimate these for themselves; and we have been among the foremost to present them with the most interesting details. But the Board of Health are very differently situated; their first care must be, not to mislead. They must sift statements, to separate those which are solid; they must weigh conclusions, to ascertain their real value. To them the public anxiously look, as to an authority by which they are to be guided; and one premature recommendation might be productive of evil which could never be remedied. The public cannot be too strongly cautioned against the nostrums with which the press is overloaded; they are almost all the products of men writing in their closets, without having seen a solitary specimen of the disease. But, again, it has been said, and with great apparent reason—why are there not among them some who have seen the disease in India? and we confess, that so strong did this objection at first appear to us, that it seemed to be unanswerable. A more extensive acquaintance, however, with the accounts given by the Indian practitioners, and in particular, the circumstance of their opinions having been, in some essential points, refuted by the history of the disease in Europe, have led us to doubt very much whether any advantage would have been derived from the assistance of these gentlemen in the Board. The doctrine that cholera is not contagious, held by the majority of the Indian practitioners, is rendered more than doubtful by its European invasion. The treatment by large doses of calomel and opium, enjoined by most of them, has been tried, and found wanting. The opinions of the Indian practitioners may be of value as regards cholera in India; but the cholera in Europe has refused to conform itself to their views, and we therefore think it would not be prudent to be guided by their opinions. We cannot avoid the conclusion, that a set of intelligent men, with minds free from preconceived notions, and with no favourite theory to maintain, are more likely to arrive at safe conclusions. All the information that is to be had upon the subject the Board of Health possess; and they have availed themselves of the assistance of several intelligent men, who have seen the disease in Asia. They know that a trust of fearful responsibility is reposed in their hands, and that the eyes of the public are anxiously fixed upon them. They number among them some of the most experienced and talented physicians of the metropolis, and they will soon have the co-operation of the enterprising men now on their return from Russia. While, therefore, we see fully the difficulty of their situation in a period of such alarm, and while we are satisfied that more is expected of them than, in the nature of things, they can effect, we feel assured that a general attention to the directions which — certainly not without due deliberation — they have issued, will do all which foresight can accomplish to mitigate the evil, should all efforts prove insufficient entirely to avert it.

RESUMÉ OF THE POLISH CHOLERA.

At a recent sitting of the French Academy of Medicine, MM. Chamberet and Allibert, members of the Warsaw Medical Commission, assisted; and upon being invited to give the meeting some account of the results of their experience, M. Chamberet rose, and said:—that he had no doubt of the identity of the Polish and Indian cholera: that with regard to *symptomatology*, the most striking circumstances were the epigastric anxiety, the excessive and multiplied discharges from the stomach and bowels, the cramps, the sinking of the features, &c. The disease, he continued, suddenly attacked those who were apparently in the most perfect health. On the second or third day the nervous symptoms prevail: the patient is like one *deadly sea-sick:* he is in a state of idiotism. If, however, he gets over the fourth or fifth day, he will probably recover.

The great predisposing cause, M. Chamberet is of opinion, is a peculiar state of the atmosphere; but besides this, there are poverty, want of food, and frequently indigestion from surfeiting, which act as occasional causes. The Polish soldiers, who get their rations for three or four days at a time, gorge themselves on the first day, and thus expose themselves an easy prey to the disease.

With regard to the *morbid anatomy* of cholera, M. Chamberet states that inflammation of the digestive tube is never absent except when the malady has proved rapidly mortal. The mucous membrane of the stomach and intestines is lined with a pultaceous liquid, of a greyish colour, mixed with mucous and alimentary matters. The liver is soft; and its membrane detachable with the greatest ease. The gall-bladder distended with a large quantity of greenish bile. The venous system is gorged with thick black blood; the urinary bladder generally empty, and contracted, sometimes to the size of a common nut; the meninges are generally injected; and a quantity of limpid, and sometimes bloody serum, is contained by the arachnoid and rachidian membranes.

As to *treatment*, numerous have been the articles of medicine employed, but all of them apparently with equal success. Calomel and nitrate of bismuth have been much lauded. Dr. Leo put twenty-two patients on the nitrate, and twenty of them died. Thirty were put on calomel, twelve of whom recovered, and the remaining eighteen died. But the approved method adopted by the committee was this:—bleeding; warm infusion; spirituous frictions; and sinapisms applied to a large portion of the surface of the body.

And as to the question of *contagion*, the whole body of the Polish physicians reject the doctrine *in toto*. The disease, it is true, broke out at Warsaw on the 10th of April, after a bloody engagement between the Poles and Russians, but weeks and months before this, it has been ascertained that the cholera prevailed sporadically in the country. One physician, in particular, distinctly noted three fatal cases; there is at least, then, some uncertainty about its origin. But it is remarkable, that there were about a hundred physicians, French, English, and German, employed about the sick in Warsaw, none of whom suffered from the cholera; ten of them even inoculated themselves with the blood of choleric patients. Nor were the porters or nurses of the hospitals, nor the undertakers, less exempt from the disease than the physicians. M. Chamberet added, that he had never seen cases that had been left totally destitute of medical aid; but the physician-general had, and he affirmed that the mortality was not greater among them than among those who enjoyed medical assistance; and that mortality was, probably, on the whole, about fifty per cent.

The thanks of the Académie were voted to M. Chamberet for his very satisfactory communications.

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

BY

DR. ANTHONY TODD THOMSON.

LECTURE II.

October [illegible]2, 1831.

Rheumatism.

GENTLEMEN,—In pursuance of the plan sketched out in my introductory lecture, I have selected four cases from amongst those admitted during the last week, as the subject of this discourse: they are those of James Pennifather, twenty-two years of age, a poulterer; Sarah Parker, a laundress, aged thirty; Elizabeth Sparrow, a laundress, forty four; and James West, a bricklayer, fifty-four. The disease under which these four persons are labouring is marked *rheumatism* in the case-book. They were all, with the exception of Pennifather, who had been for some time ailing, previously in a good state of health; and I am anxious to direct your attention to this fact, as it is one of the features which distinguishes Rheumatism from Gout. In one only of these cases, that of West, did the mode of attack correspond with the usual nosological description of acute rheumatism: this patient was seized with rigor, followed by pain in the knees, shooting along the limbs in both directions, which continued with more or less severity for nearly a week; it then suddenly left the limbs, and affected the head, particularly over the forehead, where it has ever since continued. It increases in the afternoon, and attains such a height as to prevent him from sleeping until near the morning, when he perspires profusely. He feels languid during the day; the bowels are lax; the urine is nearly natural; the tongue furred; the pulse 84, and sharp; and the skin clammy. In Pennifather, the rigor was slight preceding the pain, which suddenly attacked the side and the back part of the head and neck, and has been unattended by any remission or intermission. He perspires much at night. The bowels had been freely opened by a Calomel pill and purgative, three days before he presented himself at the dispensary, and they are still open; the urine is high coloured; the tongue clean and moist; the pulse 108, and soft. In the cases of Parker and Sparrow, the pains, which also followed a shivering fit, and were succeeded by heat and perspiration, attacked the pectoral muscles, and the scapula, passing, as it were, through the chest. In Parker, slight pains, extending from the shoulder to the wrist, were felt before the febrile attack commenced, and these gave way when the thorax became affected. The increase of pain at night, the perspirations, and other symptoms, were nearly the same as in the other cases. Sparrow is troubled with a slight cough, which leads me to remark that Rheumatism has been observed to alternate with habitual cough and expectoration, these always recurring after the decline of the rheumatic attack, and being as regularly suspended during its presence. It also, occasionally, alternates with cutaneous eruptions; and I have witnessed more than once its alternation with Hæmorrhoids.

Now, gentlemen, the history of these cases displays the difficulty of framing any general definition of a disease. In one circumstance only, that of the augmentation of the pain by external heat, do they all accord with the definition of Dr. Cullen; and in none of the cases, except West's, were the pains first felt in the large joints. Another exception to the description of authors meets your eye in the appearance of the urine, which is generally said to be high-coloured in the beginning; whereas in these cases it was natural, and only in Parker's did it assume a high colour, on the second day after she presented herself at the Dispensary; and which it has since maintained. This may be accounted for by the sweats in her case being more profuse than in the others. In none of these cases has it yet deposited the pink sediment which is usually perceived in it after the fever assumes an intermittent character, but which is not, however, characteristic of Rheumatism, as it occurs in Ague, Gout, and many other acute inflammatory diseases. The most copious deposit of this sediment which I ever witnessed occurred in a case of Hypertrophy of the heart, with Dropsy, which proved fatal. But you will find that in Rheumatism the urine is neither always high-coloured nor deposits pink sediment, and that the pains attack every part of the body. In youth they are said to affect more generally the head, the muscles of the chest, and the upper extremities; and in adult age, the hips, thighs, loins, and back; and this observation is verified by the cases before us. Parker's case, also, apparently confirms the remark made, I forget by whom, that, when the breathing only is affected the pectoralis major is the seat of pain; but that, when both the major and minor are affected, there is, besides, an acute pain, which seems to dart through the thorax, between the breast and the scapula. When first Parker was attacked with pain under the breast, the breathing was impeded, but when it had continued for twenty four hours, and may be supposed to have extended to the deeper-seated muscle, every inspiration was accompanied with pain, passing from the breast through to the scapula.

In these recorded cases the pain was accompanied by neither swelling nor redness; but in one of those not recorded, that of Sarah Padwell, a young, plump female, the wrists were much swelled, and painful to the touch, although not at all red. When redness occurs in Rheumatism, it differs from that which characterizes Gout, in not presenting the shining appearance, nor being accompanied with the tension always more or less present in gouty inflammation.

The age of the youngest of these patients is twenty-two; but I have met with instances of the disease in children of seven years; and you will find that it is limited to no period of life, from five years to extreme old age, but the greatest number of cases occur between fifteen and thirty. Besides these four patients, three other cases of Rheumatism have been admitted during the week, which is rather at variance with the received opinion that the disease prevails more in the colder than the warmer seasons. I am disposed to ascribe its present prevalence to the sudden transition from dry to rainy weather. In the year 1826 the summer was unusually hot and dry; but after the 18th of August a considerable quantity of rain fell, and numerous cases of Rheumatism appeared, although scarcely any were seen before the rains set in.

Few as these cases are, they enable me to point out to you, gentlemen, the frequency of metastasis in Rheumatism. In West's case the pain suddenly left the limbs, and attacked the head. This individual is of a sallow complexion, and of a comparatively weaker habit of body than the other three patients. Nothing is so much to be dreaded in Rheumatism as a translation of the pain and inflammation from the joints or muscles to a vital organ—the brain, for instance, or the lungs, kidneys, or stomach. It is sometimes the result of the improper application of rubefacients; and it is also said to follow copious and repeated venesection, but occasionally it occurs without any obvious cause. When it affects the brain, and death *immediately* follows, which I have seen twice happen, no appearances indicative of previous inflammation can be detected either in the membranes or in the substance of the organ; but when the fatal issue is not so instantaneous, the membranes generally display great vascularity, with adhesions, a deposition of gelatinous matter on the pia mater, and also a deposit of fibrine, forming adventitious membranes. There is commonly great dryness of the dura mater, and a smaller quantity than usual of fluid is found in the ventricles In a case of metastasis to the lungs, followed by Chorea, which came under my notice in private practice some years since, and terminated fatally, the left phrenic nerve was found to be in a highly inflamed state, with considerable congestion of the pulmonary vessels, but no other appearances of diseased action were detected. Fatal metastasis, however, is of rare occurrence; in upwards of a thousand cases which I have seen, four only proved fatal, from the translation of the disease to a vital organ.

In two only of the cases of Rheumatism admitted in the last week, including those under consideration, have I been able to trace the exciting cause of the disease. West ascribes his attack to working two days in a damp cellar; and Elizabeth Sparrow refers her's to having lately removed into a lodging which is damp. Water, indeed, in a state of vapour, seems to be the principal and most frequent remote cause of Rheumatism. A query thence arises, at what temperature does it acquire its noxious influence? From the disease occurring, even in warm climates, in rainy seasons, we might, *à priori*, suppose that a low temperature is not necessary for this purpose; but that there is some fallacy in this inference may be concluded from the results of a series of observations and calculations made by Mr. Gorrie, in the Carse of Gowrie, in Scotland, on the rain-gauge and thermometer in 1823-4-5; from which the fact appears to be established, that the greater the fall of rain in a given season, the colder that season will be found to be.—(*Horticult. Trans.* vol. vi.)

I would venture here, gentlemen, to throw out an hypothetical conjecture, which, if it do not enlighten you, may afford an exercise for your minds: knowing that much hydrogen is evolved in fogs, is it not probable that this is also the case whenever the atmosphere is surcharged with aqueous vapour? and as Professor Leslie has ascertained that the process of cooling proceeds more rapidly in hydrogen gas than in atmospheric air, how far is this likely to operate as a cause of Rheumatism? It is a well-known fact that the disease is often produced at sea, by frequent washing the decks, a custom now judiciously frequently dispensed with This observation is also confirmed by the accounts of voyagers in different parts of the globe. In Weddel's voyage towards the South Pole, we are told that when the ships were in latitude 61° 44°, longitude 31° 13′ 15″, having had a long course of dense fogs and fresh gales, the decks of the vessels were constantly wet, which produced amongst the seamen colds, agues, and *rheumatism*. As the voyagers in this case were far from land. and yet agues appeared, it may be asked whether the vegetable matter of the decks can affect the decomposition of the water, and produce something analogous to marsh miasma? In the account of Kotzebue's voyage of discovery, we are informed that near the Coral Islands, during frequent rains and gusts of wind, a sailor was attacked with Rheumatism, and another with Sciatica, (vol. 2, p 338, Trans.) Near Luçon, in the Chinese seas, Rheumatism manifested itself on account of the great dews that fell even before sun-set, (ibid, p. 339); and again, "immediately after we left Manilla Bay, (says Kotzebue) the heavy night-dews, in the neighbourhood of the high land, caused catarrhal and *rheumatic* fevers," (ibid, p 342). Rheumatism is very common in Switzerland, in situations confined between chains of high mountains, exposed to damps, and great variations of temperature; and in Canada the lumberers, who fell pines in the woods, and are exposed to great damps and chills, are generally the victims of Rheumatism. There can be no doubt, therefore, that moisture is the principal cause of acute rheumatism; but, it may be asked, does this operate in producing the disease by checking perspiration? To reply to this query, gentlemen, would lead to a long theoretical discussion, rather out of place in a practical discourse; it is of more importance to determine the length of time which elapses between exposure to moisture and the attack of the disease. In West's case it was six hours; and in general I have remarked that it is within twelve hours. Dr. Haygarth, (Clinical History of Diseases, Part I.) in noticing this period, which he terms the "latent period," referring to his tables, says, "out of twenty-one cases only four exceeded the period of forty-eight hours. The shortest time noted is half an hour, but I believe that the cause and effect are sometimes connected together without any interval of perfect health."

With respect to the treatment of this disease we can yet say little, as far as relates to the cases before us, as Pennifather only can be considered in a state of convalescence. By looking into the Case-book, you will find that the same means were employed in his case, West's, and Parker's. He and West were cupped upon the nape of the neck, and sixteen ounces of blood abstracted; and the bleeding was immediately followed by the administration of a pill containing five grains of Calomel and two of extract of Opium; a mixture was also prescribed for each, containing four fluid drachms of the wine of the seeds of Colchicum, a fourth part of which was taken every sixth hour. In both, the pains were much relieved on the following day, and in Pennifather they were completely gone on the 18th; at which time also the pulse had fallen to 72, and was soft and regular. He was then ordered three grains of sulphate of Quinia, in ℥ij of infusion of Gentian, to be taken three times a-day. On the 20th he continued still improving, and his strength much increased; ♏vii. of diluted sulphuric acid were added to each dose of his mixture. To day (22d) he says that he felt strong enough to go to work again; but in the morning he had a renewed attack of the pain in the back of his head, which, in an hour afterwards, moved to the forehead, and then left him. As his bowels had not acted for thirty hours, he was ordered a pill with two grains of Calomel and three of extract of Henbane, and a purging draught for the morning, and, after their operation, to continue his mixture. In Parker, twenty ounces of blood were taken by cupping between the shoulders, and the same medicines prescribed as in Pennifather's case. Here the Colchicum produced nausea and vertigo until it operated. One stool followed each dose. She was much relieved, and continued free from pain until the morning of the 20th, when the pain, but in a less degree returned, and continued the whole of the day. She perspires on the least exertion; the urine is high coloured, but clear; the tongue reddish, and slightly furred. A pill, containing seven grains of Calomel and four of extract of Henbane, was ordered for bed-time, and a strong purging draught for the morning, with the mixture to be continued.

The employment, in acute rheumatism, of Opium and Calomel after bleeding, was, I think, introduced by Dr. Robert Hamilton, of Lynn Regis, in 1764, but his dose of Opium seldom exceeded one grain; and he occasionally combined tartar emetic with it. In my own practice, I have long since discontinued the free use of the lancet in Rheumatism, and also the constant use of diaphoretics; and have trusted to a single local or general bleeding, where any was necessary, following it with a large dose of Opium, from two to three grains, in combination with from six to ten grains of Calomel, and a purge in the morning; which, notwithstanding Sydenham's condemnation of opiates, however acute the pain, has almost in every instance been followed by relief.

In the early part of my practical career, when fresh from the schools, I bled freely in Rheumatism, and repeated the bleedings; but I soon found that the buffy coat and cupped state of the blood continued to shew themselves on each bleeding, until the strength of the patient was reduced to an alarming degree. This was sufficient to set me to think for myself, and I have scarcely used, or ordered to be used, the lancet for upwards of twenty-five years, in acute rheumatism. When the symptoms authorize the abstraction of blood, I find every indication answered by cupping over the seat of the pain; but it is only when the pain attacks the head, or thorax, that I find it requisite.

I have rarely seen much benefit derived from Colchicum until it purges, provided this do not occur too soon after it is taken; in which case, although it purges, yet it does not bring away bilious stools, and little or no rosacic acid; the pink sediment appears in the urine. When this rapid purgative effect occurs, it is necessary to combine the wine of Colchicum with Magnesia, or some alkali, by which the irritability of the stomach and duodenum is allayed; and thence the Colchicum has time to stimulate both the hepatic and pancreatic ducts, and to be taken into the system. If it still run off by the bowels, I order the powder of the bulb, and combine it with the compound powder of Ipecacuanha. Were I to venture to theorize respecting its action, I would refer much of the benefit derived to its operation on the liver, and the influence of this organ over the function of the kidney, causing the excretion of rosacic acid, which appears to be critical in this disease. The reciprocal influence of the liver and kidney on one another, is well illustrated by an experiment of Dumas and Prevost. They cut both kidneys from a dog, and on examining the body after the death of the animal, they found the liver inflamed and the gall-bladder turgid with greenish or deep brown bile. The effect of the Colchicum in augmenting the proportion of the rosacic acid which is passed by the kidneys, is illustrated by observations of Professor Chelin. In a severe case of Gout, he ascertained that, before the use of the wine of Colchicum, the quantity of the acid deposited, both free and combined with Ammonia, was 0.069

On the 4th day of using the wine, 0.076
8th day 0.091
12th day 0.112

When the disease assumes its chronic or protracted form, I have seen the solution of tartar emetic more useful than any other remedial agent. I order gr. vi. of the tartrate of antimony and potassa to be dissolved in ℥vi. of distilled water, and taken in doses of two table spoonfuls every fourth hour, so that the six grains may be administered in twenty-four hours The two first doses generally produce vomiting; but the others only slight nausea. The disease commonly yields to this treatment in the course of six or eight days.

I shall reserve any remarks on my observations respecting the use of bark and Quinia in the latter stages of Rheumatism, until we have seen the result of the cases now under treatment*.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

GENERAL DISPENSARY.

Operation for Stone, with Clinical Remarks.

William Henry Looker, aged three years and eleven months, was admitted under the care of Mr. Coulson, October 21st, 1831. The mother says that the child has been afflicted for these last eighteen months with a "complaint in his water," and that latterly the child's sufferings had been most acute, particularly when it passed its urine. He was sounded and a calculus distinctly felt. The boy's health had of late very much suffered, from the constant pain and irritation under which he had been labouring.

Saturday, Oct. 22d.—Mr. Coulson operated on the child. A lithic acid calculus, of the size of an almond, was extracted; and, during the extraction, a small one, of the size of a large grain of wheat, fell on the ground. The operation was completed within the minute.

This is the third dispensary patient on which Mr. C. has operated for stone since April last. The child is doing extremely well.

After the boy was put to bed, Mr. Coulson made the following observations:—Gentlemen, you are aware that a mode of relieving a patient with stone is now coming into use, by which the cutting process is avoided, and the stone is reduced to minute fragments in the bladder, either by crushing or drilling it.

Nothing, I can assure you, would give me greater pleasure than to see the necessity for performing the lateral operation entirely dispensed with; for though a surgeon may well be proud of performing operations with celerity and safety, there is an object of still higher importance, and more worthy of his ambition, which is the lessening, as far as he can, the necessity for their performance at all. But there are cases in which, I fear, the lateral operation for stone will always be necessary. The child on which I have just operated is an illustration of this.

The clergyman of the parish in which the mother resided, from the most humane motives, recommended her to take the child to Baron Heurteloup. The Baron saw the boy two or three times, and after mature deliberation on the case, sent a note to the mother, of which the following is an extract:—"For such young children his operation is less favourable and safe than for grown-up persons; and the old operation is, in cases of very young children, less dangerous and less to be dreaded. Under these circumstances the Baron would recommend Mrs. L., as the most prudent and safest plan which she can follow, to have the stone taken out by the cutting operation, and that without delay." Such are the observations of the Baron Heurteloup, and I am sure that you cannot fail to be struck with the frankness and candour with which this ingenious man delivered his opinion on the merit of his own mode of operating. The ardour of enthusiasm has not blinded his judgment, but he candidly acknowledges some of the cases to which his operation is least, or not at all, applicable. Prior to performing the operation for stone, the surgeon should be extremely particular in preparing his patient, by those means which experience has shewn to be best conducive to this end. Celsus was aware of the necessity of bringing the patient into a fit state for the operation, by diet and abstinence; in fact, the most successful lithotomists are those who are most attentive to this point. Mr. S. Cooper, in his Surgical Dictionary, or at least in the edition which I have, has given no practical directions on this most important point. Obsolete instruments, and obsolete modes of operating, are detailed with minuteness, and this indispensable part of the treatment of a lithotomy patient is not at all alluded to. If the patient's health is much impaired, and the bowels are relaxed, which is not unfrequently the case in children, you must, before the operation, lessen the irritability of the bladder and bowels by an anodyne, and improve, as far as you can, the patient's health, by medicine and attention to diet. I generally give, on the night preceding the operation, a few grains of hydrarg. c. creta, with the pulv. rhei, and early on the following morning some castor oil. Two clysters should be given before the operation, one two or three hours after the oil, composed of gruel, olive oil, and salt; and the other about an hour before the operation, with common gruel and twenty or thirty drops of laudanum in it. If, when the surgeon arrives to perform the operation, the last injection has not come away, he should urge the patient, if he be an adult, to go to stool; if, however, it is a child, it will generally happen that, if the injection has not passed off, it will do so at the time the staff is introduced. On no account should the surgeon undertake the operation till the injection has come away. The patient should be placed on a table of sufficient height, so that the perineum be opposite to the breast of the surgeon. The table should rather be a little too high than too low, for it will be an advantage to you in the operation to be a little under your work than above it. After the patient is bound, his shoulders and back should be raised and supported with pillows, he should be brought to the edge of the table, and his thighs be widely separated by an assistant; but it is of great importance that the nates be kept straight, and that an inclination be not given to one side more than the other. I now introduce a curved staff, and give it to the care of an assistant. You no doubt observed that Mr. Pereira, who held the staff for me (and has done so on two or three other occasions), gave to the instrument a different direction to that in which it is usually held. Instead of holding it perpendicularly, or nearly so, Mr. P. inclined the handle a little towards the ground, with the groove turned towards the left side. By this inclination of the handle, you certainly make the groove of the staff less prominent in the perineum; but there is this advantage, when you have cut into the groove, you have no occasion to alter the position of the staff, and the fore-finger of your left hand is quite at your disposal for protecting the rectum, and guiding the knife. I find that I can perform the operation much more rapidly in this way, than by taking the staff into my own hand. M. Langenbeck is a strong advocate for this mode of holding the staff; he, however, advises the handle to be inclined still more towards the ground than I do. I begin the first incision rather low, about two fingers breadth above the anus; the bulb of the urethra will be then avoided. In fact the external incision, if commenced higher up, can be of no use to you; and I find that this, the upper part of the wound, is often the slowest to heal. Respecting the remaining steps of the operation, I have little new to offer. For the division of the prostate I use the long straight knife, with the beak in the middle line of the point. I began in 1828 with the gorget, but whatever merits this instrument may possess in the hands of an experienced operator, I feel confident that, for a beginner at least, the knife is the safest and best instrument which he can employ. If any hemorrhage occur, you must endeavour to compress the bleeding vessel with your finger.

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* In Dr. Thomson's Lecture inserted last week, *for* "res, *augustæ* domi," *read* "res *angustæ* domi."

The after treatment consists, if no inflammatory symptoms ensue, in merely keeping the patient quiet, though it is extremely difficult to carry this into execution with children. The knees by some surgeons are tied together; by others they are not; but they should always be kept raised, and the scrotum supported. No application is required to the wound, but the urine should be received on sponges, and the parts kept clean. Some urine soon begins to flow through the natural passage. In a man above sixty, on whom I operated some time ago, a considerable quantity of water came through the urethra on the morning following the operation. The time, however, varies at which the water entirely ceases to pass through the wound; and in some rare cases urinary fistulæ remain.

PRACTICAL DIRECTIONS REGARDING CHOLERA.

[The following memorandum on the subject of cholera has been issued by the Board of Health. The "rules and regulations" intended to prevent its introduction, and which recently appeared in the London Gazette, we shall probably take another opportunity of bringing before our readers.]

BOARD OF HEALTH.

College of Physicians, Oct. 20.

The following are the early symptoms of the disease in its most marked form, as it occurred to the observations of Dr. Russel and Dr. Barry, at St. Petersburgh, corroborated by the accounts from other places, where the disease has prevailed:—

Giddiness, sick stomach, nervous agitation, intermittent, slow, or small pulse, cramps, beginning at the tops of the fingers and toes, and rapidly approaching the trunk, give the first warning.

Vomiting, or purging, or both these evacuations, of a liquid like rice-water, or whey, or barley-water, come on; the features become sharp and contracted, the eye sinks, the look is expressive of terror and wildness; the lips, face, neck, hands, and feet, and soon after the thighs, arms, and whole surface, assume a leaden, blue, purple, black, or deep brown tint, according to the complexion of the individual, varying in shade with the intensity of the attack. The fingers and toes are reduced in size, the skin and soft parts covering them are wrinkled, shrivelled, and folded; the nails put on a bluish pearly white; the larger superficial veins are marked by flat lines of a deeper black; the pulse becomes either small as a thread, and scarcely vibrating, or else totally extinct.

The skin is deadly cold and often damp; the tongue always moist, often white and loaded, but flabby and chilled like a piece of dead flesh. The voice is nearly gone; the respiration quick, irregular, and imperfectly performed. The patient speaks in a whisper. He struggles for breath, and often lays his hand on his heart to point out the seat of his distress. Sometimes there are rigid spasms of the legs, thighs, and loins. The secretion of urine is totally suspended; vomitings and purgings, which are far from being the most important or dangerous symptoms, and which, in a very great number of cases of the disease, have not been profuse, or have been arrested by medicine early in the attack, succeed.

It is evident that the most urgent and peculiar symptom of this disease is the sudden depression of the vital powers; proved by the diminished action of the heart, the coldness of the surface and extremities, and the stagnant state of the whole circulation. It is important to advert to this fact, as pointing out the instant measures which may safely and beneficially be employed where medical aid cannot immediately be procured. All means tending to restore the circulation and maintain the warmth of the body should be had recourse to without delay. The patients should always immediately be put to bed, wrapt up in hot blankets, and warmth should be sustained by other external applications, such as repeated frictions with flannels and camphorated spirits; poultices of mustard and linseed (equal parts) to the stomach, particularly where pain and vomiting exist; similar poultices to the feet and legs, to restore their warmth. The returning heat of the body may be promoted by bags containing hot salt, or bran, applied to different parts of it. For the same purpose of restoring and sustaining the circulation, white wine-whey, with spice, hot brandy and water, or sal volatile, in the dose of a tea-spoonful, in hot water, frequently repeated, or from five to twenty drops of some of the essential oils, as peppermint, cloves, or cajeput, in a wine-glass of water, may be administered; with the same view, where the stomach will bear it, warm broth, with spice, may be employed. In very severe cases, or where medical aid is difficult to be obtained, from twenty to forty drops of laudanum may be given, in any of the warm drinks previously recommended.

These simple means are proposed as resources in the incipient stage of the disease, where medical aid has not yet been obtained.

In reference to the further means to be adopted in the treatment of this disease, it is necessary to state that no specific remedy has yet been ascertained; nor has any plan of cure been sufficiently commended by success to warrant its express recommendation from authority. The Board have already published a detailed statement of the methods of treatment adopted in India, and of the different opinions entertained as to the use of bleeding, emetics, calomel, opium, &c. There is reason to believe that more information on this subject may be obtained from those parts of the continent where the disease is now prevailing; but even should it be otherwise, the greatest confidence may be reposed in the intelligence and zeal which the medical practitioners of this country will employ in establishing an appropriate method of cure.

Henry Halford,
President of the Board.

CAJEPUT OIL.

M. Lamare, an old practitioner in the Isle of France, has lately written a strong letter to the Paris Académie de Médicine inveighing against the use of cajeput oil. It is not at all employed by the Indians, he says, in the cure of cholera: it is an *incendiary* medicine that cannot be too carefully guarded against: it may, indeed, be used with some advantage externally in chronic rheumatism; but it contains copper, and is dangerous, &c. In short, he is indignant at the pains which seem to have been taken in the French journals to cry it up, which he attributes altogether to cupidity and ignorance.

The reading of the letter elicited some facts from members of the Académie, who entered into a discussion upon it. The great specific gravity of cajeput oil is, according to M. Laudibert, a characteristic of its genuineness, which fraud cannot imitate. Its green colour does not always prove that it contains an oxide of copper, according to M. Caventou: it is frequently produced by an organic colouring substance inherent in the leaves; and the quantity of copper found in it is no more than 1-22nd part of a grain in each *gros*. Neither is the oil caustic, according to M. Pelletier, except when taken quite pure, like the essential oil of peppermint, &c. M. Lamare's letter, however, was thought worthy of being communicated to the journals.—*Gazette Médicale.*

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THE PRINCIPLES AND PRACTICE
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Syſtematic Diſſertations
ON
MIDWIFERY,
AND ON THE
DISEASES OF WOMEN & CHILDREN,
Forming a Cyclopædia of those Branches of Medical Science

BY DAVID D. DAVIS, M.D., M.R.S.L.
PROFESSOR OF MIDWIFERY IN THE UNIVERSITY OF LONDON.

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FROM THE INTRODUCTION.

IT is the object of the above work to treat of the art of Midwifery, together with those other branches of medicine and surgery which are usually taught by Professors of Midwifery in the principal medical schools of this and other countries.

It is the usual practice of lecturers on Midwifery, and one which, indeed, cannot well be dispensed with on the part of teachers, who have to supply their pupils with opportunities of attending cases, to discuss the whole of the obstetric department of their subject at the beginning, or at an early period of their course; and then to take up in succession, respectively, the two remaining departments, the Diseases of Women and the Diseases of Children. The Author, however, hopes to be able to interest a greater number of readers by adopting what, he flatters himself, will be considered a more consecutive and philosophical arrangement of his matter.

The peculiar diseases of women are principally affections of organs which are proper to the sex. Of such organs there are diseases of structure and disorders of function. To obtain an accurate knowledge of either of these varieties of morbid action, it is manifest that an acquaintance must be previously possessed with the structure and functions of the same organs in their healthy state. In accordance with these views, the Author has chosen for himself an arrangement, which he believes will enable him to treat of all the subjects, which he will have to discuss, in their most obvious connexion and natural juxta position, under their respective heads of—STRUCTURE, FUNCTIONS, AND DISEASES BOTH OF STRUCTURE AND FUNCTIONS.

The Pelvis being the frame-work to which are naturally attached most of the organs interested in the diseases of women, the work will properly commence with a concise anatomical description of that part of the human skeleton. Then will follow a short statement of its uses or functions during health. Next will present itself for examination, the pelvis in its diseased state, or when become the subject of malformation or distortion incompatible with the performance of one or more of the most important functions which it has to sustain in the destinies of an adult female. Thus under one principal head of subject will be given every thing necessary to be known about the pelvis; including its anatomy, or a description of its structure, form, and relations in the human body; its physiology, or a description of its uses or functions; and, lastly, its pathology, or a description of its diseased conditions. Under their several heads respectively will in like manner be described and treated of, all the organs and structures attached to or contained within the pelvis; first, simply and anatomically in their healthy states; secondly, as agents in the performance of healthy functions; and, thirdly, as the subjects of morbid conditions or actions. Thus it may be easily seen, that, at a comparatively early period of his labours, the Author, by this arrangement, will have it in his power to bring under the consideration of his readers, some of the most interesting and important diseases to which the human female is subject. After, for example, giving a brief anatomical description of the unimpregnated uterus, he will be competent, according to the principle of his plan, to proceed without delay, to the consideration of its functions, and, especially, of the one great function, MENSTRUATION, by which it is qualified to become the subject of so many others. To this will follow a practical examination, including the causes, the history, and the treatment of all the diseases incident to this important organ; of its disorders, first, of function, which will include the irritable uterus, disordered menstruation under all its forms, leucorrhœa, sterility, &c. And, lastly, its diseases of position and of structure; viz. morbid descent, prolapsion, procidentia, and hernia; inflammations and indurations of all kinds, producing schirrosities, cancer, and fungoid enlargements, &c., diseased formations within its cavity, producing polypi, false conceptions, hydatids, &c., or within its proper substance, producing tumours of other kinds; together with numerous other diseased phenomena of a painful, and, ultimately, in too many cases, of a fatal character. Under a branch of the same general head of subject, will be treated of, the function and the diseases of the UTERUS, in its gravid state.

In this part of our investigations, will accordingly be presented to the reader, the principal facts appertaining to the curious subject of conception; as also, the development of the ovum, from the first moment of its mysterious existence, to its mature birth at the full period of gestation. Here, also, will be considered all the healthy phenomena of the pregnant state. In the remaining section of this interesting department of the work, will be considered such other conditions or results of pregnancy as are to be identified with disordered actions. This part of the inquiry will include the subjects of malpositions of the pregnant womb; such as prolapsion, retroversion, hernia, obliquities, &c., at various periods of gestation; painful states, and unkindly development of the uterus from rigidity or other causes; inflammations, separations of the ovum from its parent structure; diseases and death of the ovum in utero, productive of serious consequences to the mother; fevers and other constitutional diseases complicated with pregnancy; morbid states or actions of organs contiguous to the gravid uterus, as well, also, as of organs more or less remotely situated, but, nevertheless, exquisitely consenting with that great centre of female sympathies. Here also then, as in other parts of our undertaking, it may be observed that the same order and consecution of subjects is strictly pursued.

In that part of the work which more immediately treats of PARTURITION, will be introduced such a development of principles and rules of practice as are usually designated in books and in schools of medicine, THE THEORY AND PRACTICE OF MIDWIFERY; which will comprise an enumeration of all the varieties of labours, a practical distribution of them into classes, orders, and subdivisions, as founded respectively on their characters and causes; and, finally, an ample descriptive exhibition of the means, whether manual or instrumental, preventive or remedial, best calculated to ensure, if possible, the preservation of both the mother and her offspring; or, at least, all the good attainable by the most judicious indications of our art in its present state of acquirement.

The Diseases of Children will be treated of in the latter part of the work, and will be arranged under certain heads of subject, founded on the nature of the tissues principally or exclusively affected by them. The Author does not consider this arrangement as being perfectly unobjectionable, for it frequently happens that more tissues than one are implicated by the same disease. But, upon the whole, he adopts it as one sufficiently practical for his purpose, and as bearing something of analogy, at least in its principle, to that of which he has availed himself in the preceding and larger portion of his work. Accordingly, he will treat of the diseases of children under the several divisions following:—

1. Of the DIGESTIVE ORGANS, their FUNCTIONS and DISEASES.
2. Of the structures, functions, and diseases of the respiratory organs.
3. Of the circulation of the blood; its organs and derangement.
4. Of the anatomy, physiology, and diseases of the mucous membranes, not already treated of under any of the foregoing heads.
5. Of the affections of the lymphatic and glandular systems.
6. Of the brain and nervous system; their functions, mutual relations, and diseases.
7. Of the structure and properties of muscles; their dependence on the nervous system, and their diseases.
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*** Any truly important cases, or instructive results of treatment, on subjects coming within the scope of this Work, authenticated and addressed to the Author, in Fitzroy Square, will be deemed essential favours.

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LECTURES
ON
THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

BY DR. ELLIOTSON.

LECTURE V.

Explanation of the term Diathesis—Symptoms of Disease—Mode of examining these—Appearances of the Face—of the Tongue—of the Eyes—Risus Sardonicus—Facies Hippocratica—Expression of the Countenance—Examination of the Hand—the Pulse—its Characters.

THERE is a term, gentlemen, which, on reflection, I think I omitted to explain, or even to mention at all—namely, the word *diathesis.*

When the body is particularly predisposed to any kind of affection, it is said by some to have that particular *diathesis*, or a disposition to it. However, you will generally see this term limited to imply two states—an inflammatory state, and a state of weakness, of debility, if not of putrescency. You will hear the one called the *phlogistic*, or *sthenic*, which you know means strong, and the other called the asthenic, or weak. When a person, for instance, is in a high state of excitement, with a full and strong pulse and heat of body, and under any particular complaint, he is said to have the sthenic or phlogistic diathesis—in fact, an active inflammatory state of the body. If, however, the state be one of debility, so that any disease which is actually present is attended with weakness, and still more if by extreme collapse and a disposition to putrescence in its progress, then it is called the asthenic diathesis. I believe, in modern times, these terms are rarely used, except when a certain disease is present, so that they really imply, not the disposition to disease, but the character of the disease—and, indeed, as to whether it is actively inflammatory, attended by strength, or whether it is attended by weakness; but even of these two, you seldom hear persons speak of either, in this country, excepting the phlogistic.

I may mention that the word diathesis is used by old authors in other senses—to signify the general disposition to disease at any particular season, so that the constitution of the period was said to be its diathesis. If the disposition of any particular season of the year—not the regular seasons—were to produce such and such kinds of disease, the word diathesis was applied to that disposition—to the disposition, if I may so speak, of the atmosphere at the time. But, generally speaking, the word is used as I have pointed out above.

However, you will find a constitutional disposition to certain diseases, when present, called a diathesis. For instance, it is not uncommon, in practice, to hear persons talk of a scrofulous diathesis. Where a person has every look of scrofula, with its actual presence in one part, you will hear individuals say, that is a scrofulous diathesis. A disposition to many other kinds of disease, when present, is sometimes called a diathesis, but it is the phlogistic diathesis of which we hear the most.

This term is sometimes used to signify that full state of the body which would render a person very liable to an active inflammatory affection; but is certainly more frequently limited to that state when the disease actually has begun. When a violent disease begins, and there is quickness and strength of pulse, and heat, the person is said to have the phlogistic diathesis. Now and then, persons with merely a full pulse are said to be labouring under the phlogistic diathesis; but that state is characterized more particularly by the word plethora, of which I spoke in the last lecture. I stated, that when the blood was in excess to the strength and to the vessels, true plethora was said to exist—*plethora vera.*

Speaking of plethora, you will pardon me for recapitulating what I mentioned in the last lecture—viz. that there is true plethora, *plethora vera* or *absoluta*—that is to say, out of proportion to the strength or the vessels—*plethora ad vires*, or *plethora ad vasa.* I also stated that a fanciful kind of plethora was mentioned in old authors, where the blood is not supposed to be excessive in quantity, but to be expanded; and that is called *plethora ad molem*, or *plethora apparens*;—and another species of plethora was supposed to exist when the containing vessels were diminished in capacity, so that, though the quantity of blood was the same, the vessels became too small for it; and that has been called *plethora ad spatium*—the space being too small.

In the last lecture I began the consideration of symptoms in general. I may mention that, in some writers, you find symptoms and signs distinguished from each other, but I think it mere trifling to dwell upon such matters. With respect to symptoms, I was going to consider them as they were observable by the patient, or observable by others, because many are observable by the patient alone, and are not cognizable to others; those, however, which others observe, of course the patient, for the most part, may observe likewise. I have considered the chief of those of which a patient alone can take cognizance, and I will now proceed to consider those which the by-standers, and particularly medical by-standers, observe.

Now, in observing the symptoms of disease, it is right to remember that providence has blessed us with five senses, and all these five senses it would be ungrateful, as well as unscientific, in us not to employ. I am led to speak in this way on account of its being a modern practice to employ the ear extensively. Those who do so are laughed at as innovators—as people who take unnecessary pains, and are perhaps attempting a piece of quackery, in order to produce an impression upon patients; at any rate they are laughed at: but it will be impossible for them to be laughed at long. If nature have blessed us with hearing to observe certain phenomena around us, there can be no reason for not employing it when we come to observe the phenomena of disease; and if it be a fact that symptoms take place of the most useful nature that are cognizable by the ear only, it is our duty to employ our ears for the purpose of detecting them. We are bound, I think, to employ all our five senses in the investigation of disease. The chief, however, of which we make use, are the sense of sight, the sense of hearing, and the sense of touch; but the smell, and in some instances even the taste, may be called in to our aid; the smell, however, much more frequently than the taste.[*] It would be as absurd to shut our ears as to shut our eyes, yet, because the extensive employment of hearing has only been introduced lately, those who were not instructed in this point when they were students, are, some of them, too proud to condescend to learn in their old age, and are good enough to pity us younger men.

As to those symptoms which are cognizable by our various senses, they discover themselves and are apparent in every part of the body, from the head to the foot; but the two parts which give the chief symptoms to the sight, and I may say to the touch, and the chief symptoms not only in reference to themselves but to all other parts of the body, are the *head* and the *hand*. It has become the custom, in all civilized nations, to cover all parts of the body except the face and hands; and these two parts afford far more information respecting the state of the body, and respecting diseases that are present, and not seated in these individual parts, but at a distance, than any others. It is certainly a striking coincidence; but we must remember that while necessity compels mankind to keep the face, and almost always the hand, uncovered, Providence has ordained that these same two parts shall convey the chief information to others respecting our mind and body at all times.

With respect to the face, we observe that it gives us two sorts of information—first, as a mere portion of the body only, a portion of the surface which is far more affected by every change than almost any other excepting the hand, and therefore so far as it is *face;* and, secondly, it gives us another description of information so far as it is *countenance*, so far as it expresses the state of the mind and the state of feeling altogether. The observations which you make upon this part of the body may be considered as they respect simply *face*, and as they respect simply *countenance.*

With regard to the face as really a part of the surface of the body, if there be fulness of blood it is shewn particularly there. If you were to take the same quantity of surface in any other part of the body—the abdomen, back, or thighs—you would not get the same information from the part, simply as surface, as you do from the face. If the body be at all full, you see it in the face. If the circulation be rapid and the heat considerable, it is the face that shews it; the cheeks become tinged much sooner than any other part. If there be the least degree of coldness, you perceive it in the cheeks, nose, and lips. You discover hectic fever in an instant by the red patch upon the cheek. If the patient be labouring under difficulty of breathing, you find the eyes immediately suffused, you find the lips more or less livid, and the whole face frequently of a leaden hue; sometimes, indeed, I have seen it black. If it be jaundice that the patient labours under, you discover it instantly in the face; by one part of the face you will detect it long before it appears in any other part of the body except the hand; and when the disease has nearly declined, it is in the face as well as the hand that it lingers the longest. In the white of the eye you perceive the presence of jaundice before it is apparent any where else, except at the root of the nails. If the patient be bloodless, you first discover it in the paleness of the face, the want of the usual vermilion of the lips, and in the absolute paleness of the tongue; there is not a white crust upon the tongue, but that organ is absolutely blanched. One of the chief marks of scrofula is shewn in the face; there is the dilated pupil and the tumid under lip.

Then, again, in the *tongue* you have an immense number of symptoms. The body cannot labour under inflammation in any part but the tongue becomes affected by it. If there be strength, generally the tongue becomes white; if there be inflammation within the stomach and intestines, it frequently becomes red—red at the tip, red at the sides, perhaps red all over, and perhaps there are red stripes upon it. If the stomach be simply in a state of dyspepsia, and the bowels confined, you have the tongue covered with a yellow thick mucus. The indications of the tongue are innumerable; they have been observed from the remotest ages; and no one would think of finishing his attention to a patient without looking at that organ. The tongue sometimes will become brown, sometimes it will become absolutely black. In what are called putrid diseases, where there is a high degree of debility, and a disposition to putrescency, the tongue will become black. In other cases it will become glazed throughout, shining, and glassy; and in this state it will frequently be cracked into the bargain. In other diseases, again, as, for instance, in delirium tremens, it is frequently covered by a creamy mucus—not a thick yellow mucus—it is moist, not dry—but a mucus more like cream than any thing else, and so liquid that the tongue is moist. I need not insist upon the great importance of that part of the face or head—the tongue.

Then, again, if we take the *eyes*. We discover in a moment whether there is great excitement of the brain going on by the sparkling of the eyes, by their redness, and by their being watery. We can discover whether a person has headache by the oppression shewn in the eyes; and in some affections of the head, the eyes squint—are contorted in various ways. Frequently, too, when a person is asleep, he does not completely close his eye, and that is often an indication of disease. Then, again, the state of the pupil is very characteristic of many affections of the head. If you take the eye altogether, there is generally an appearance of brilliancy, or of dulness; or if you take particular parts, there is a dilatation of the pupil, or on the other hand, an extreme contraction; and all these things it is of the highest importance to observe, because they point out particular affections. For example, if you take apoplexy, you see the pupil usually dilated, and you know that there is great oppression of the brain; but if the pupil be amazingly contracted—reduced to a pin's point, you may almost to a certainty say that the patient will die: I do not say it is *absolutely* certain, because it is wrong to speak positively. If a person have taken opium in a large quantity, there is the same effect produced—generally a great contraction of the pupil; and where you are not with certainty informed of the patient having taken this narcotic, the appearance I have just described will assist you materially in forming the diagnosis. Then besides the strabysmus, and the open state of the eye during sleep, you have contortions in various instances, particularly if a child have convulsions of another part of the face—convulsions of the muscles of the lower jaw, so that he gnashes and grinds his teeth together during sleep, and his eyes roll.

From the state of the *orbit*, or at least the contents of the orbit around the eye, you judge of the extreme debility, exhaustion, and emaciation of the patient. If the parts around the eye become absorbed, the eye sinks back in the orbit, and a hollowness is produced, which is characteristic of extreme exhaustion and inanition, and this alone will enable you to say in a moment how much worse the patient has become.

From the face *altogether*, simply as a part of the body, you are able frequently to tell at once that the lungs are affected, or that there is a great disposition to affection of the lungs. It is impossible sometimes to doubt for a moment that if the patient be not labouring under phthisis, he soon will. From the peculiar appearance of the countenance altogether, particularly from the appearance of the eye, its transparency, together with the languor of the cheeks, or the flush upon them, no doubt can exist as to the nature of the disposition. And with respect to other parts, you can tell that the patient is labouring under organic disease of the abdomen from the look of the cheeks: I do not say it is so always, but the appearance of the face is of great use in enabling us to detect that circumstance. In organic diseases of the stomach, intestines, and surrounding parts, you have often a faint greenish yellow appearance of the face, something like that of a faded leaf, with minute vessels, not forming a patch of red, as in hectic fever, but reticulated, like net-work, on the cheeks. Where you observe this, there is generally some part of the abdomen in a state of organic disease. You discover the strength of a patient in a moment by his countenance. When you are attending a case of fever, there is frequently no occasion to ask a question: you can tell whether a patient is better or worse by viewing his countenance. So with respect to many other diseases, when the patient is getting either better or worse, you can tell immediately by his face, what change has taken place subsequently to your previous visit. Every body makes this observation; and hence, when an individual meets a friend, he tells him that he looks well, or he is sorry to see him ill. If the appearance of the face be so characteristic of the state of an individual's health to an inexperienced person, how much more so must it be to the medical man who studies minutely every shade of change and difference that takes place in each particular part!

Speaking of convulsions of the face—such as squinting or contortions of the eye, or a grinding and gnashing of the teeth, I may state that there is one set of convulsions about the mouth which is mentioned under the term *risus sardonicus*: it is a horrid sort of grin which you often see in persons who are about to expire. To me it is the most terrible of all sights. It has been thought to characterize inflammation of the diaphragm, but I do not know whether that is the case—inflammation of the diaphragm is a rare affection. It is common, however, for persons at death's door to grin frightfully. The phenomenon receives its name from a herb which grows in Sardinia, of a poisonous quality, and said to produce in those who eat it this horrid contortion of the mouth just before death.

You likewise see a singular appearance of the countenance, mentioned by almost every writer under the title of *facies hippocratica*; it is the usual appearance of the face immediately before death, when the body is at last completely exhausted, and life about to be resigned. It is so called because Hippocrates gave a most accurate description of it. His description is, sharpness of the nose, hollowness of the eyes, collapsed state of the temples, collapsed and contracted state of the ears; the edges of the nostrils everted; the skin of the forehead firm, tense, and dry; paleness of the whole face, perhaps even blackness, or a livid and a leaden appearance. These are the characteristics of the facies hippocratica. When you see a person dying from a disease that has exhausted him gradually, this is exactly the appearance of the face.

I may mention, although it is not in regular order, that among the symptoms of phthisis, one is taken from the teeth; not that all phthisical patients have the same state of the teeth, but in many there is a beautiful whiteness of the teeth, with a degree of transparency and brilliancy which is comparatively seldom seen in persons who are not disposed to phthisis.

Speaking of eruptions, I may mention that the exanthematic and acute inflammatory cutaneous diseases, shew themselves much better on the face, for the most part, than anywhere else. It is on the face first that you discover a child is going to have the measles. When a child is in a state of pyrexia, and you are sure it is going to be ill of something or other, the face will first disclose it, if it be the measles. There you have the running of the eyes—there you have the fulness; and the first papulæ that present themselves are generally seen on the face. Most acute cutaneous diseases attack the face among the earliest parts; or, if they do not, yet the face, if affected, discovers the true nature of the disease better frequently than any other part; the reason of which is, that the face is much more vascular than other parts. The cheeks are more or less inclined to be red in most individuals, and the least feverishness reddens the face more than any other part of the body; and therefore it is that acute cutaneous diseases generally shew themselves best in that situation.

With respect to the face simply as a part of the surface of the body, you discover paralysis in it more easily than in any other part. If a person be sitting still, you cannot tell whether his arm or his leg is paralyzed; but if his face be paralyzed, you will generally in an instant discover it by the part affected being drawn to the opposite side, and perhaps from his inability to swallow his saliva. And so with regard to the tongue; the moment the person begins to speak, you frequently in paralysis can discover the existence of the disease; the tongue frequently in paralysis moves to one side, but it generally moves in an opposite direction to that in which the mouth is drawn. In fever, and other diseases, the tremor of the tongue is very characteristic. In general the whole body trembles, but the tongue does so particularly. In paralysis agitans, if the person lie still in bed, yet if he put out his tongue, it is seen to be in a great degree of tremor. In cases of dyspnœa and apoplexy the tongue is swollen.

But the face is particularly indicative of the general state of the individual's mind, and of the general state of the strength likewise; and we then, perhaps, should rather call it the *countenance*, and speak of the countenance than of the face—the countenance being the term generally used in reference to the state of the feelings. You will judge directly whether a patient is in pain or not, whether he is suffering, by the countenance. In colic, and in enteritis, the agony is so depicted in the countenance that you in an instant discover that something must be wrong. I need not say that in insanity that passion of the mind which is predominant discovers itself in the countenance. You may at once detect a good-natured, a furious, a proud, or a desponding maniac. You may also discover much by the way in which his head is carried, to say nothing of his countenance. Those that are proud, carry the head upright; those that are cast down, carry it the reverse. But the countenance, independent of the position of the head, is indicative of the character of the disease; and again, an idiot is almost immediately discovered by the expression of his face. You ascertain in a moment by the countenance what the person's feelings are with respect to strength as well as to comfort, and what passions of the mind are predominant. You discover, as I before-mentioned, whether a person is better by observing his face, by noticing whether it looks fuller; but you discover much from the very expression of the countenance. You see, therefore, what a vast source of information is given by the face, not with respect to affections of that particular part, but with respect to all other parts of the body.

Speaking, however, of the general expression of the face, I must mention that you learn a great deal of the patient's feelings by the attitude of his body. In fever nothing is a more unfavourable omen than for a patient to be seen lying on his side at first, and gradually getting on his back till he lies altogether on his back; but when he sinks also in his bed, the case becomes still more alarming. The reason is simply this—more muscular strength is required in order to lie on the side than on the back, and to remain in a firm posture than to sink down; and therefore, when a patient sinks down, it is an indication that his strength is nearly exhausted. You discover a great deal, too, by the restlessness of the whole body. One of the most unfavourable symptoms in many diseases is extreme restlessness. You may have restlessness in women from mere fidgets; but in acute diseases, when great debility has come on, restlessness is often one of the fatal symptoms.

Of course, if disease take place in the face locally, it will shew itself there as in other parts of the frame, whether it be swelling, mortification, local cutaneous disease, or whatever else happens to it.

But again, to go to the *hand*, the part which, together with the face, in all civilized and uncivilized countries is most exposed. In the hand we have also very great indications, though certainly they are not so strong as in the face. The hand is one of the extreme parts, and shews the first fall of temperature. It is one of the first parts that become cold, exactly as the nose and ears do in the head. I believe the difference between its temperature in fever and in health is often greater than the difference of the temperature of most other parts of the body. A cold sweat is shewn in the hand amongst the earliest parts, and the very hectic fever which flushes the face flushes also the palm of the hand, and indeed the sole of the foot; but it is shewn particularly in the palm of the hand, from the greater fineness of the integuments. Difficulty of breathing again, discovers itself by the appearance of the nails. If you observe in the difficulty of breathing a purple hue of the lips, a fulness and a leaden colour of the face, and a purplish colour of the eyes, you discover likewise a purple hue of the nails, at least the parts under the nails. As I mentioned, in jaundice, that the disease appears first at the roots of the nails, as well as in the sclerotica, and after the disease has disappeared from the rest of the surface, you will discover it still lingering in the system by the yellowness observed in the same two parts, so I believe the changes go on, *pari passu*, in dyspnœa with respect to the eyes and nails. The dryness of the hand, its mordent heat, its clamminess, its firmness and softness, the very grasp of the patient, are all significant. If phthisis is often seen in the eye, the ends of the fingers are no less indicative of it or other internal scrofula.

Besides all this, it is in the hand that we easily make the most important conceivable observations upon the state of the circulation. It is in the hand, or at least at the wrist, that we feel the pulse much more easily than at any other part of the body. The same observation might be made any where else; but it would be done far less accurately, and with far less ease, than at the wrist. With respect to all other observations, about temperature, about heat, about cold, about clamminess, and about sweats, I think they are not only all made much more accurately in the hand than any other part, but the hand appears to be affected by these particular symptoms more altogether than most other parts of the surface of the body.

In regard, however, to the *pulse*, great indications of disease are taken from it. You will find the pulse generally spoken of in the first place, as great or small—*pulsus magnus, pulsus parvus*; you find it mentioned likewise as strong or weak—*pulsus validus, pulsus debilis*; and as hard or soft—*durus, mollis*. A pulse may be large, may be considerable, and yet not hard; a pulse also may be hard, and yet by no means large,—on the contrary, it may be exceedingly small. A pulse is not necessarily strong because it is large; neither is it necessarily weak because it is small in volume. If a pulse be both hard and large, it is a strong pulse also; if a pulse be small and soft together, then it must be considered as weak; and mere softness may be such as to amount to weakness; and if it be exceedingly weak, you may extinguish it by the least pressure of the finger. This pulse has been called *pulsus vermicularis*, and among us *fluttering*, if also quick. It is a pulse easily recognized in practice, and indicates death to be at hand in many cases. If a pulse be exceedingly hard, and at the same time small, then it has been called a *wiry* pulse. A wire is necessarily fine and hard, and a pulse which has the same attribute is called a wiry pulse. This is different from a thready pulse, because thread is different from wire. A thready pulse is merely that which is small, soft, and weak—a vermicular motion—hardly worthy of the name of a pulsation. It is chiefly in violent inflammations of the abdomen that you have the wiry pulse; however, you must not depend upon the pulse only in such cases. If there be other symptoms of inflammation of the stomach, or the intestines, or the peritoneum, you must not say that is not the disease because the pulse is not wiry. These are the qualities of the pulse with respect to its volume, solidity, and force,—large or small, hard or soft, strong or weak.

But there are other varieties of pulse, taken from its *rate*. A pulse may be quick, and a pulse may be slow; there is the *pulsus frequens*, and the *pulsus rarus*. The pulse will sometimes go up to 160; sometimes it will go beyond that, so that you cannot count it, especially if weak; but then you can ascertain the state of the circulation by the heart, where you can count 200 and upwards with perfect ease, when you cannot count the pulse at the wrist. You find a distinction made with regard to a frequent pulse—you find a *pulsus frequens* and a *pulsus celer*; *frequens* is where there is a large number of strokes in a given time, and *celer* is where the strokes take place in a short and abrupt manner. *Pulsus frequens* and *pulsus rarus* are opposite to each other; the former being a rapid, and the latter a slow pulse. The opposite of the *pulsus celer* is the *pulsus tardus*, in which, whatever number of beats take place, they occur in a gradual manner. In England practitioners call this pulse a jerking pulse: jerking and sharp are our terms corresponding to *celer* in regard to the pulse. No matter whether it is quick or not, we say it is a sharp or jerking pulse; whereas, on the other hand, that which takes place as it were deliberately is called *pulsus tardus*.

I may mention the great importance of feeling the pulse at both wrists. If you feel the pulse at the left wrist only, you may often think that the patient, from extreme debility, cannot bear bleeding; but if you feel it at the right also, you will frequently form a very different opinion. Nothing is more common than for the pulse at the two wrists to vary exceedingly; and it is the pulse at the right wrist that, in the greater number of cases, is the correct guide. The difference is so great in so large a number of cases, that it should be a general rule to observe both pulses. I never myself have seen the pulses different in point of time, but there is a great difference in respect to force. It is also certainly right, in cases of great nicety, where you are in doubt as to the means you are to use, to examine likewise at the heart; and in cases of local disease of that organ, it is also right to examine the heart itself; for in some diseases you will have simply, from an affection of the heart, a very violent pulse, such as would lead you to bleed to a great extent; and in those cases, the pulse is not an indication of the general strength of the patient, but receives its character solely from the disease of the heart. On the other hand you will, in some diseases of the heart itself, find a very small pulse, and on examining the heart you hear it beat violently, but through a diminution in the aperture the blood escapes in small quantity. It is right to examine not only both wrists where there is great nicety required, but I also advise you to listen to the heart.

I should likewise mention with respect to the pulse, that its rate, size, and force, are sometimes irregular; it will vary in its strength and size at different beats, and it will also vary in its frequency. You will sometimes have a pulse merely irregular, beating always with nearly the same force, but beating at an irregular rate, and that is an irregular pulse in the common acceptation of the word; but besides that, you frequently find it irregular in force, size, and frequency, so that it will give a great thump or two, and then you have a number of strokes of no strength at all, and then again it will beat violently. Irregularity, therefore, you must remember, does not refer to frequency only, but likewise to force. Sometimes the pulse intermits.

You must also of course remember, as physiologists, that the pulse differs in every age; that the younger we are, the more quick is the pulse; that the pulse of the female is quicker than that of the male; and that, generally, the inhabitants of a warm climate have a weaker pulse than those in a more temperate latitude. It is necessary to take all these things into consideration, because, if you were to find the pulse of a child 120, and that of an adult labouring under fever likewise 120, you would commit a great error in supposing that the child likewise laboured under fever. It is necessary, therefore, to remember that the pulse varies according to age, sex, and climate, and that in young children it is particularly quick. I had an instance last week of the great use of examining a patient in bed, and not being contented without what is contemptuously called a mechanical examination, in an enlargement of the abdomen. I was called to visit a lady in the family way, about thirty years of age, whose pulse was about 80 or 90, but on listening to the abdomen—she being single, and having some disease, as she said—on listening to the abdomen, there was another pulsation about 128. The pulsation did not arise from any of the branches of the iliac arteries; that was impossible; for if they had beaten, it would have been at the same rate as the pulsation at the wrists. She had a pulse of 80; something within her had a pulse of 128; and what that was, I left her to settle by herself. Being a single lady, all that I could say was, that if she waited patiently, the whole of the disease would come away to a certainty in two or three months.

ON THE USE OF PURGATIVE MEDICINES IN INFLAMMATORY AFFECTIONS OF THE STOMACH AND BOWELS.

To the Editor of the London Medical Gazette.

SIR,

THERE is scarcely any probability that medicine will ever become quite free from the imputation of being a "conjectural art." The human frame is liable to be affected by so many agents, both physical and moral; the functions of its several organs are performed in a manner so complicated and mysterious, that, when any morbid change takes place in it, it is often extremely difficult, if not impossible, to discover either the cause or the nature of that change. But, in addition to this grand obstacle in the way of medical science, there are other subordinate ones, with which it is of more importance to be acquainted than with even the primary one itself. For this last, originating in the nature of the subjects which the science embraces, is inseparable from it; while the others, being purely accidental, admit of being removed.

Among the most powerful of these accidental impediments may be reckoned the readiness with which the generality of those who cultivate medicine submit to be influenced by the example and the authority of their predecessors. For thus their thoughts, starting from the same point, and running in exactly the same line, reach, of necessity, the same conclusion; and by this repeated iteration the track is, in process of time, worn into so deep a groove that none but an intellect of extraordinary powers can effect a deviation from it. The consequence of this is, that when once an error is adopted by a few persons of reputation, or even by a single person of marked celebrity, the great body of the profession is drawn after, and a long time must elapse before that error can be got rid of. For it is not often that we meet in any scientific pursuit with a mind possessing that unusual degree of vigour, without which it is impossible both to resist the attraction of multiplied examples, and at the same time to overcome the obstacles that present themselves at the very entrance of a hitherto untrodden path. But let a new track be once struck out, and such is the fondness for novelty, that though it wander from the way to truth far more widely than the old one, yet forsaking this latter, almost the whole crowd will press forwards upon it. Thus it is, that both in the theory and in the practice of medicine, improvement is retarded by a slavish submission to authority on the one hand, and by an inconsiderate attachment to novelty on the other, qualities which, however opposite to each other they appear, are here, at least, closely united in their origin.

The truth of the above observations will, I think, be rendered sufficiently evident by a reference to that particular measure in the practice of medicine of which I am about to treat, namely, the use of purgative medicines. It is only a very few years since these were thought an essential part in the treatment of almost every disease. "The alimentary canal," it was said, "being the place in which the process of nutrition commences, it is absolutely necessary for the well-being of the whole system, that its functions be performed in a regular and healthy manner." Again, "the stomach and bowels, but particularly the former, are connected with every other part of the body by a sympathy so close and so powerful, that when a disease occurs, be its seat where it may, they cannot escape being more or less implicated in it." Now these arguments are just and reasonable; and the practice founded on them, restricted within due limits, was experienced to be highly beneficial. But not satisfied with this, one or two persons, of considerable eminence in the profession, proceeded much further. According to them, not only is the alimentary canal affected in almost every disease, but it is from some derangement in its functions that most diseases originate. Entertaining this opinion, they regarded attention to the state of the stomach and bowels as the main, if not the only, curative measure indicated, no matter what was the nature of the affection, or where it was seated; and thus they framed a system of therapeutics which had, at least, the merit of being pre-eminently simple.

But this very circumstance ought to have rendered the profession wary in receiving it; for a remedy so simple and so general, implies simplicity and uniformity of derangement, a thing scarcely to be expected in a machine so complicated as the human frame is, both in its structure and its operations. Such, indeed, was the opinion respecting it entertained by many reflecting minds; but by the great majority of the public, both professional and otherwise, it was received, cherished, and maintained, with a zeal bordering upon enthusiasm. Every fact that militated against it was treated with contempt, or entirely overlooked; while all those which seemed to lend it support were brought forward, insisted on, and magnified in importance. Did you hear a physician belonging to this sect interrogate a patient? All his questions related to the state of the secretions, the frequency, the quantity, and the quality of the stools. The answers of the patient went, perhaps, to shew that here all was healthy and natural; but this was quite disregarded by the physician. He felt quite certain, whatever appearances to the contrary existed, that the stomach and bowels were the theatre of the first movements of the disease; or that they had become, at least, the head quarters of its concentrated forces. To these parts, therefore, his operations were entirely directed. Blue-pill, calomel, jalap, and black-draught, were the instruments with which he worked, and he used them without much regard to moderation. Even of those who ridiculed the theory many soon came, almost insensibly to themselves, to adopt to its fullest extent the practice. Thus were all the distinctive marks of diseases broken down; or if any were allowed to remain, it was only to divide them into two classes, those which admitted of violent purging, and those in which aperients only ought to be used. In this manner affections of the most opposite nature were treated with precisely the same remedies; and practitioners, so far from doubting the reasonableness of such a proceeding, prided themselves on their ingenuity in discovering, as they thought, the seat of the disease, when not a single symptom pointed to it.

Such was the method of practice which but a little time since prevailed throughout the profession. Now that the mania which established and extended it has passed away, we easily behold its extravagance; and wonder how, in opposition to reason and the evidence of well-known facts, it ever came to be so generally adopted. And yet if we examine attentively the doctrines respecting this part of medical practice, which are at the present day even more generally received and acted on, we shall, if I mistake not, find them to be still less consonant with reason and with truth.

It is a fact, unquestioned by the supporters, either of the reigning or of the deposed doctrine, that in a great proportion of the diseases to which we are subject, the mucous membrane of the intestines is more or less affected. The controverted point, then, is respecting the treatment of such cases; and here, if it be proved that the tenets of Hamilton, Abernethy, and their disciples, though confessedly extravagant, are nevertheless not only more consistent with reason and analogy, but are actually found to be more beneficial in practice than those of Broussais and the French school, it will scarcely, I think, be disputed that in adopting the latter we betray greater weakness of judgment than those did who put such implicit faith in the former.

It cannot have escaped the notice of any one who has been in the habit of reading the reports of cases treated in the French hospitals, that since the "physiological doctrine," as it is called, has come to be generally acted on, there have occurred a far greater number of instances in which ulceration of some part of the digestive canal has been the only, or the principal morbid appearance after death, than an equal number of similar cases ordinarily present in the hospitals of this country, where such an extreme dread of purgative medicines has not yet seized upon the minds of the profession. Now it seems to me that this fact alone is decisive of the question; for I am not aware that there exists any other cause adequate to account for the difference; and, indeed, if we only reflect that in mucous membranes ulceration is a very frequent consequence of unchecked inflammation, and then consider the extremely inefficient method of treatment practised by the Broussain school, we will find it, I think, unnecessary to search after a different cause than this. What, for instance, can their lavements, their diluents, and their ptisans avail when violent inflammatory action has been established in the stomach or in the bowels? These measures, it is true, are aided by leeching and a pretty rigid system of abstinence; but, if I am not greatly mistaken, the application to the external surface of the abdomen of even a larger number of leeches than they usually have recourse to, cannot have a very powerful influence over the circulation in the internal parts; and the flame of inflammation, when once lighted up, does not require constant supplies of fresh food to maintain its existence; the system itself is almost always sufficiently well stored to furnish it with an adequate quantity of fuel. In what I here say, it is far from my intention to contend that abstinence, and the application of leeches, are measures altogether powerless. On the contrary, I am well aware that they are often very beneficial, but then they should be used merely as auxiliaries to other more efficient remedies, and not be trusted to alone, or in conjunction with means infinitely less active than themselves. I am not ignorant that, to the generality of persons, the application of thirty or forty leeches appears a very active kind of treatment; but in the particular affections of which we are treating, it exerts, in my opinion, by no means so powerful an influence. The grounds upon which I have formed this opinion are the following:—

1. The muscles and integuments of the abdomen, to which of course the application of the leeches is made, are principally supplied with blood by vessels which originate at a considerable distance from those furnished to the intestines, and which have very little connexion with them.

2. The slowness with which the blood moves through the intestinal capillaries, as well as through the portal system generally, and its tendency to congestion there, render them less easily affected by any diminution in the circulation than other parts much more remote from the place where the blood is drawn, and much less intimately connected with it.

This last objection, it may be urged, equally applies to the abstraction of blood generally, and I am ready to concede that it probably does. Where, then, are we to look for a remedy capable of achieving what neither of these measures, nor even both of them together, can properly effect? Before answering this question, which I expect to be able to do in the most satisfactory manner, I may remark here, that even in cases where the inflammation was of the most acute form, I have seen the application of a large blister succeed in checking it after blood-letting, both general and local, had been tried to no purpose. I can scarcely pretend to explain why it is that blisters should here prove so pre-eminently useful; but it may, in some measure at least, proceed from their rousing the capillaries of the intestines, and thus effecting a re-establishment of the suspended secretions. It is most probable, however, that had they been applied before the removal from the system of a proper quantity of blood, their effects would not have been so beneficial.

Having made these observations upon a practical fact, which is not, I think, in general sufficiently attended to, I proceed to state, in reply to the question above proposed, the means by which we have it in our power to effect what blood-letting, both general and local, fails in accomplishing. These means are simply the use of purgative medicines, by which alone we can hope effectually to relieve the overloaded intestinal vessels. Nor is there, as far as I am able to judge, the slightest cause for apprehending an increase of inflammatory action as the result of their operation. Unnatural vascular distention, if it do not itself constitute the inflammation, is at least a very prominent feature of it; and I have already shewn, that it is only by the use of purgative medicines that that distention can be thoroughly removed. Indeed, the manner in which nature herself not unfrequently succeeds in terminating the disease by an increased secretion from the inflamed parts, indicates clearly enough the propriety of attempting, by similar means, to attain the same object. To this argument it may be objected, that there is a wide difference between the process by which this increase of the secretions is produced naturally, and that by which it is effected when artificial means are employed. But in this objection there is stated, as a positive fact, what is nothing more than conjecture, and conjecture, too, without any probable foundation in truth, for there is not the slightest reason to suspect that purgatives operate by exciting an action different from the natural one. They do nothing more than cause an increase of the natural action; and this is the identical process by which the disease is often spontaneously terminated. I am not here opposing mere assertion to assertion, for the effects in both cases are strikingly alike; and similarity of effect is a pretty sure proof that the causes are not very different.

I am aware that artificial purgation, to whatever extent it may be carried, will not, in many cases, succeed in arresting the progress of the disease; but neither does spontaneous purging invariably effect the same end; so that this circumstance merely goes to prove that vascular distention does not alone constitute the disease; but that, in addition to it, there is some other morbid affection (probably of the nerves of the part) which may continue to exist after the distention has been removed. But after all this, it may still be urged that active purgative medicines, though they produce merely an increase of the natural action, cannot but irritate the inflamed intestinal surface; and that such irritation must, of necessity, aggravate the affection. Now this is a question which could never be satisfactorily decided by mere theoretical reasoning, for scarcely an argument could be advanced on one side that might not be met with an antagonist one from the other. Let us look, then, to the facts furnished by experience, and see to what conclusion their evidence will lead us.

If it were my purpose to make a general attack on the doctrine of Broussais, I could easily convict it of extravagance in supposing the presence of intestinal and gastric inflammation in numerous cases where no such inflammation really exists. Often and often have I seen tenderness over the stomach and abdominal region assumed as an undoubted proof that inflammatory action was going on in those parts when I had the utmost possible certainty that the tenderness was the result solely of the pressing and the pommelling which had been practised at a previous examination. But it is confessed, even by the most ardent supporters of the doctrine, that in many cases where they contend for the presence of inflammation, no sensible signs of its existence are afforded; while, at the same time, they cannot deny, that even in rude health, when the digestive functions are performed in the healthiest and most regular manner, the slightest pressure over the stomach will often be followed by a very keen sensation of soreness. These are facts which it is much easier to wink at than to meet with a successful opposition.

But the object which I hold in view is, to prove that even in those cases in which inflammation of the alimentary canal does really exist, the use of purgative medicines not only is not pernicious, but is actually productive of better effects than any other measure that we can have recourse to. And here I shall adduce a single fact, which, if the above observation be not strictly true, appears to me utterly inexplicable. In *enteritis*, attended with constipation, the general practice, at least in this country, is, after a due abstraction of blood, to throw in the strongest purgative medicines, almost without any regard to quantity, till several evacuations have been procured. And so far is the operation of these medicines from proving pernicious, that until it has taken place to a considerable extent, the patient cannot be considered free from danger. If it be objected to me, that in such a case as that to which I have alluded, there are hardened fæces, which act as a mechanical obstruction, irritating the parts, and that the utility of purgatives consists in their procuring the expulsion of these, I answer that the objection does not very much affect my argument; for it is inflammation that constitutes the disease, of which the constipation is, in general, but a mere symptom; yet we find that copious purging is the most effectual remedy for it, the principal use of the different other measures, blood-letting itself not excepted, consisting in their disposing the intestines to be more easily acted on by the purgative medicines. But even though I were to concede that in every case of enteritis the obstruction precedes the inflammation, and is the cause of it, my argument would still, nevertheless, hold good; for in *enteritis*, inflammation, by whatever cause excited and maintained, must be allowed to exist; and yet it is not aggravated by purgatives, but, on the contrary, is more relieved by them than by any other remedies. But it has been contended, that in *enteritis*, attended with constipation, the disease is entirely, or for the most part, seated in the serous membrane; so that in it purgatives cannot by contact irritate the inflamed surface. Now if it were true, that in this disease the serous membrane is exclusively the seat of the inflammation, the above reasoning might, perhaps, be admitted; but as morbid anatomy proves the case to be otherwise, its rejection ought not, of course, to cost us a moment's hesitation.

Another argument, proving unanswerably the utility of purgative medicines in inflammatory affections of the bowels, may be derived from the benefit following their use after the operation for strangulated hernia. It is the practice with most experienced surgeons to get the bowels thoroughly opened as soon as possible after the intestine has been reduced; and when this has been once effected, the danger of the patient is considered to be greatly diminished. This is a fact which will scarcely, I think, be disputed; and, as in all cases where the strangulation has existed even for a few hours, there must necessarily be inflammation, I cannot possibly see how the Broussain could attempt to explain why that inflammation is not aggravated by the purgatives.

The utility of large doses of calomel in the various dysenteric affections, so common in tropical climates, is rendered unquestionable by the concurrent testimony of Johnson, Annesley, and a whole host of other witnesses, many of them scarcely less eminent than these for closeness of observation and correctness of judgment. Now that these affections are of an inflammatory nature; that, in fact, they consist essentially in inflammation of the mucous membrane of the alimentary canal, scarcely admits of a doubt. How, then, does it happen, that in them calomel, so irritating in its nature, so drastic in its operation, and therefore so peculiarly the horror of Broussais and his disciples, proves beyond all comparison the most useful remedy with which we are acquainted? Whence, too, is it, that within certain limits, its good effects are more and more conspicuous in proportion as a larger dose of it is given. According to the "physiological doctrine," every increase in the quantity administered ought to produce a proportionate increase in the severity of the symptoms. The very reverse, however, of this is, as has been already observed, what really takes place. Small doses, by augmenting vascular action, without carrying it to such a height as to produce copious secretion, only torture the disease, which, out of resentment, as it were, for the injury done to it, always assumes additional violence, and not unfrequently succeeds in demolishing the fabric which contains it; while a single large dose is often sufficient to strangle it on the instant.

In the catarrhal affection which prevailed so generally throughout the country a short time since, I had numerous opportunities of proving the truth of the opinion for which I am here contending. The gastro-pulmonary mucous membrane, including, of course, that which lines the nostrils, mouth, and fauces, was the part principally affected. In general the symptoms were not very severe; yet there were some cases in which there prevailed excessive vomiting and purging, with violent internal pain, as well as external tenderness. Here, then, were symptoms the most unequivocal of inflammation—and inflammation, too, of rather an acute nature; and yet the cases in which recovery took place soonest were those that had been treated, from the commencement, with extensive purgation. Six or eight grains of blue pill at bed-time, with a drachm of compound jalap powder on the following morning, universally succeeded in ameliorating the symptoms. Repeated once or twice, with merely an antimonial powder, or draught, given occasionally in the intervals, they seldom failed in effecting a cure. In general, half a grain or a grain of opium was administered with the pill, both to ease the pain and to pacify the stomach, which was irritable and averse to retaining the medicines.

Contrasted with the success of this method of treatment, that of the Broussaian plan, which was tried in a few similar cases, shewed poor in the extreme. In these cases, the purging and vomiting, together with the pain and sickness which accompanied them, instead of being soon considerably mitigated, or of ceasing entirely, as usually happened after the operation of the purgative medicines, continued day after day, exhausting the patient, till at length nature succeeded in effecting, by a copious secretion from the intestines, what the exhibition of one or two smart purgatives might have easily accomplished on the first or second day after the attack. It is scarcely necessary for me to add, that when the disease was thus allowed to run its course, it left behind it a degree of debility, particularly in the digestive organs, which was not recovered from till after the lapse of a considerable period of time.

JOHN M'DIVITT.

Kegworth, Oct. 24, 1831.

BOTANY OF NEW ZEALAND;

BEING A

Description of Trees, Plants, &c. indigenous to that country.

BY GEORGE BENNETT, Esq. M. R. C. S. &c.

[With Engravings.]

THE vegetable productions of New Zealand are very numerous. Among the lofty timber trees which adorn the New Zealand forests, the "Pines" are the most conspicuous, from their stately, erect, and elegant growth: in the form of plank and spar, they are an article of commerce between that country and the colonies of New South Wales and Van Diemen's Land, and profitable voyages have been made with a selected assortment of spars even to England. I am acquainted with six New Zealand pines, and have specimens of them in my Botanical collection: they bear the native appellations of Kowri, Remu, Tánakáá, Káwaka, Tótara, and Kái-kea-tea, the whole of which are found growing on elevated land and in good soil.

The timber produced from the New Zealand pines varies in quality: the Kowri is generally preferred for the yards and masts of ships, as well as for other purposes; the Tánakáá is much heavier than the Kowri, sinks in the water, and is not very durable. It has been stated that the spars brought from New Zealand have been found on trial to be of equal gravity with Riga spars, and to possess a greater degree of flexibility than the very best species of fir procured from the north. The wood of the Kowri is much finer grained than any timber of the pine tribe; and the trunks are of such a size as to serve for the main and foretop masts of the largest three-deckers. The Prince Regent, of 120 guns, is supplied with them: they have also been used in sea-going ships, and the reports of their qualities are most favourable*.

Among other timber trees are the Knightea excelsa, with its clusters of beautiful crimson flowers; the Edwardsia microphylla, with its pendant golden blossoms; the Puredi (Ephialis Sp.), Pohu-tukawa (Metrosideros Sp.) &c. &c. Ferns abound in the New Zealand forests, among which may be recognized the genera Polypodium, Trichomanes, Lycopodium, Cyathea, Asplenium, &c.; the beautiful Todea pellucida of Brown, Trichomanes reniforme, Lindsia trichomanoides, &c.; and on the declivities of the hills, a beautiful species of Astelia, flowering in May, June, and July, is found very abundant. A great variety of the mosses and fungi are found also in the New Zealand forests, the latter attaining very large dimensions.

Dammara Australis, LAMBERT.
Dammara . . . . RUMPHIUS.
Agathis . . . . SALISBURY.
Kowri of the natives of NEW ZEALAND.

Natural Family, *Coniferæ*.

This magnificent tree is an ornament to the New Zealand forests. It is the Agathis of Salisbury, the Dammara Australis of Lambert, and is named

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* Quarterly Review, vol. xxxi. p. 64, Dec. 1824.

Kowri by the natives of New Zealand, which name, it is said, was given on account of the great diameter it attains. It grows very erect, and attains the height of from eighty to ninety feet, and in diameter is considered the largest tree in New Zealand. It is said to attain twenty-four feet in circumference, and I have seen several cut down measuring from sixteen to twenty feet. The timber is of an excellent quality, close grained, durable, and valuable for a variety of purposes, either in plank or spar: as yards and masts for shipping, they have been found, by repeated trials, superior to all others, both in their flexibility as well as durability. The wood is of a white colour.

The leaves, when young, are alternate, but in form lanceolate, which change, as the tree increases in growth, to an elliptical or oblong form.

This tree yields a great quantity of resin[*], of a white or amber colour, very transparent, burning with a bright flame, and having a very agreeable smell. It exudes spontaneously and concretes on the trunk, but an incision being made in the tree and left for a night, on the following morning a large quantity is found to have exuded, which continues still to exude from the incision for a long time afterwards. This tree yields the largest quantity of resin of all the New Zealand resiniferous trees. It resembles the resin named Dammar in the East Indies, and is, indeed, produced from a tree of the same genus, and might consequently be employed for similar purposes for which that resin is used in India—as a pitch for ships, &c. &c.; and might form an article of commerce from New Zealand to the colonies of New South Wales, Van Dieman's Land, &c.; and, from the quantity I have seen exude from one tree, it may be collected abundantly in a short period of time. The natives name the resin the Wai (Wy), or water of the tree.

The pigment, or soot (Ngarahu), which is the colouring substance used by them in the operation of tatauing, is usually prepared from this species of pine, from its possessing more resin than any of the others. The manner of preparing this soot among the New Zealanders is as follows:—

An oven is formed of stones, in which a fire is made of the wood of the pines, but more usually of the Kowri pine; from this a soot collects on the roof, and after sufficient quantity has been collected the fire is extinguished, and a mat being placed at the bottom of the oven, the soot is scraped off the upper part and collected in the mat placed underneath. It is then placed into a vessel, and sufficient water added to form it into lumps; in which state it is preserved until required for use. The soot yielded by this process is of a fine black colour, and a gum resin (damuh) is also used by the Javanese, which, being burnt and the soot collected, is used by them in the manufacture of their ink, which is beautifully soft and black, and is said probably to equal, in most respects, that of the Chinese. Our process of obtaining a lamp-black[*] is not far different from that adopted by the savages of New Zealand, and it is the resiniferous among trees that yields the most beautiful lamp-black.

When I inquired of a New Zealander, why they did not use any other kind of tree, and why particular pines were preferred? his reply was, because the pines contain more Wai (Wy), or Resin; and the Kowri was preferred because it contained more than any other species. When Resins burn they yield a quantity of smoke, from which much soot is deposited; this was observed by the savage, and adopted as an opportunity of acquiring a pigment for adorning his person. Among savages I have invariably found a very intimate knowledge to exist of trees, plants, &c. and the various uses to which they were capable of being applied[†]. The natives of New Zealand also collect the resin of the Kowri tree and use it as a masticatory, similar to a curious bituminous substance, a kind of Asphaltum (with a smell of Naptha), named Miniha *.

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[*] Vare is the term used in the New Zealand language for the Gum-resin; it also signifies the gum which exudes from the eyes. Wai (Wy), or water of the tree, is also used.

[*] Thus mentioned by Dr. Maton, in Lambert's work on the Genus Pinus. "A sort of box is made, nicely closed in every part, with the exception of some holes on the top, which are covered with a sort of linen cone. At a little distance from the box a furnace is constructed, with a very small mouth, and the inferior part communicating with the inside of the box by an horizontal chimney. Into this furnace are put the dregs and coarser parts left in the preparation of tar; and in proportion to the consumption of these a supply is kept up, so as to furnish a constant draught of smoke into the box. The smoke goes chiefly into the cone, when it deposits its soot or lamp-black, which is employed almost exclusively in printing and dying."

[†] The black pigment used for tatauing at Tahiti, is procured from the nuts of the Tiairi, or Candle nut-tree (Aleurites triloba), and also at Tonga'abu

There is a magnificent coloured engraving of this tree in Lambert's splendid work on the Genus Pinus; but it is to be regretted that the expensive style in which that work is published renders it almost inaccessible to the botanist, and consequently renders it less useful to science.

Knightea excelsa—Brown, Lin. Trans. vol x. 1811.

Riwa-riwa of the natives of New Zealand.

Natural Family, *Proteaceæ*.

This tree is abundant in the New Zealand forests, and is named Riwa-riwa by the natives; it is more usually found growing on the declivities of hills. It is a handsome tree, and attains the height of from sixty to seventy feet, but seldom exceeds more than seven or eight feet in circumference. The trees that came under my observation were invariably straight in their growth. The timber is of an excellent quality, but, on account of its small diameter, is not cut into plank, but is used for making planes, &c. by Europeans. The flowers are of a beautiful crimson colour, and the capsules are of a dark brown colour, growing in bunches of from twenty to thirty, each capsule containing from four to six winged seeds. I collected several specimens of this tree (with the capsules in a mature state) at Wyshaki Cove, River Thames, New Zealand, in June 1829. A branch of this tree is figured in the Linnæan Transactions, vol. x. 1811, both in flower and fruit, but the mature seed has not been delineated.

THE HOROEKA TREE OF NEW ZEALAND.

This tree, curious in form and its mode of growth, has not yet been observed by botanists either in flower or fructification. During a diligent search during the months of April, May, June, and part of July (1829), although I collected nearly a hundred specimens (thirty of which I brought to England), I was unable to procure it in flower or fruit. It is found growing usually in shady situations, on elevated land, as well as in vallies, and in good soil: it attains the height of from twenty to forty feet. In the very young trees the leaves are scattered; but in those which have attained more maturity and elevation, the leaves become tufted. No branches are thrown out, unless a short stalk, from which the tufts of foliage arise, can be so named. The leaves are about a foot in length, and an inch in breadth, and are irregular at their edges, and abrupt at their terminations, of a dark green colour above, the centre stalk being of an orange colour, and underneath the leaves have a brownish-red tinge.

In young as well as in more mature specimens, I have observed a change in the form of the leaf, on some the whole of the foliage being *ternate*, on others some of the leaves only being *ternate*, and others being of the usual form; others again have *ternate*, *binate*, and *single* leaves, on the same tree. This anomaly was more usually observed on young specimens than in mature trees.

The leaves on the summits of those trees which had attained a great elevation, became of increased breadth and decreased length; but this was not confined to old trees, young ones often exhibited similar anomalies in their foliage*. Some of the trees grow very erect, others bend towards the ground.

Some of the trunks of trees that I measured were only a foot to a foot and a half in circumference at the base, although in height they were thirty feet and upwards. The wood is very close-grained and flexible; and some Europeans residing at New Zealand informed me that they used it in boat-building. The accompanying sketch, taken from a recent but young specimen, will show the peculiar form of the leaves; (*a*) the ternate leaf.

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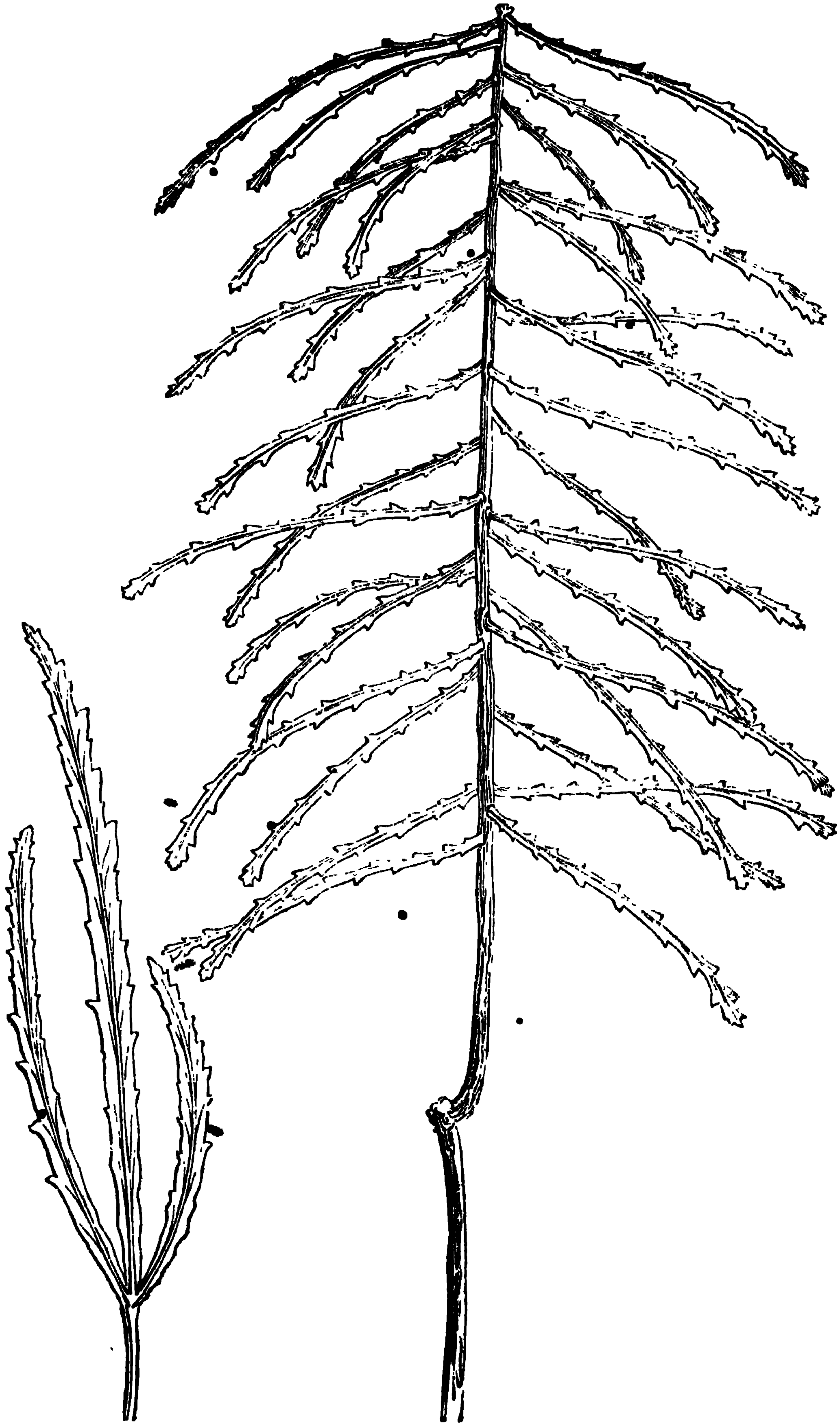
from the nuts of the same tree, where it is named Tui-tui. At the Island of Rótuma, from the nuts of the Fifau (Calophyllum inophyllum). All of these nuts contain oil. The nuts are first baked to a charcoal; afterwards pulverized, and mixed with oil.

* An account of which was published in the Medical Gazette of June 4, 1831, vol. viii. p. 302.

* It was at Wy-haki, river Thames, that the principal opportunities were afforded me of observing the growth and changes of foliage in this tree. Although I observed numerous specimens in a valley near the watering place at Kororadeka, Bay of Islands, I did not observe the anomalies in the foliage at that place.

Since writing the preceding I have been informed that a specimen in fructifica-



tion has been procured at New Zealand by one of the missionaries, and sent to Mr. Cunningham at Sydney, who has since brought it to England; we may therefore expect a description of this curious tree from that able botanist, and learn to what natural family it belongs.

OBSERVATIONS ON HERNIA.

To the Editor of the London Medical Gazette.

54, Stamford-Street, Oct. 24th, 1831.

SIR,

IN the *Medical Gazette* for February 19th last, there are some observations on hernia by Mr. Lizars, in which he has done me the honour of quoting my opinions; but as he appears to me to have misconceived the real nature of those opinions, I trust that it will not be deemed otherwise than an act of courtesy to him and your readers, to communicate (through the medium of your journal) a further explanation of my views upon the subject. I should have done so earlier, but other matters diverted me from the purpose.

Mr. Lizars commences by testifying to the difficulty in deciding upon the propriety of operating, and to the difficulty of diagnosis and prognosis, in cases of hernia. His words are, "there are four species of hernia which, in my opinion, demand an operation, viz. the acute or inflammatory strangulated, the slow or chronic strangulation, the incarcerated hernia, and the obstructed hernia." With due deference to Mr. Lizars, I conceive the whole difficulty in understanding the nature of hernia arises from the multiplicity of names used by surgical writers without any adequate or proper idea attached to them. I believe that if the terms, strangulated hernia, chronic strangulation, incarceration, engouement, &c. were abandoned, it would conduce to a better understanding of the subject. For example, all the symptoms which are said to denote a strangulated hernia, may exist without any strangulation, or even without any stricture whatever; such symptoms being common to all mechanical obstructions of the bowels, whether from adhesion of the intestine, intussusception, or to a contraction of the calibre of the canal, and by no means peculiar to strangulation. The term slow, or chronic strangulation, only applies to a state of constriction *not amounting to strangulation*, and is therefore incorrect. Incarceration conveys no idea beyond that of irreducible hernia: it does not express the morbid condition (obstruction) to which an irreducible hernia is subject. Engouement also is but the name for an effect: it does not express the cause of the disorder. Whence arises the engouement, or accumulation, but as an effect of some cause impeding the action and passage of the bowels? Engouement is, therefore, the *effect* of obstruction.

We should consider hernia first in its simple state, hernia meaning a simple protrusion; second, in its obstructed state, obstructed hernia meaning a hernia affected by some cause obstructing the function and passage of the intestinal tube. If the cause is stricture, the symptoms will be more or less acute and violent, depending upon the greater or less degree of constriction, and this may very properly be called acute obstructed hernia. If the cause is a confinement of the intestine in an unfavourable position, *without any stricture*, whether from adhesion, or from the intestines being in too narrow a space to carry on their peristaltic action, or transmit their contents, the symptoms, although the same in kind, will be more mild and protracted, and this will be properly defined by the term subacute obstructed hernia. The term obstructed hernia expresses the morbid condition, obstruction, under which a hernia, when requiring an operation, labours. The terms, acute and subacute, attached, express the *different degrees* in which they are affected. The only other term, in my opinion, which need be applied to hernia, is that of inflamed hernia, which term I have, in my Treatise on Obstructed and Inflamed Hernia, &c. given to denote a state of inflammation among the contents of a rupture, independent of any direct mechanical obstruction, but arising as a consequence of the morbid changes which the parts undergo from long protrusion.

I cannot but dissent from the opinion of Mr. C. Bell, quoted by Mr. Lizars, that "the obstruction kills by the violence of the inflammation, occasioned by the accumulation in the upper parts of the canal, and the violent workings of the muscular coat of the intestines."

The obstruction kills by the stoppage of an important vital function without the necessary intervention of any inflammation, as I have proved in the Treatise I have published, and which will be still further proved in an appendix to a forthcoming edition. Mr. Lizars has quoted from my book a passage commencing, "the character which the disease assumes is that of peritoneal and general inflammation," &c. The above passage Mr. Lizars has quoted as a part of my description of obstructed hernia. this is an oversight, such passage belonging to the description I have given of inflamed hernia, a state of the intestine particularly distinguished from obstructed hernia; I having also particularly stated that inflammation among the intestines is by no means a necessary consequence or attendant upon obstruction.

Requesting an early insertion of the above, I am, sir,

Your obedient servant,
HENRY STEPHENS.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The History of the Contagious Cholera; with Facts explanatory of its Origin and Laws, and of a rational Method of Cure. By JAMES KENNEDY, Member of the Royal College of Surgeons, London.

IF we were disposed to pay Mr. Kennedy an ambiguous compliment, we might recommend his volume as the most convincing in itself, and the most satisfactory that we have met with for some time. The contagious character of cholera is here clearly proved, from the facts stated; and the treatment, of which the author approves, is fully borne out by the cases adduced. The professions of the title-page are largely fulfilled; and as the book is meant to be intelligible even to unprofessional people, we think the author may congratulate himself on having accomplished his object. The reader has but to be persuaded that here is the *whole* history, and here are *all* the reports worth giving, and nothing can be more conclusive than Mr. Kennedy's labours. But, unfortunately for us, we have dipped rather too deeply into the sea of evidence, in the shape of multiplied reports, despatches, and official documents of all sorts and sizes, to permit us to be so easily satisfied, or to come from the perusal of the work untinged with some remnants of scepticism. Our pages, for months past, have shewn how much attention we have bestowed on all that was to be said on both sides; and though, to confess the truth, we are strongly inclined to adopt Mr. Kennedy's view of the question, to which we think the balance of the evidence will be found to preponderate, yet candour forbids us to declare ourselves perfectly satisfied as yet, and our duty as reviewers obliges us to remain unwedded to any particular set of opinions. This, however, by no means prevents us from appreciating the straightforwardness with which our author hoists his flag and announces the party to which he belongs. Mr. Kennedy, in fact, spends little time in reasoning directly on the contagiousness of the cholera; his historical matter and his collection of reports are arranged with sufficient ingenuity to carry the desired inferences along with them; and he is willing, he says, to consider the two following general facts as amply sufficient to justify his views:—

"The first is connected with the local progress of the disease, as when it begins in a camp or a town. Here, its first appearance is announced in the attack of one or of a few individuals, and the number of the cases gradually increases. This course cholera has universally pursued. Now, had the cause of the disease been generally diffused in the atmosphere of the camp and town, would not great numbers of the people have been attacked, almost immediately, on the occurrence of the first case?

"The second general fact is the following:—Among the islands of the Indian Ocean, it was observed that the cholera uniformly commenced its ravages in the sea-port towns, or in those towns seated a few miles inland, which have a constant intercourse with their harbours at the shore. In the island of Mauritius, the disease first appeared in the town of Port Louis. In Bourbon, the town of St. Dennis was the first attacked. In Java, the town of Samarang, and so of the islands Sumatra, Penang, Borneo, Celebes, Lucon, &c.

In the Persian Gulf, also, the same order of infection was observed. Muscat, the principal trading port town, first received the disease. Then the port of Bahrein, and Busheer and Bassora. How can this extraordinary and uniform partiality which the cholera exhibited in its choice of sea-port towns for its first inroad be explained, unless on the principle of imported contagion?"

It is to be particularly noted, that by far the largest portion of the volume is occupied with the history of the *Indian* cholera, and the principles and mode of cure which the author adopted when he was out in Hindostan. The disease, as developed in Western Europe, Mr. K. seems to think is identically the same as the Indian disease; nor does he seem to consider that the treatment, by rigorous depletion and dosing copiously with calomel and opium, so familiar in Indian practice, should be in any degree modified in the management of cholera among ourselves Upon this subject we have more than once expressed our decided opinion, nor need we repeat our reasons, founded upon experience and the strongest testimonies, for deeming that herein lies Mr. Kennedy's most essential faultiness. We doubt not, however, but that most readers will make this observation for themselves: were we but sure of it, we should have no hesitation in recommending the volume generally, as one of the most interesting on its awful subject that has yet fallen into our hands.

The work is furnished with two maps, from which we copy the following dates of first visits of the cholera:—

Jessore and Calcutta	Aug	1817
Delhi..............................	July	1818
Bombay	Aug.	—
Madras...	Oct.	—
Palamcotta	Jan.	1819
Manilla and Canton	—	1820
Pekin	—	1821
Busheer, Shiraz, Yezd, and Ispahan	—	—
Indian Islands.................	—	1823
Orenburg	—	1829
Astracan (up the Volgar) to Moscow	July to Sept.	1830
Down the Don to Azof,	Sept. to Oct.	—
Warsaw	April	1831
Dantzic and Riga	May	—
Archangel and Petersburgh,	June	—
Pesth and Bucharest	July	—
Berlin	Aug. 31,	—
Vienna	Sept.	—

Between the years 1823 and 1830, Persia seems to have been the *nidus* in which the slumbering pest was kept alive, and from which it got into Europe through the port of Astracan.

Reports of Medical Cases. By Dr. Bright. Price 9*l.* 9*s.*

[Continued from page 88.]

PRESSURE ON THE BRAIN *resumed.*

Obstruction in the Lungs.—The next source of pressure on the brain adverted to by Dr. Bright 's obstructed circulation through the lungs.

A bulky gentleman, above 70, who for several winters had been subject to chronic bronchitis, consulted Dr. Bright. He was in his drawing-room, walking about, but talking frequently; knew the doctor when he entered, and answered some questions about his health coherently; then at once seemed to become forgetful, and talked wildly. His attention, however, was immediately recalled, but as speedily was gone again. The pulse was large and full, about 80; the tongue dry, brown at the centre, and white towards the edges; the cough gave him great pain in the forehead; his expectoration was copious. Under the use of calomel, with purging mixture, ipecacuanha, and blisters to the chest, he was completely restored in a fortnight. Another case, however, which was very analogous, did not end so favourably. When Dr. Bright was sent for his patient was sitting up in his drawing-room, dressed as usual; his countenance was dusky, and his lips and tongue purple; he answered questions rationally for a few moments, and then became incoherent; his breathing was very difficult, and he complained much of weight at the chest; his pulse was large. He was cupped to eight ounces on the pit of the stomach, and had expectorant medicines; but he continued to get worse, and died early next morning.

We confess that both the above cases seem to us rather to shew cerebral disturbance from the circulation of imperfectly arterealized blood than from pressure.

A man, aged 46, was admitted into Guy's Hospital with dyspnœa, cough, and abundant expectoration of frothy mucus; his shoulders required to be elevated; his extremities were cold and œdematous; his pulse feeble and irregular. Stimulants were administered, but he grew more and more oppressed, and died within two days. On examination after death, general turgescence was found in the vessels of the brain, with a peculiar mottling throughout the whole medullary substance, which became still more conspicuous when a slice was cut off, and allowed to become dry. The lungs bore marks of old inflammation, and the mucous lining of the bronchi was red, thickened, and in places even granulated.

Several cases follow in which cerebral congestion arose from emphysema of the lungs. In these cases the clear hollow sound emitted by the chest on percussion, and the faint murmur of respiration which was present at the same time, clearly indicated the nature of the disease. The head affection, however, presents nothing peculiar, under such circumstances, to distinguish it from others dependent upon imperfect action of the lungs. In the two cases which follow, great congestion in one, and actual extravasation of blood on the surface of the brain in another, were dependent upon hooping-cough. Yet farther instances are given of congestion of the brain, brought on by pulmonary obstruction; but the illustrations we have given will be sufficient on this head. We observe that Dr. Bright attributes the symptoms to the joint operation of mechanical congestion and a chemical change in the blood.

Pressure from Serous Effusion, independent of Inflammation.

This constitutes an entirely distinct pathological condition from any of the preceding, and the first case adduced is one in which the cause must be regarded as entirely mechanical. A woman was hanged, and the brain examined forty-eight hours after her death. Drops of fluid blood presented themselves on the surface of the dura mater and on the inner surface of the bone, which had a mottled appearance from effused blood. There was *no* coagulum in any of the sinuses; there was "decided, but not great" serous effusion under the arachnoid; the medullary substance of the brain was of a dusky colour, and full of bloody spots; the ventricles contained some serous effusion; the vessels at the base of the cranium by no means injected; the arteries, indeed, were empty. In this case the appearance of cerebral congestion was not nearly so great as in some of the pulmonary affections above detailed, and not nearly so well marked as in the cases of death by hanging published some years ago by Dr. Monro, of Edinburgh, and by the late Dr. Kelly, of Leith.

Four cases are next detailed in which persons had become insensible from breathing the fumes of burning coals. They had all well-marked symptoms of cerebral congestion, but two of them recovered. In the two who died, slight serous effusion was found beneath the arachnoid, and there was some, but not considerable, appearance of increased vascular turgescence. In the case which follows next in order, effusion had taken place into the ventricles and beneath the arachnoid, from vascular congestion dependent on diseased heart. In another the same effect resulted from bronchitis; and in a third, which was attended with apoplectic symptoms from serous effusion, the kidneys were found granulated. In this case the urine was coagulable. The same appearance—namely, effusion of serum beneath the arachnoid, also presented itself in several cases of anasarca, where the kidneys were granulated, and the urine coagulable.

In case 118, a weak exsanguine man, with tuberculated lungs, had effusion under the arachnoid; in case 119, the same occurred in a case of emaciation, with bilious vomiting; and in three cases of diabetes, the only instances in which Dr. Bright has examined the brain in that disease, serous effusion was discovered. The author concludes this section with the following remarks upon the cases which it contains.

"In the foregoing cases of serous effusion we have an opportunity of tracing several of the different circumstances under which this morbid affection takes place within the cranium, independently of inflammatory action. These circumstances, for the most part, are immediately and obviously connected with a state of congestion, from causes which either act to a certain degree mechanically, or produce a state of debility which favours the irregular accumulation of blood. In the first case, direct mechanical pressure was applied; and if, as we suppose, the effusion depended upon the congestion arising from the ligature, the effect must have been produced very speedily. This, however, accords with what we have reason to believe occurs occasionally in disease, where, from the rapid appearance of symptoms, effusion seems to have taken place within a very few hours before death. Some facts have been recorded by Dr. Kellie which shew that causes producing cerebral congestion are calculated to bring on very speedily serous effusion: he has related the cases of two individuals who were found dead from a night's exposure to cold, where serous effusion had taken place to a considerable extent, both in the ventricles and beneath the arachnoid. The well-known effect of cold in producing a state of unconquerable drowsiness, is satisfactorily explained, if any such elucidation were requisite, by these two cases; for excessive venous congestion was found accompanying this effusion, and it is this state of congestion, no doubt, which brings on the lethargic slumber, and terminates in effusion, or not, according probably to the previous condition of the sufferer.

"The five cases in which symptoms of cerebral congestion depended on inhaling fumes from coal and from the brick-kiln, afford further examples of the same tendency to serous effusion when the circulation through the brain is obstructed; for in the two cases where death unfortunately gave us an opportunity of investigating the state of the brain, serous effusion had actually taken place; in most of the others there is reason to believe that the morbid affection proceeded no further than the state of congestion, though it is not impossible, from the slow recovery in Case CVII., that effusion had actually taken place to a small extent.

"We have likewise, amongst the cases of serous effusion, an example in which the congestion depended on the circulation being obstructed in the heart, a circumstance which is by no means uncommon; there are others in which bronchitis and phthisis, by permitting the natural flow of blood through the lungs, have produced such a condition in the vessels of the brain as has led to serous effusion: frequently, indeed, as we have before seen, these same causes of obstruction produce only venous congestion, a difference which depends upon the previous condition of the body. In some cases simple debility is perhaps capable of producing this effect, of which phthisis and diabetes seem occasionally to afford examples; though in both of these, other causes are brought into action, besides debility, in a manner likely to favour the effusion. We have sometimes less questionable examples of this cause of serous effusion, afforded in hæmorrhagic diseases and in the exsanguine constitutions of anasarcous patients, though in a great majority of these cases decided affections of the kidney, or of some other important organ, as the spleen, serve to throw doubt upon the extent to which debility should be considered the prevailing cause of the effusion."

Pressure from Effusion of Blood within the Cranium.

A very interesting and instructive collection of cases follows. They may be divided into those in which the effusion had taken place on the surface, and those in which it had occurred in the substance of the brain.

A diminutive lad, aged nineteen, was visited on the 30th of November; he was lying on the bed insensible, with the breathing slightly stertorous. He had complained of being unwell; and while on the close-stool suddenly exclaimed, "Oh, my head," at the same time laying his head on the bed. In this state he had continued till the time of being seen by the narrator, (Mr. Streeter). The pulse was jerking, and the skin moderately warm. A vein was opened, but ere two ounces had flowed he became cold, and the pulse fluttering; the arm was instantly bound up. Some brandy, and a little compound spirit of ammonia, were administered; cloths, dipped in warm water, were applied to the pit of the stomach; and as the head felt hot, cold cloths were applied suddenly, and with a jerk, (as a substitute for cold affusion, and to avoid wetting the bed) to the temples. As no abatement of the insensibility took place, two cupping-glasses were applied to the back of the neck, but without previous scarification. Still no apparent improvement manifesting itself, one scarification was made, and about an ounce of blood abstracted—watching its effect on the pulse, which becoming depressed, the depletion was immediately discontinued. After a few hours he regained his sensibility, and was able to speak, though languidly.

He was blistered on the back of the neck, and purged; and he appeared gradually improving till the 8th of December, when he had a return of pain in the head; during the night he became insensible, and died at eight next morning. Dissection exhibited at least eight ounces of blood on the left hemisphere of the brain, and this was found to have proceeded from the bursting of an aneurism, about the size of a small bean, where the anterior and middle lobes join: it was connected with one of the smaller ramifications of the middle cerebral artery.

A lady, advanced in life, suffered a sudden attack of apoplexy in May 1827, but which was so entirely relieved by bleeding that in a few days she was free from any symptom of it except slight headache. In February 1828, having dressed as usual, she fell down on walking into her drawing-room. She was bled, and recovered a little, so as to complain of excessive pain by screams and moans, but she could not speak, and died in about half an hour. No trace of the former attack was found on examining the head. Blood was effused deep between the convolutions of the brain, particularly on the left side. There was some blood in the third ventricle; none in the others. The rupture was supposed to have taken place where the vena magna Galeni joins the lateral sinus; near that part the chief coagulum was situated.

A gentleman fell and received a blow on the back of the head, not sufficient to excite much attention, though it caused some external tenderness. Some days afterwards he dined out, and on the following day had a party at home. The morning after, he complained of pain in the head, became sick, and on the third morning suddenly lost the use of the left side, and could scarcely articulate. He was bled copiously, but died in the evening. On the posterior surface of the right hemisphere was a large quantity of blood. Serum was effused in other parts of the brain, and also in the spinal canal.

A gentleman, aged 72, fell down while walking, having tripped against some wooden pegs, on one of which he struck his face. His nose bled very freely, but he got up and walked home, three quarters of a mile, and called on Mr. Estlin, of Bristol (by whom the case is related), on his way. No injury of any kind, except the bruise on the face, could be detected. He was purged, a cold lotion applied, and he was ordered to keep quiet and abstain from wine, &c. He continued, however, to walk about, as well as to eat and drink as usual. Mr. Estlin then adds—

"On the 11th of May (twelve days from the fall) he was met by his daughter, walking *from* his own house; and on being asked whither he was going, said he was proceeding *homeward*,—and shewed other marks of loss of judgment. I saw him soon after his return, and found him in a confused state of mind, with some incoherence He occasionally fell off into a sort of doze; and asked if I had been sent for, and had returned any answer? at the time I was sitting by him. He frequently took up a book and appeared to read; he complained of his head, but did not express any sense of severe pain. He walked with difficulty up to bed (two stories): I bled him, and purged him freely; the bowels were much loaded, and were torpid. On the following day some paralysis of the limbs appeared, and rather violent spasms of the face, particularly of the muscles of the lower jaw and neck, coming on in paroxysms, and lasting for a minute: his speech became very inarticulate, and he dozed a great deal. The symptoms daily increased. He was blistered, leeched, &c. When asked if in pain, he always said, "No;" and seemed, when thoroughly awake, conscious of what was said to him. His pulse was not quick or unsteady, and the pupils were not in any respect dilated. He died on the 18th of May."

On sawing through the cranium, a copious bleeding took place on the right and anterior part of the skull. When the calvarium had been removed, much coagulated blood was discovered beneath the dura mater, which extended to the base of the middle lobe, and invested the origin of the fifth pair of nerves. No injury of any part of the bone could be detected. The ethmoid was particularly examined. The source of the hæmorrhage could not be made out. Mr. Estlin thinks that the effusion was gradual.

A tall, large-made man, aged 46, of rather intemperate habits, and of gouty diathesis, had had at different times, some years before his death, fits of giddiness, pain in the head, and other cerebral symptoms, together with a peculiar fluttering sensation about the heart. About six months before his admission, he had a fit of an epileptic character, which occasionally returned; and he appeared, about two years previously, to have had a paralytic affection of one side of the face. His pulse was seldom above 30, and sometimes fell to 22. He had been in the hospital for gout during some months in the autumn of 1828, when he had frequent fits of palpitation, with confusion in the head. In May, 1829, he was again admitted, labouring under a similar condition. One of the most remarkable symptoms above alluded to continued—viz. the slowness of his pulse, which now averaged 32, while his respiration was so much as 28. He again left the hospital on the 1st of September, returned next day in his usual state for some mixture which he was in the habit of taking, and on the 3d, while in the act of taking a glass of spirits, he uttered an exclamation, fell, and died without a struggle. The following remarkable appearances were found in the head, which, we ought to remark, was not examined till five days after death.

> "On raising the anterior lobes from the skull a very marked black appearance was observed on the inside of the dura mater, covering the bone on each side of the crista galli: it was evidently a carbonaceous deposit in the layer of arachnoid lining the dura mater, and was made up of a number of black points: this black colour increased greatly in intensity on all that part of the dura mater which lines the petrous portion of the temporal and the lower part of the parietal bones, and proceeded quite back to the tentorium, and was observed likewise on the dura mater of the occipital bone, almost to the foramen magnum; and although chiefly on the right side, passed over slightly to the left. There was also a very small patch of the same black deposit, shaded off on all sides, under the left anterior lobe; the intensity of this colour varied, but was greatest near the petrous portion of the right temporal bone: a very slight stain of the same dark carbonaceous colour was traced on some parts of the surface of the brain, corresponding to the darker parts of the dura mater. This grey appearance was evidently not in the substance of the dura mater, but almost confined to the arachnoid which lines it, and could in some parts be peeled off, leaving the membrane nearly natural. The surface of the dura mater, next to the bone, was of a natural colour and appearance.
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> "The heart was remarkably large, appearing to distend the pericardium; it was at least twice the natural size, and the parietes throughout very feeble and thin; but all the valves were healthy, and the heart contained no fibrin or coagulum, and very little blood. There was one small patch of disease near the arch of the aorta, but the large vessels were in general healthy, though they were much stained with blood.
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> "The other viscera appeared healthy, but had in some degree lost their usual characteristic colours by the changes of approaching decomposition; unfortunately the kidneys were not examined."

The author conjectures that the heart was the primary source of disease, and that some spasmodic affection of that organ, connected with the gouty diathesis, had been the immediate cause of the sudden dissolution. The circumstances, he remarks, were not unlike those which occurred in the case of the late distinguished Dr. Marcet. *He*, too, had recently recovered from an irregular attack of gout; *his* death was instantaneous, and no obvious organic lesion, sufficient to account for the event, was found on post-mortem examination. In both, the body ran with great rapidity into decomposition. The peculiar appearance of the dura mater is certainly very interesting; it probably depended upon the injury which took place at the time of the paralytic attack, three years and a half before death, and had consisted originally in an effusion of blood upon the arachnoid at the part; and probably the pressure thus produced was the immediate cause of the extreme slowness of the pulse—a phenomenon which, in a less degree, is frequently witnessed in cerebral diseases attended with pressure.

MEDICAL GAZETTE.

Saturday, November 5, 1831.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—Cicero.

PRECAUTIONS AGAINST CHOLERA.

We entered upon the consideration of cholera now some months ago with a decided impression, derived from the opinions of the Indian writers, that the disease was not contagious, and this sentiment has been indirectly hinted at in various preceding articles. No long time elapsed, however, ere we began to doubt the accuracy of this opinion, and latterly a farther acquaintance with the history of the disease, and a careful attention to its progress, and to the phenomena it has displayed in Europe, have led us to abandon our preconceived notions, and to yield our conviction in favour of contagion. The grounds on which this inference has been founded, we shall detail in another number; but as we have no object but to discover the truth, and as our feelings are not interested on either side, so we have given, and shall continue to give, all the information which reaches us, whether it make for or against the opinions at which we have at length arrived; and we earnestly call upon those who have adopted opposite views to follow our example in this respect,—an adjuration which the ex-parte statements of certain recent writers have called forth, though various circumstances lead us to fear that it will be unavailing;—the end in view with some, apparently being, not to discover which is right, but to maintain their own side, be it right or wrong.

If cholera be contagious, then there are obvious means of excluding it; we do not say that these will prove effectual, but that the nature, end, and operation of the precautionary measures, are apparent. If, on the other hand, it be not contagious, then there is no human contrivance by which its introduction can be prevented, or even impeded. Now, admitting that the mode of its propagation may be matter of reasonable doubt, we would ask, which is the more dangerous error—to hold that it is not contagious, and it turn out in the sequel that it is so, or to hold that it is contagious, and it prove to be otherwise? There cannot be a shadow of doubt with regard to any thing so palpable. It cannot for a moment be denied, even by the staunchest anti-contagionist, that the direct tendency of teaching the public that a disease is not contagious, is to make them disregard those precautions which they would otherwise take against it; and thus, if the doctrine of non-contagion be erroneous, to multiply an hundred-fold the extent of the calamity. Let those, then, who ridicule the idea of contagion, beware, that the time do not come when the victims of their rashness shall denounce them as the deceivers that betrayed them into danger, and heap coals of fire upon their heads, by reminding the public of the exertions which they made to thwart the authorities in their reasonable endeavour to repel the invader, ere yet he had effected a landing on our shores.

To the quarantine now in operation we last week adverted, and we again recur, as affording the only hope which remains of excluding the disease from England; and if the regulations could be rendered as complete in practice as they are in theory, our hopes would be by no means faint; but with so extensive a coast to act upon, with so many temptations, and so many opportunities afforded by smuggling, of evading the sanitary precautions, we fear that much reliance is not to be placed on their efficiency. While, however, a chance of success remains, it behoves the press to co-operate with the government in carrying those measures into effect. True, they are injurious to trade, but what of that? the profits of the merchant must give place to the safety of the public: true, they are detrimental to the revenue; but surely it were better, if need be, to levy a tax upon the purses of liege subjects than upon their lives. Besides, the period of doubt cannot last long: if the disease come, why then farewell to farther quarantine, at least by sea: if it be kept out, then the measureless benefit of its exclusion will reconcile the most prejudiced and discontented to the temporary inconvenience.

Regarding the progressive approach of cholera to the west of Europe as rendering it extremely probable that this country will be subjected to its visitation, we are gratified to find that the inhabitants are preparing themselves in earnest for its reception. Local Boards and Committees are everywhere being formed; the work of precaution is in progress, and a general purification has begun. That it is so, is to be attributed to the recent manifesto issued by the Board of Health, and circulated by the government in every town and village throughout the land. Oh, for the consistency of certain among our contemporaries, who doubtless, if their modesty permitted, could easily tell us in whose hands the care of the public safety might have been much better placed than where it is! First they raised a cry against the Board of Health, because they did too little, and in this perhaps there was some shew of justice; now the complaint is, that they have done too much. Their first directions were too general, their last are too particular; their first announcement was not sufficiently impressive to arrest attention, their last is calculated "to fright the isle from its propriety." True, the document circulated by the Privy Council is of a nature to excite alarm, but was it not high time that the people should be roused to a sense of their danger? Would it have been kinder and more benevolent to have let them sit with their hands before them till the enemy was upon them, rather than warn them to be prepared that they might repel the aggressor? Would these whining deprecators of the measures demanded by common prudence—would they have us, like the ostrich of the desert, run our heads into the bush, and shut our eyes against the pursuer, till we became his prey?

In such a case as this, and with a people wrapt up in the busy pursuits of life, an appeal to their fears was the only method by which they could be roused from their habitual indifference; and we rejoice to find that it has not been made in vain. The tone of mild precaution was disregarded; but the more vivid picture of approaching danger has arrested attention, and the whole coast is on the *qui vive.* It is true, that more of apprehension mixes in the exertions at present making than may possibly be warranted; but those who have paid any attention to the workings of the public mind, must have observed how soon it becomes habituated to existing circumstances, and resumes its composure under events which at first were viewed with dread. It is incomparably better that the people should look the evil in the face—the worst of the panic will be over before the disease actually comes, and its presence will not create an hundredth part of the consternation it would otherwise have done.

Entertaining these sentiments, we earnestly call upon our readers to co-operate, to the utmost of their power, with the local authorities in carrying into effect the recommendations published by order of the Privy Council*. It is very true that some of the suggestions for the isolation of the sick appear to us of doubtful expediency; but all the preliminary measures have our fullest concurrence, and the more completely they are carried into effect, the less hazard there is of the ulterior precautions ever being required. Every man may do something towards improving ventilation, enjoining cleanliness, and urging the benefits of temperance. A desire to avoid susceptibility of the disease is one of the strongest motives by which to influence men's minds; already, indeed, has this salutary principle begun the work of reformation; and it is engendering habits of regularity, which the unaided precepts of prudence or morality have never succeeded in effecting. In France they have too much sense to keep the people in ignorance, lest they should be frightened; and it is ordered, that every house in Paris be visited by persons competent to judge of its condition as to ventilation, sewers, and other circumstances of a similar nature, so that not a family in that great metropolis but have the probable approach of the disease forced upon their attention; and we anticipate as the result, that all which human sagacity can devise will be put in operation to prepare for its reception. Why should it not be the same with us?—why are not the civil authorities enjoined to enforce the necessary precautions? Let us be prepared, if the disease should unhappily reach us, to meet the emergency with calm and well-directed efforts; and above all, let us not be betrayed into a false security by those who make light of a disease in which they have no experience. Those who talk the boldest while the enemy is not within sight, are generally the first to play the craven when he comes. Are they who exclaim that the alarm is false aware that it is ascertained that not fewer than an hundred thousand persons have been cut off in Hungary, and eighty thousand in Galatia, where the principles of non-contagion have prevailed, and where the people have waited for the disease in passive inactivity? Should the confident predictions with which the press has been teeming, that the disease will either not come, or come robbed of its terrors, prove fallacious, we should then have to encounter the most fearful of all calamities in such a case—that of a sudden and general panic, by which the energies of the public mind would be paralyzed at the very moment when coolness and self-possession would be most required.

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* We have not deemed it necessary to insert the Regulations issued by the Privy Council for the prevention of Cholera, as they have already been so very extensively circulated.

Since writing the above, our attention has been directed to some observations of Dr. James Johnson, recently published in the Times; we have also seen another letter from that gentleman in the Herald, into which it had been copied from the Courier; and we perceive that we are to be favoured, through the same medium, with a succession of papers of a similar nature. This somewhat singular mode of giving publicity to his opinions, the worthy doctor assures us, does not proceed either from "vanity or self-interest," and that he has no end in view but "the good of his country." That he should anticipate such a result from his epistles puts at rest the idea of "vanity" having any share in his motives. With regard to the absolute absence of "self-interest" we should not have been so well convinced had not the doctor given us the above satisfactory assurance; but for this, it is just possible, that having his name and address kept before the public in successive numbers of a daily paper in extensive circulation, and accompanied by the laudatory comments of a friendly editor, might have been regarded by ill-natured persons as having some slight weight in leading him to adopt a proceeding so unusual with respectable members of our profession. We subjoin an extract from the Courier, that our readers may judge of the tone in which the passages we allude to are written:—

"Dr. Johnson is, from his long acquaintance with the disease in India, and from his high standing here as a physician, a better authority, perhaps, than any other in this country; and it is not very creditable to the Board of Health to have put forth their opinions publicly, without having consulted that gentleman.'

We cannot for a moment suppose that Dr. Johnson had any thing to do with the insertion of this, or other equally absurd paragraphs; but we do think that most men would have avoided affording them even the indirect countenance they receive from his immediately becoming a contributor to the paper in which they appeared.

EXTRACTS FROM JOURNALS,

Foreign and Domestic.

ON THE IDENTITY OF SMALL-POX AND COW-POX, AND ON A MODE OF INDUCING THE VACCINE PUSTULE IN THE COW AT PLEASURE.

DR. SONDERLAND, of Bermen, the author of the paper which we shall here translate almost without abridgment, if his experiments be correct, has at length succeeded in establishing what physicians have long laboured to discover—a satisfactory and simple explanation of the protective power of cow-pox against small-pox; and has announced, we will venture to say, the most important discovery which has been made in the pathology of these diseases since vaccination was first introduced—by shewing that they are modifications of one another, and that cow-pox in the cow is simply small-pox in man, and may be produced in that animal at will by the variolous contagion. Of the authenticity of his facts we do not pretend to judge. All we can say is, that the author, if we judge from the language of Hufeland towards him, is a respectable practitioner, and a public medical officer.

"The simplest and surest mode," says he, "of producing cow-pox in the cow, and thus proving indisputably the identity between the contagion of cow-pox and that of human small-pox, is to follow the procedure here laid down.

"Take a woollen bedcover which has lain on the bed of a small-pox patient who has died during the suppurating stage, or is suffering from the disease in a considerable degree, and is lying in a small, imperfectly ventilated apartment; and when it is well penetrated by the contagion, roll it up immediately after death or on the fourteenth day of the disease, wrap it in a linen cloth, and then spread it for twenty-four hours on the back of a quey in such a manner that it cannot be thrown off by the animal. Then place it for twenty-four hours on the back of each of three other queys, and afterwards hang it in such a manner in their stall that its exhalations may rise upwards and be inhaled by them. In a few days the animals will fall sick and be seized with fever; and on the fourth or fifth day the udders and other parts covered with hard skin will present an eruption of pustules, which assume the well-known appearance of cow-pox and become filled with lymph. This lymph, which exactly resembles the lymph of genuine cow-pox, if used for inoculating the human subject, will induce the vaccine or protective pock. The only precaution which it is necessary to observe is that the person about to be inoculated shall not be exposed in any manner to the contagious effluvia of the cow-house either directly or through the intervention of the experimentalist's clothes, otherwise he may have natural small-pox.

"A bedcover impregnated with the variolous contagion, if firmly rolled up and wrapped in linen, and afterwards in paper, and then properly packed in a bucket, will retain the contagion for at least two years, so as to infect a cow with cow-pox, provided it be kept in a cool, shady place, where the temperature does not fall under 32 or above 52 degrees.

"My present occupations prevent me at this particular period from giving a full and scientific exposition of the consequences which must follow from this discovery; but I may state them shortly in the aphoristic form.

1. "This discovery is new; for, although many have suspected the identity of small-pox in man and cow-pox in the cow, and have in consequence performed inoculation with the matter of both, yet no one has previously ascer-

tained the possibility of transmitting the contagion to the cow in the gaseous form, so as to decide the question beyond all doubt.

2. "The desire of physicians and governments to discover cow-pox in cows, in order to revive the vaccine lymph, is more than fulfilled by the discovery of a simple method of engendering cow-pox in the cow at will.

3. "Jenner's discovery of the protective power of vaccination hitherto imperfect, is now perfected, because the hitherto unknown nature and origin of cow-pox are laid open.

4. "All previous uncertainty regarding the quality of vaccine matter, its degeneration, the loss of its protective property, and the like, must now cease, because we have obtained a clear insight into the nature of cow-pox and can lay down a substantial theory of its operation.

5. "This discovery must tend to widen the boundaries of physiology, pathology, and therapeutics, since it shews how the subtile contagion of small-pox, so hostile to the nervous system of man, may be conveyed in the aeriform state from him to the cow, excite in that animal a similar disease, but in doing so be changed by the special constitution of this class of animals into a permanent contagion of a different kind.

6. "An instructive lesson may be drawn from this discovery how the poison of diseases in the gaseous form may be communicated to the lower animals, and according to the difference in their constitution engender diversified products, which may be then used as pretective means against the diseases from which they originated. Such, for example, may be subsequently proved of scarlet fever, measles, yellow fever, and plague.

7. "It is now clear why, in recent times, cow-pox has been seldom or never seen in the cow. For the cow-pox of the cow arises merely from infection by the variolous exhalations from men recently affected with small-pox, and coming in contact with the cow. As epidemics of small-pox have been rare during the last thirty years, cows could seldom be exposed to infection, and have therefore seldom exhibited the disease."—*Edin. Med. Journ. and Journal des Praktischen Heilkunde, Januar.* 1831.

BRONCHOTOMY IN THE HORSE.

The number of the Veterinarian for November contains four cases in which the trachea was opened, with the effect of saving the lives of the animals: these were all instances in which suffocation was impending from inflammation about the larynx. The following is the description of the operation as performed in the first case:—"An incision was made, about four inches in length, through the integument and sterno-thyroideus muscle; a portion of one of the rings of the trachea was excised, large enough to admit one's finger; a thread was passed through the muscle and integument on each side, and fastened to the mane, to prevent the aperture from closing." The cases were treated by Mr. Toombs, of Great Barrington.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Oct. 29th.

The Cholera.

A REPORT having been circulated through the newspapers, that the all-engrossing subject of cholera was to be discussed here this evening, there was a very full attendance of members and visitors.

DR. SIGMOND in the Chair.

Whether the "order of the day" had been correctly reported or not, we cannot tell; but certain it is, that much time was lost in solemn silence after the president had announced that the business might now begin, until at length

DR. GRANVILLE volunteered to open the discussion. He expressed himself highly dissatisfied with the regulations of the Board of Health, which he thought were both useless and severe. On other occasions he had proved himself a stout advocate of contagion—as all would remember, on the Plague question; but he would now take credit to himself for maintaining as strenuously the opposite doctrine with regard to Cholera: on this question he was a steady non-contagionist. There was one great character of a contagious disease absent in cholera, there was *no eruption.* Nor was the spreading of the disease like that which would result from infection or contact: in St. Petersburgh, for instance, where the utmost care had been taken to exclude, the malady, *one* case was at length officially announced, and in less than four days after there were between 400 and 500. How could this possibly have resulted from contagion, unless the sick had actually run out into the street, and touched and infected every one they met? The same conclusion was also to be deduced from the history of the disease, as it appeared in other places. He regretted that we had not sufficiently exact accounts of the pathology of cholera: of the matter or fluid discharged from the stomach, there was but little information: it was only occasionally, and almost by accident, he (Dr. G.) could gather that it was sour to the taste; from which he was naturally induced to infer the presence of muriatic acid, especially as the other symptoms would warrant the supposition, that the patients suffered as if poisoned with that acid. The learned Doctor having indulged, in the course of his speech, in a vein of merriment upon the published regulations of the Board of Health, and upon the history of the disease generally, as given by the contagionists, apologized, in conclusion, for having treated with levity a subject of such awful interest: he did it merely, he said, to give animation to his arguments.

Dr. O'Shaughnessy contended that there was no ground for supposing that patients in cholera died as if poisoned with muriatic acid, or that there was evidence of any excess of that acid in the contents of their stomachs. And as to the absence of an eruption, he would instance hooping-cough, a decidedly contagious complaint, which was also unattended with an eruption. On the subject of cholera, Dr. O'S. expressed himself to be an advocate for its contagiousness: he instanced its making its appearance in the track of human intercourse at the greatest heights and lowest depths; and the recent information which had reached us with regard to the propagation of the disease among the pilgrims at Mecca, and thence spreading into Egypt, seemed to be decisive.

Dr. Granville, in explanation, maintained that pertussis was not contagious; and attributed the pestilential cholera among the pilgrims to their numbers, last season, and their long-continued prostrations over broiling sands, at 34° of Reaumur.

Dr. Gregory thought that the contagious cholera was a *new disease*, sent by Providence to visit the earth, as the small-pox was in the seventh century. The cholera, too, would probably abide with us, like the small-pox, appearing from time to time, after its first ravages, in a virulent form. The cholera had originated in India in 1817, and it still continued to exist in that country, and most likely would never be eradicated. The same, no doubt, would be its destiny in Europe. With regard to quarantine, he feared no measures would be sufficiently rigorous to exclude the disease: nothing short of an impenetrable wall built all round the country could effect such an object.

Dr. Johnson called the attention of the Society to the difficulty of coming at exact information with regard to the mode in which the disease was propagated: the evidences to be found in books were suited to the writers' preconceived opinions. He took the latest book published on cholera—that by Mr. Kennedy—and what did he there find? The author professes to give a selection from the excellent Indian Reports, and, out of some hundreds, thirty-seven are given. Now it is a well-known fact, that the great majority of Indian practitioners are non-contagionists; yet, what has Mr. Kennedy done?—of his select thirty-seven, all but two are contagionists. As to cholera being a *nova pestis*, as Dr. Gregory would have us believe, he (Dr. J.) could not subscribe to any such doctrine. He had *seen* the disease himself, in India, twenty-seven years ago, and Mr. Curtis had had experience of it twenty years earlier. Nay, he was not sure but that Sydenham had witnessed it, in the peculiar cholera of 1669, as he says nothing about the presence of bile in the discharges of the stomach, in his account of it. Precautions for the exclusion of cholera are useless: even if the impenetrable wall of Dr. Gregory were built as high as the sky, the disease will come among us *if it chooses*.

Dr. Stewart made some remarks on the probable influence of certain vicissitudes which had taken place in the weather within the last year or two. Severe cold had been experienced in southern climates, and extreme mildness in the northern latitudes.

Dr. Johnson (after a pause in the debate) rose to express his opinion that the propagation of cholera can only be accounted for by the supposition of exhalations from the earth: there is something going on beneath the earth's surface that we know nothing about.

Mr. King sagaciously inquired, "whether any member of the Board of Health was present?"—and upon being answered in the negative, rejoined, in a solemn tone, "Then that is to be noted!" He then inquired, with equal solemnity, "Whether there was any evidence, or could any gentleman inform him whether there was any such thing as cholera at all in the world?"—(laughter.) He was then proceeding to panegyrise Dr. Barry, and to propose that the discussion on cholera should not be resumed till that gentleman's return from Petersburgh, when

Mr. Chinnock moved an adjournment of the discussion to Saturday next (this evening); which was carried by general consent.

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

BY

DR. ANTHONY TODD THOMSON.

LECTURE III.

October 27, 1831.

Rheumatism—Remittent Fever—Epilepsy.

Gentlemen,—Since our last lecture one of the cases of Rheumatism, that of Pennifather, has been discharged cured, and two others are likely to be so in a few days. In all of these cases the severe symptoms yielded to the use of the Calomel, Opium, and Colchicum, and little remained but slight indications of pain, preceded by rigor in the evening, for which the Sulphate of Quinia was ordered, and speedily removed them. In the case to which I alluded in my last lecture — that of Cadwell, the swelling of the wrists, and the pain excited by touching them, had greatly increased; but I pointed out to you the cause of this, in the intemperate habits of the patient, who smelt strongly of gin, and who acknowledged that she had indulged in a few glasses of that cordial, to comfort the weak state of her nerves under her sufferings. I take this opportunity, gentlemen, of pointing out to you, that it is only in persons who indulge in the abuse of stimulants that I have ever seen any thing like the co-existence of Gout and Rheumatism, forming that mixed disease which has been named Rheumatic Gout; and, although I am aware that Rheumatism makes its attack on the most temperate and regular habits, yet I can assure you that it is infinitely more difficult to cure it when the use of wine or spirits is continued during the treatment of the disease, than when these are altogether set aside. Indeed, I know scarcely any disease in which attention to diet and regimen is so essential as in Rheumatism.

The employment of Bark as a remedy in Rheumatism was in use as early as the time of Morton, who, although a bad authority in matters of theory, yet was an honest physician; and his word can be implicitly relied upon, in reference to the result of any mode of practice. His observations were afterwards confirmed by the experience of Sir Edward Huln, Sir John Pringle, and Dr. Fothergill; but it was not until the clinical experiments instituted by Dr. Haygarth, on the effects of this remedy in acute Rheumatism, that its value was fairly appreciated. Dr. Haygarth's practice was, first to reduce the febrile violence of the attack by antimonials, and then to throw in the Bark. He found that in no case did any mischievous effects follow its employment; and when manifest relief was not obtained at first, it was generally afterwards ensured by suspending the use of the medicine, and again having recourse to antimonials, until sufficient evacuations were procured. In such cases, the Bark was not again employed until the inflammatory symptoms were abated. The perusal of Dr. Haygarth's work, and an entire confidence in his details, induced me to give his plan a fair trial; and, with little variation, I have continued to follow it ever since. I have, certainly, seen the Bark do harm in acute Rheumatism; but then the precautions necessary to be observed previous to its administration, have not been duly observed. The substitution of the Sulphate of Quinia for the Bark has only added to our power over acute Rheumatism: sometimes, I have found it to be requisite to combine aromatics with the Sulphate; in which case every indication has been answered by the essential oil of Calamus Aromaticus rubbed up with Sugar, as an Oleo Saccharum, and added to the Sulphate. I was induced to prefer this aromatic to any other, from having observed the powerful influence of the root of Acorus *Calamus* in curing Agues, when used in the form of infusion, as a vehicle for the administration of the Bark. In Sussex the powder of this Acorus is used alone as a remedy for the cure of Agues. When Bark has failed to fulfil the intentions of the prescriber, Arsenic has been occasionally employed, after the attacks of the rheumatic pains have become decidedly intermittent. My experience of its efficacy does not authorise me to speak confidently in its favour.

With respect to the use of topical remedies, you will soon perceive that I very rarely order them. I have several times seen recessions of the complaint follow the employment of Rubefacients; and as, in such cases, we have no method of preventing the disease from falling upon a vital organ, it is better either to reject them altogether, or to employ them with the utmost circumspection. The only topical remedy which I have freely employed is the application of leeches to the pained part, followed by either emollient poultices, or the pediluvium, when the lower extremities are the seat of pain. The comfort of the patient is thus much improved; but topical bloodletting, however performed, does not contribute to the cure of the general disease.

From what I have already said respecting the bad effects of intemperance in retarding the cure of Rheumatism, it is scarcely necessary to force upon your attentions the propriety of prescribing a Milk Diet. Sydenham very properly interdicted the use of animal food and of all fermented liquors. Indeed, in every disease, if you reflect that remedies are intended to make a certain impression on the habit, you need not be informed, that the less this is interfered with, or interrupted by the administration of stimulant food, the more likely are the remedies to prove beneficial.

Before closing my remarks upon these cases of Rheumatism, you may expect that I should say something of Chronic Rheumatism, some cases of which are upon the books of this Dispensary; but, in my opinion, *Acute* and *Chronic* Rheumatism are the same disease, merely modified by duration; equally active as respects the symptoms: and, in fact, requiring the same plan of treatment. In private practice you will find this truth too frequently presenting itself to humble the vanity of the inexperienced practitioner: you will find patients labouring under Rheumatism after having suffered for ten or twelve months, who will inform you, that their pains are as severe as they were at the commencement of the attack; you will find the same hard, tense beat of the pulse; the same buffy coat on the blood; and the same pink deposit in the urine. Under these circumstances, however, the means that would have insured success, if employed in the early period of the disease, will not now succeed: and it is only under the administration of alterative doses of Calomel and Conium, in direct combination with Sulphate of Quinia, or some other tonic, such, for instance, as the Buckbean, Menyanthes *trifoliata*, that we can contemplate a salutary issue.

In long protracted cases, when the pains are confined to particular joints, I have seen much benefit derived from a plan imitative of the Douch Baths at Aix-les-Bains. On reading an account of these baths, which are of a temperature from 116° to 142° Fah., I was induced to form an opinion that the principle of their action was percussion in conjunction with a high temperature. The Douching apartments, which are caves of the rock, have the hot water conducted into them through channels, that terminate in tin tubes of about two inches in diameter. A large stream of water falling from a considerable height is thus directed upon the affected part, whilst the patient is immersed in an atmosphere of warm vapour. The sensation experienced resembles that of a severe cudgelling; but this is followed by decided relief, and the repetition of the Douching, at intervals of two or three days, generally produces a permanent cure. To imitate these baths, I set my patient in a chair, at the side of which is placed a bucket or large vessel capable of containing three or four gallons of boiling water, and envelop patient and all with a blanket, pinned close round his neck. In a few minutes he is bathed in a copious perspiration, and in this state he is directed to apply percussion to the pained joints by means of an elastic ball, made of cork covered with kid leather, and fixed on a handle of cane or whalebone, about a foot in length. I have seen the most decided benefit follow this practice; and one case, of seven years' standing, after the daily employment of this *dry douching*, if such an expression be allowable, my patient was beaten into excellent health, and threw aside a crutch, which he had for years been forced to use.

As a prophylactic, after an attack of Rheumatism has been allayed, you will find nothing so serviceable as the daily use of the Shower Bath. If the patient have been much reduced, and the re-action is likely to be feeble, the bath should not be taken until the system has been fortified with breakfast; but when the vigour of the habit is restored, it may be used immediately after rising in the morning.

Remittent Fever.

I have now to direct your attention, gentlemen, to the two cases of Remittent Fever which have lately been before you; the subject of one of them, Henry Schmidt, has been discharged cured; William Coates, that of the other, which is of a less decided character, has just been admitted. Schmidt is a German, 26 years of age, of a delicate frame of body, with black straight hair, hazel eyes, and a pale complexion. He is by trade a cabinet-maker. He informed you, that for some weeks past he has felt unwell, without being able to specify any complaint, except that his feelings differed from their usual state. About six days before his application to the Dispensary, he was attacked by a rigor, which was not followed by much increased heat of body, and by no sweating. It left him, however, extremely languid, and ever since he has been one day better and another worse; but on no one day has there been a complete intermission of symptoms. He has generally felt better in the morning; his uneasiness, accompanied with alternate heats and chills, gradually stealing on in the afternoon, and not declining until about midnight; but although these states were of daily recurrence, yet they were evidently more severe every other day. On the day of his admission, 18th October, his complaints were, loss of appetite and all relish for food; he stated that he suffered from a constant, although slight nausea; the tongue was much coated and dry; the bowels were confined; the urine natural; and the pulse, which was weak and compressible, beat 100. He had constant severe headache; the skin was clammy, the extremities were rather cold, and he mentioned that he felt occasionally ready to sink, and could not command his ideas. He slept heavily, but awoke unrefreshed. He was ordered a pill containing gr. vj. of Calomel, to be followed by a brisk Cathartic, and a mixture composed of four grains of Tartar emetic, in eight fluid ounces of Camphor mixture; three table spoonfuls to be taken every fifth hour. The pill and draught produced two stools only; but the tongue was cleaner, and the report says, "this morning (October 20th) he has perspired freely, which has brought down his pulse to 88. The Antimonial has not produced sickness; on the contrary, he feels less nausea, and his headache is gone." The pill, purging dose, and mixture, were continued. On the 22d he felt much better in every respect, and his countenance had lost the anxious expression which so strikingly marked it at his admission. The bowels were still torpid; but the skin was natural, the tongue much cleaner, and the urine natural. He was now ordered a mixture, consisting of ʒvj. of Sulphate of Magnesia, six fluid ounces of Infusion of Gentian, and one fluid drachm of diluted Sulphuric Acid, the fourth part of which was to be taken three times a day. Under the use of this mild aperient, a farinaceous diet, sub acid drink, and rest, he continued improving until this day (the 27th) when he was discharged cured.

Although this case, gentlemen, may be regarded as a mild specimen of the usual Autumnal Remittent of this climate, yet it presents some points for remark; and merits your consideration as a form of disease which is likely to come frequently under your care. The first circumstance in the case to which I would particularly recal your attention, is the peculiar physiognomy of the patient, which on his admission was so characteristic of general suffering, and of disease affecting the nervous system. The countenance, which was pale, sallow, and pinched, wore a look of great anxiety; whilst the eye was indicative of a disturbed state of the mind. The contrast between this expression and the lively, open countenance which the patient now wears, presents a theme for reflection too striking to have escaped your observation, and affords me also an opportunity of pointing out to you the great aids which the attentive study of the physiognomy of diseases affords us, both in diagnosis and in forming a prognostic. The second remark respects the evidence which you may draw from the symptoms of this case—the alternate heats and chills, the nausea, the muscular tremors and sinking, and the wavering state of mind—are evidence in support of the opinion that much of what constitutes fever is referrible to the state of the nervous system, in opposition to that theory which would refer all fevers to inflammation of one part or another of the body. In a fever, such as that now before us, there are no symptoms which would authorize us to fix upon one part of the body more than another as the seat of the disease. That the vascular system shares in the general derangement of the nervous, is obvious; and it is the conjoint affection of both which constitutes the disease.

On inquiry, I could trace this fever only to exposure to cold acting upon a weakened frame, predisposed to morbid impressions from depression of spirits, arising from want of employment and its necessary consequence—bad food, and a scanty supply of it. The poor man, who you know is a foreigner, had been for some weeks unemployed. There was a general state of languor prevailing; the functions were performed feebly, and torpor existing in some of the viscera, which, by throwing, as it were, the circulation off its balance, produced that irritable condition of the system which is so favourable to the production of fever. Thence vicissitudes in the weather, such as have lately occurred, succeeding to a warm summer, are likely to produce effects on the body which, in any other condition, it would resist.

With respect to the treatment pursued in this case, it was obvious that, with a compressible pulse at 100; with very little cerebral excitement, and without any symptom indicative of local inflammation, this case did not demand the abstraction of blood. The other case—that of William Coates—to which I have referred, presents evident symptoms of Pulmonary Inflammation, and, consequently, eighteen ounces of blood were taken from the arm with unequivocal advantage, and he bore the loss well. In Schmidt's case, the aspect of the countenance, the rigors and coldness of the extremities, indicating intestinal irritation, pointed out clearly the necessity of purging,—the only question was the kind of purgative to be employed; and, in ordering the administration of Tartar emetic, it was my object, not merely to evacuate the bowels, but also the stomach. I was fully aware that the severe pain of the head, which so closely resembled Meningitis that it would have authorized, in some minds, cupping or the application of leeches, depended solely on intestinal irritation; and, if the accuracy of the opinion may be judged of by the result, it was fully confirmed: besides, in evacuating freely the stomach and lower intestines, the habit, I hoped, would be prepared for the use of Sulphate of Quinia, should it be necessary in the decline of the disease. This, however, has not been requisite, the action of the Calomel and purgative on the liver having emptied the hepatic ducts, and improved the biliary secretion, and that the Antimony having restored the suspended action of the capillaries. Nothing further was, therefore, required to accelerate the return of health than to keep up a moderately soluble state of bowels, and gently promote the tone of the digestive organs; an intention which has been fully accomplished by the means prescribed.

Epilepsy.

Before concluding this lecture, I will merely direct your attention to the case of *Epilepsy* admitted on Thursday, and defer any comments on it until we shall have time to estimate correctly the value of the treatment which is now in progress.

Arthur Leni, æt. 37, a green-grocer, of a sanguine complexion, complains of what he terms fainting fits, which attack him once or twice a day, and throw him down in the streets, whilst pursuing his occupation. On inquiring into the previous history of the case, it appears that he had a severe Epileptic fit at fourteen years of age, but it was not succeeded by another until seven years afterwards, when he suffered a second attack, and this was followed by fits recurring once or twice in every subsequent year, until two years since, at which time they increased in frequency, and now he is scarcely during any day free from a repetition of the attack. The fit, he says, comes upon him unawares, throws him down, and he becomes insensible; but he is informed that he struggles much, and he has lately bit his tongue: he continues in the fit for a quarter of an hour, after which it is followed by profound sleep. On awaking he can scarcely distinguish the objects around him, and this loss of vision continues for some hours. He complains of severe and constant headache; there is no intolerance of light during the day, but he experiences something like it in candlelight; the conjunctiva is slightly inflamed. There is tinnitus aurium, like the constant ringing of bells; the tongue is furred; the pulse 92, small, and irritable; the bowels are open, and the urine is high-coloured.

He was ordered to be cupped behind the ears, and ℥viij. of blood only to be taken; eight grains of Calomel, and three of Extract of Henbane, were desired to be taken at bed-time, and followed by a brisk cathartic on the following morning. On the 29th, two days afterwards, he attended again at the Dispensary, and stated that he had had no fit since the cupping; but, although he admits that he is better, yet, he complains still greatly of his head, and of the tinnitus aurium; and adds, that he feels also a sensation, as if there was wind in the head. He feels now and then faint; his appetite is still impaired, and the tongue furred; the pulse is 84, and feeble; the urine is high-coloured. He has been ordered twelve pills, each containing one-fourth of a grain of Calomel, one grain and three-fourths of Camphor, and a grain and a half of Extract of Henbane: two to be taken every eighth hour: and in the intervals a purgative, containing ʒj. of wine of the seeds of Colchicum.

The most singular incident in this history is the long period which elapsed between the first and the second attack; and the rapid progress which the disease has lately displayed without any obvious cause. This disease is one which may arise from visceral irritation, and in that event is curable; but whether the present case be of that kind can only be determined by attentively watching its progress.

NOTE FROM MR. S. COOPER TO MR. COULSON.

To the Editor of the London Medical Gazette.

Sir,

I will thank you to insert the following note to Mr. Coulson in your next number, if perfectly convenient to you.—I am, sir,

Your obedient servant,
Samuel Cooper.

Dear Sir,—I beg to correct a mistake into which you have fallen, by stating in your lecture published in the Medical Gazette, that I had said nothing in my Dictionary, of the treatment of the patient previous to lithotomy. The account rather surprised me, and, at first, I imagined that the omission must really have been the fact; but, on referring to the two last editions, I observe that the subject has not been neglected, as may be seen by referring to page 803 of the 5th edition, and page 828 of the 6th or last.

With respect to the minute descriptions of the various modes of lithotomy, some of which are now obsolete, I regret that you do not appear to like them; but I and many other members of the profession feel an interest, and know that there is much utility, in the historical views of what has formerly been done in surgery. Were we always to pay due attention to them, we should not frequently suppose various schemes and practices to be quite new, merely because they are revived.

I remain, dear sir,
Yours very truly,
S. Cooper.

Great Russell-Street,
Oct. 29, 1831.

Wm. Coulson, Esq.

BOOK RECEIVED FOR REVIEW.

Elements of Anatomy, General, Special, and Comparative. From the Encyclopædia Britannica, Seventh Edition. By David Craigie, M.D. With Fourteen Plates.

W. Wilson, Printer, 57, Skinner-Street, London.

THE
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SATURDAY, NOVEMBER 12, 1831.

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LECTURES
ON
THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

BY DR. ELLIOTSON.

LECTURE VI.

Symptomatology (continued)—Indications afforded by the Pulse—by Examination of the different Regions of the Body—by the Excretions—Therapeutics.

I MENTIONED in the last lecture, gentlemen, that two parts of the body give us more information respecting, not their local affections, but the state of other parts, than those parts themselves, and that these were the face or the head altogether, and the hand.

I went over the chief symptoms which appear in the head, including the face, mouth, eyes, tongue—in fact, every thing above the neck; and the chief symptoms which were indicated by the hand: and at the conclusion of the lecture I was speaking of the information obtained by the pulse, which is observed more easily in the hand than elsewhere; not that the hand itself, or the wrist, has any peculiar quality of giving us information respecting the state of the circulation, although it is a great indication respecting other matters, but so it happens that the artery, (and it is a curious coincidence,) is to be felt better there than at any other part, and there we derive our chief information respecting the state of the circulation. The doctrine of the pulse is called *Sphygmica.*

I mentioned certain cautions which are necessary to be observed in noticing the pulse; namely, to examine the pulse of each wrist, and in some cases to examine it also at the heart. I mentioned likewise that it was necessary to take into consideration the age of the individual, the sex, the climate, the season; and it is likewise necessary to ascertain the immediate circumstances in which the patient is placed—whether he is in a room of high temperature or not—whether he is near the fire or at a distance from it; and also to ascertain what he has taken previously, because food accelerates the pulse, and the more stimulating its quality, of course the greater is the effect. You will observe different pulses at different hours of the day. It is necessary to take these matters into account, for if you were to find the pulse quicker than at your previous visit, and not to inquire whether the patient had taken food or not, or what other circumstance had occurred, you would be led into error. This is often a circumstance of great consequence; for if you find the acceleration of the pulse accounted for by the patient having taken food, your prognosis remains as favourable as before. This has always been observed; and you find in Celsus, what I dare say you all know, particular injunctions to medical practitioners not to feel the pulse immediately on going into the chamber of the sick, but to wait some time, to enter into conversation, and allow the flurry of the patient at the sight of his medical attendant to subside; for it continually happens, that the pulse will augment from seeing the medical attendant, more especially for the first time. You will find the advice given by Celsus on this head to be in a high degree worthy of attention. It may appear trivial, but I do assure you it is of the utmost importance, and you will be led into a great error, especially in a public institution, if you were to put your hand on the patient's wrist at once; it is better to ask some questions, and to remain some time before you feel the pulse.

However useful the information to be obtained from the pulse, if you neglect all these particulars, or if you take them into consideration and do not at the same time compare the other symptoms with the state of the pulse, the pulse will lead you into very great error. The pulse itself, it is to be observed, affords only one symptom, and that may not be at all in unison with the real state of the patient. The pulse would sometimes lead you to suppose that there is the greatest danger, when there is none; and, on the other hand, it would sometimes lead you into security, when the patient is in the most imminent danger. However, this is not peculiar to the pulse; there is scarcely a symptom, which, if taken alone, would not lead you to draw a false conclusion. It is necessary, in examining any case, to take the whole collection of symptoms together—to set one against the other: sometimes you find them all harmonize, and at other times you find more or less confusion, but it is from the balance of the whole that you form your diagnosis and prognosis. The pulse has been called, in the most ancient times, *res fallacissima,* the most false of all symptoms; but the truth is, all symptoms are delusive, if taken separately: it is the collection which enables us to form our judgment.

I mentioned the chief varieties of pulses—the varieties which I think sufficient for all practical purposes, and which are the only ones that I have observed. You find in books an extreme *minutiæ* on this point, and I think a great deal of absurd trifling. It is the Chinese, I understand, who have made the most minute observations on this subject: they describe all sorts of pulse, such as, I dare say, no one ever observed, and such as no one ever will.

Now as to these two parts of the body—the face, or rather the head altogether, and the hand—our observations upon them are chiefly made by sight and by touch; however, there is another sense that may be employed with respect to the head, and that is the sense of smell. The breath that proceeds from the mouth has sometimes peculiar odours, which, if we do not take pains to notice, we are compelled to observe.

The head and the hand, of course, give all the symptoms common to other parts of the body, when they themselves are affected. When either of these parts is inflamed, the various local symptoms present themselves It is not, however, of local symptoms that I have been speaking, but symptoms indicating the state of the other parts of the system in general.

To proceed, however, downwards: the *neck* affords a certain degree of information, but not much. A long thin neck is frequently taken, in conjunction with other symptoms, as a sign of phthisis; whereas, a short thick neck indicates a disposition to fulness of the head and chest. The pulse in the neck is also an indication of different diseases. In many affections of the head, the carotids will throb violently, so as to be seen at a distance; and, in many affections of the chest, the same circumstance is observed. Here, again, you see the necessity of attending to more symptoms than one. When you see a great throbbing of the carotids, you cannot tell whether it arises from an affection of the chest or of the head, for they both give rise to it. The pulsation of the jugulars is likewise frequently an indication of thoracic disease; a difficulty in the transmission of the blood through the chest will give rise to a great pulsation of the jugulars. I need not say that the tenderness of the neck indicates various diseases. By observing tenderness in that situation, you may frequently ascertain the existence of disease in the larynx, when otherwise you might be doubtful as to the nature of the affection.

With respect to the *chest,* it affords us a vast source of information; its sides and its shape indicate the general constitution. Phthisical patients generally have a narrow and flat chest: the chest is contracted in various directions—shallow and flat in the front, and narrow at the sides. If a person be disposed to apoplexy, from mere plethora and congestion, and in gouty persons, the chest is disposed to be circular. Frequently the chest is deformed, and frequently it acquires an enlargement in various parts, from disease in the pleura. When a collection of water or pus, particularly of pus, takes place in the pleura, it is not uncommon to find the sides of the chest become enlarged. It is, therefore, frequently of great importance in thoracic diseases to strip the patient, and look at his chest: you may frequently discover by the eye a slight deviation in its two sides, which you cannot observe by the touch. A deviation so slight, that it would escape you if you merely resorted to measurement, is frequently perceptible to the eye. Of course, the touch will give you considerable information with respect to tenderness; and also with respect to the state of respiration, and the state of the heart, by the thrill which you experience. In some cases of difficulty of respiration, you find under respiration a thrill; and in certain obstructions of the heart itself, you find, on pressing over a part of the heart, a similar circumstance takes place, corresponding with the action of the heart. In some cases of aneurism of the aorta, and of diseases of the heart, you find by the hand a strong pulsation.

But although the sight and the touch give you this information, there is a third sense which imparts especial aid with respect to diseases of the chest, and that is hearing. The function of the lungs differs from the function of many other parts in this circumstance—it is performed with a noise, and so, likewise, is the function of the heart. The function of the brain and of the liver goes on without noise; but it is peculiar to the function of the lungs and of the heart that a certain sound should take place. If this noise take place in health, in sound physiology, you must expect that when the function of these parts is disturbed, the physiological sound will become pathological, that the sound will be altered; and such actually is the case. If this be the case, it is our duty to attend to it; if it be not the case, let those who say so prove that they are correct. We declare it is so, and we ask those who entertain a contrary opinion to listen for themselves.

As to the lungs, you will sometimes find the healthy sound of respiration cease altogether. A lung becomes impervious at a certain part, the air will not go through it, and this may arise from an obstruction to the course of the blood into the lung; but it may arise from the lung itself becoming consolidated, or from external pressure of the lung, the dropsical pleura preventing the lung from expanding. However, you may ascertain by this that respiration is not going on in that spot. Besides an absolute want of sound in certain diseases of the chest, you have in others an unnatural sound; the sound of respiration does not cease, but becomes strangely altered. You have information, also, not by listening only, but by striking. If you strike the chest, the nature of the functions of the lungs in health causes a hollow sound to be heard: the lungs are filled with air, and a hollow sound is necessarily produced. If something take the place of air—if the lungs become consolidated, or filled with fluid, or surrounded by it—it stands to reason that if you then strike that part of the lung, the air being absent, you will have a dull sound. This actually is the case, these things are physical necessities, they occur simply from the common law of physics; they must occur, and if they do occur we ought to observe them.

However, nothing would be more absurd than to attend to these auricular symptoms solely. The fault of those who merely employ the ear in diseases of the chest is just as great as the fault of those who do not employ it at all. There is a sufficient number of other symptoms frequently to point out the nature of the affection without employing the ear, but whether that is the case or not, it is as much our duty to attend to other symptoms as to those that we learn from the ear. You will, by looking at a patient while he is breathing, frequently find what is the state of respiration. You find that, like the pulse it is more frequent than natural, or not so frequent as natural; we may have the *respiratio frequens* and *respiratio rara*, corresponding with the pulsus frequens and pulsus rarus. Now and then we find respiration performed suddenly, the inspiration and the expiration are sudden—and that is called *celer*; and the opposite state, where the chest expands slowly, is called *respiratio tarda*, corresponding with the pulsus celer and pulsus tardus. These things may be learned simply by the sight. The respiration undergoes exactly such alterations as the pulse; it may be regular or irregular, it may be slow or quick, sudden or gradual. It may be deep or exceedingly shallow, little air being taken in at a time, and, on the other hand, you may have a full respiration. It may be full and deep, corresponding with a full and strong pulse. I do not mean to say that the function of the lungs and the pulse take place synchronously; but, as you have in the pulse quickness and slowness, fulness and smallness, jirking and sluggishness, so in respiration we have frequency and slowness, shallowness and depth, suddenness and sluggishness, irregularity, and so on.

When respiration takes place with difficulty, it is called, in medical language, *dyspnœa*: and if it be so difficult that the patient cannot breathe unless he sit upright, then it is called *orthopnœa*. Orthopnœa is merely an intense dyspnœa.

Independently, however, of the frequency of respiration or its slowness, you may hear particular sounds. You may hear a wheezing sound, which is called *sibilous*, or you may sometimes hear it performed with a snorting sound, which is called *sonorous*. Even without putting the ear to the chest, or employing a piece of wood as an intervening substance, it may sometimes be heard when standing at a little distance.

With respect to the *voice*, that affords you also much information. The voice is frequently suppressed, faint, harsh, and shrill; and these various changes will indicate great debility, or they will indicate particular diseases of the lungs themselves, or of the larynx, or of the air passages at large. From listening to the voice with the ear, in contact with the chest, or with the intervention of a piece of wood, or any similar substance, you will frequently ascertain that the voice does not sound as it sounds in general; so that you are able to say that an excavation exists in the lungs.

From a *cough*, too, with the naked ear we learn much, and the varieties generally enumerated are slight, severe, harsh, crowing, hooping, shrieking, tearing and hacking, loose and dry. There is an infinite variety with respect to coughs. Slight, severe, crowing, and hooping, I need say nothing about. You occasionally hear a cough tearing the patient to pieces, and it frequently indicates more disposition to spasm than actual inflammation. Now and then I have heard a cough which has been followed regularly by a shriek, so as to be absolutely alarming. At the end of every set of expirations, I lately heard one deep inspiration, not attended by a hoop, as the hooping-cough, but by a shriek, as though the individual were about to have a dagger plunged into her.

In regard to the *heart*, you have great information by examining the chest itself; not only by the touch do you ascertain the force of the heart, but by listening you hear various alterations in its sound. Here again it is very necessary not to attend to any one symptom, or to the occurrence of any one sound, but to attend to the whole set of symptoms. Instead of the usual double sound which you hear in a healthy person, when the heart is affected in a certain way you hear a sound as if a pair of bellows were blowing, sometimes as if a file were in action, sometimes as if a substance were being rasped. If you attend to these only you may be led into error; but you frequently, at the same time, observe great difficulty of breathing on the slightest motion; you observe a swelling of the legs and a deficiency of urine, and you cannot but believe that there is organic disease. In other cases you only hear the sound, and then you would not be justified in saying there was organic disease, unless you listened time after time and always found the same occurrence. In observing these sounds of the heart, the greatest care is necessary not to form a judgment at your first visit. You may, by attending to the general symptoms, also see a sufficient proof of organic disease, but frequently there are no other circumstances present. These auricular sounds may arise from temporary causes, and you will not hear them two successive days. This shews that these symptoms, like those of the pulse, and almost every other, are never to be taken alone, but only in conjunction with other symptoms, and only when they have been dwelt upon carefully and repeatedly.

With respect to the *abdomen*—to descend still farther—the chief symptoms in that region are to be observed by the sight and the touch. The hearing gives you no great information respecting the abdomen, except in the circumstance of striking it, if it be enlarged. If you strike it, especially with the intervention of some little substance, either the finger or a piece of ivory, you learn whether the tumor arise from liquids or solids, by its emitting a dead sound as if you struck the thigh; whereas, if it arise from mere air, you find it sound hollow, like a drum. By going over the abdomen carefully, you may ascertain whether there is enlargement of any organ; and you may ascertain the shape of an enlarged liver with the greatest accuracy, the dulness occurring where you strike over the enlarged organ, and the hollow sound where the abdomen is in its natural state. This is the only point in which the ear is of much assistance. The ear is of the greatest use in diseases of the chest; but in the abdomen you must more particularly have recourse to sight and touch.

Frequently, if you expose a patient in the case of abdominal disease, you may discover an alteration of figure which the hand cannot at once detect. Nothing can be more absurd than for a practitioner to prescribe at once for a patient who comes to his house for diseases of the chest and of the abdomen. It is very well to prescribe in that way for diseases of the skin, the head, and other affections; but with respect to diseases of the chest and the abdomen, if, in many cases, you do not listen, or have the patients more or less undressed, it is impossible to form a correct opinion. You may form a good guess, and you may be right; but you will not be able to form such an opinion as a philosophic mind would always wish.

In regard to the abdomen, by the sight you may discover enlargement, either local or general; but it is by the touch certainly that you acquire the greatest information altogether. You may feel an enlargement of various parts; you may feel the induration of different viscera; and, if there be fluid, you may ascertain its existence by the occurrence of fluctuation. But one great use of employing the hand to the abdomen is to ascertain the existence of inflammation; inflammations of the abdomen are detected easily in general—from the circumstance of the abdomen having no bony coats—by the hand pressing upon it; and it is always necessary, in acute diseases, carefully to examine the abdomen by means of the hand. In fever, and various affections of the alimentary canal particularly, if you do not carefully go over the abdomen with the hand, you may allow inflammation to escape you, which, if neglected, may prove dangerous. In fever, at any rate, it is right every day or two to feel the abdomen carefully, beginning at the epigastrium, which is the part most frequently affected, and going more or less all over it; but in every case where the abdomen is affected, you find it of the greatest importance to make these minute observations. It is nonsense to say that a physician is to use his pen and a surgeon his hand. Every physician must be more or less a surgeon, and every surgeon must be more or less a physician. It is as impossible for a physician to do without the use of his hand in the case of the abdomen, as it is for a surgeon in the case of fractures; and it is not creditable to any physician to pride himself upon not making these mechanical examinations, as I believe they are contemptuously called.

With respect to the *excretions* of the body, the sight and the smell give us the chief information. It is by the sight that we judge chiefly of the character both of the fæces and the urine; the smell, however, gives certain information in these particulars, as it also does with regard to the excretion of sweat. The sweat in many diseases is exceedingly sour, and in others it has peculiar smells, of which I shall hereafter speak. However, we sometimes are more minute in our observations respecting the urine; at any rate, it is necessary not to depend merely upon the sight and upon the smell, but to employ chemical means for ascertaining its quality. It is frequently requisite to ascertain whether the urine is acid, or neutral, or alkaline; it is right frequently to see whether it contains albumen or not; and in other cases it is necessary to ascertain whether it contains sugar. The most minute observations are sometimes necessary; and, of course, it is only with respect to the excretions that chemical minuteness is at all of service.

As to one sense, that of taste, I believe it can only be employed with regard to excretions, and I presume but with one of them—the urine. Some go so far in professional enthusiasm as to wish us to taste every thing; but it is not usual except in the case of the urine, and then, for the most part, it is better to get the patient to taste for himself. However, this is not *absolutely* necessary with regard to the urine, because by evaporation, and by weighing the urine, you can easily ascertain whether it contains sugar or not. I never yet in my life tasted it, but I have no objection, in diabetes, to taste the saccharine extract, for that is not urine.

Having gone through the chief of the organs of the body, I may say that the *whole surface* together gives such information as the head, but in a very inferior degree; we gain certain information, but it is undoubtedly far less than we gain from simply inspecting the face. From examining the whole body we learn whether there is a degree of fulness or emaciation; by observing its plumpness we can tell whether the patient is in condition or not. We observe likewise the dryness or the moisture of the skin, and the firmness of the flesh. We observe whether it is swollen or not by dropsy. It is to the surface of the body that we employ the sense of smell, in regard to the excretion of sweat. On the surface of the body we can ascertain the increased temperature of the patient; and for more minute observation a thermometer is employed. It is generally put into the *hand*, one of the two principal parts, or put under the tongue—a portion of the *head*. Every part of the surface of the body has its own affections; and it is there that not only the chief symptoms of cutaneous diseases—the symptoms of all its own affections, are to be found, but there often we observe affections of parts below. In inflammation, for example, of the dura mater from an injury, the surface immediately above frequently becomes œdematous. We learn the state not only of the skin itself locally, but of parts beneath it, by looking at the skin immediately superjacent.

These are the chief observations which I have thought it necessary to make, generally speaking, on the third branch of general pathology, called semeiology, or symptomatology.

The fourth division of general pathology is *therapeia*, or the general treatment and prevention of diseases.

Now in regard to most diseases nature has a tendency to get rid of them. It is ordained by providence, to a great extent, that the injurious causes to which we are exposed should have but a temporary effect, the body having the power of resistance, or they either existing temporarily, being applied but temporarily to the body, or being able to exert no more than a temporary influence. In this way it is that, generally speaking, we may say, although there are many exceptions, there is a tendency to shake off disease. This power of the body to shake off its morbid state is called by writers the *vis medicatrix naturæ*, or *autocrateia*. For example, if any thing too acrid is taken into the mouth, a great flow of mucus and saliva is the consequence, which has a tendency to dilute the acrid matter and wash it away. So with respect to acrid matters taken into the stomach; the stomach has a tendency to reject them; or if they be passed through the stomach into the intestines, the intestines are excited to action, and they are got rid of. This is the general tendency of the frame.

Again, many causes of disease are only temporary. A person is exposed to a great cause of disease; the cause ceases, and, of course, in many instances the effect ceases likewise. It is not a necessary consequence, however, that the effect should cease because the cause is removed; but it very frequently thus happens. Again, there are other causes which cannot produce an influence upon the body, however long they are applied, for more than a certain period. If you take the poison of small pox, it produces a disease of only a certain duration; the patient may die, but if not, it can only last for a certain time; and when the disease has once occurred, the body, in the greater number of cases, becomes insusceptible of it again. These are the various means which nature has provided for getting rid of disease—for getting rid of noxious causes before they produce actual disease, and for getting rid of diseases themselves.

Some have so great a confidence in nature, that they leave every disease to itself, and that sort of treatment is called *the medicine of expectation—medicine expectante.* Such treatment does well in many cases; it is often the best in many cases, both medical and surgical, but it is totally inapplicable to a large number. No one with a violent inflammation of the lungs would consent to sit down quietly in the hope that he would grow better day by day, when he knows that he may obtain immediate ease from the lancet, and get rid of the complaint. When a person has taken poison into his stomach, of course he would be mad to wait for nature to effect a cure; he would send for the physician's medicine, or for the surgeon's stomach-pump.

Now the treatment of the diseases of the human body is either preventive or curative; we either attempt to prevent disease from occurring, or we attempt to remove it; and when we cannot remove it we attempt to palliate it. The treatment of the human body in the way of preventing it becoming the subject of disease is called *prophylactic*; that which attempts to cure disease is called *curative;* and when we cannot attempt to cure a disease, still we may do a great deal in the way of palliation,—so that medicine is prophylactic, curative, and palliative. It is a great point very often to enable a person to live, while he must live, in comfort and happiness; and if he must die, it is a great point to enable him to die easily. In many cases we have to adopt both curative and palliative treatment together—that is to say, we aim at curing the disease, but we adopt palliative measures all the time, for the purpose of lessening any one symptom, or particular symptoms, which it is a desirable object to get rid of, or lessen, before the cure of the disease enables us to do so. For example, in the case of inflammation of the lungs, although we may attempt to cure the disease by bleeding, it is sometimes of the greatest importance to put an early stop to a violent cough by a narcotic.

In the treatment of diseases, whether it be prophylactic, (and it can hardly be called the treatment of disease when no disease exists; but we must allow an extended use of terms,) whether our means are to prevent diseases,—prophylactic, or whether they are to cure them,—curative; or whether they are to palliate them,—palliative; they may be of two kinds—they may be *rational* or they may be *empirical.* In the treatment of many diseases—not only in their prevention—we act rationally, we proceed upon general principles, and the whole treatment is in the highest degree philosophical. We make an accurate diagnosis, in the first instance; we then see what is the wrong state of the system, and we employ such means as are evidently calculated to remove that state. But in certain diseases, and in certain varieties of disease, we are obliged to act empirically —to act in a certain way without knowing why we are likely to be successful.

Now this empirical mode of practice is of two kinds—that is, sometimes we have a specific remedy for a disease without knowing why, and, in other cases, it is impossible to know the exact variety and character of the disease, and we fire a shot at random that can do no harm, but which may do good. The treatment of inflammation is in the highest degree rational, the treatment of various spasms is the same, and the treatment of continued fever, too, I must think altogether rational. The treatment, however, of ague, of itch, and of syphilis, is empirical; for it is impossible to know why a few grains of sulphate of quinine cure ague, why a few grains of sulphur will cure the itch, and why syphilis will cease sooner if you exhibit mercury than if you do not. No one would *à priori* imagine that any of these remedies could have such a peculiar property; there is nothing in the nature of the disease, or in the nature of the remedy, that would lead us to any such conclusions. We also are obliged to be empirical frequently where we cannot ascertain the proximate cause of a disease. For instance, epilepsy arises, in cases innumerable, from an unknown cause situated in the head, or in distant parts. If we can ascertain that it arises from irritation, our treatment is rational, we remove this; if we can ascertain that it arises from an inflammatory state of the system, our treatment is rational, we remove this; but sometimes we cannot conceive that it arises from either of these sources, and then we use certain remedies that are known occasionally to do good; not that they are a specific against epilepsy, as bark is against ague, or sulphur against itch, but because they continually do good in the disease. They are not specifics, for they often will not cure one case out of many, and in many they are totally inapplicable, but still they frequently do cure the affection. I allude to copper, zinc, nitrate of silver, &c.

The mode of treatment which is pointed out from the circumstances of the case, is called the *indication*; and things which the nature of the case forbid us to do, are called *contra-indications.* To discover the indications which are necessary in the treatment of a disease, we must make a careful diagnosis of the nature of the case; then we must make an equally careful diagnosis as to the variety of the particular disease; and then we ascertain the stage of the affection, the exact strength of the patient, and the incidental circumstances—for example, we ascertain whether the place in which he lives at all accounts for his disease, or whether the season of the year is such as is likely to have affected him. We must likewise observe whether there is any peculiar constitution of the period. Sydenham used to mention the character of epidemics as the constitution of the year. Continued fever has sometimes been attended by so much tendency to debility, that evacuations were in the highest degree improper, and the patients did much better when they were allowed to take merely simple food, with scarcely any other treatment. This has been the case during several months lately. If the patient were left alone, or treated with what in truth is no treatment at all, he has in most cases done well. If the least evacuation were attempted, the disposition of the disease was to great prostration of strength and rapid sinking. It is necessary, therefore, to know the constitution of the period; and, but for this, I certainly should have destroyed many patients by treating them as I had been accustomed to do; however, I was warned by the experience of others, and by careful observation of what was going on.

It is necessary also to know what are the contra-indications—whether there is any peculiarity in the patient. Some patients will not bear a remedy which is very appropriate to a disease: the disease may appear evidently to point out the necessity of a certain mode of treatment, but a peculiarity in the individual may render it exceedingly improper. This peculiarity sometimes relates not only to particular medicines, but to particular articles of food; and sometimes there may be some other disease present which forbids the employment of a remedy: for example, if a person have a hernia which cannot be well kept up, and you wish to give an emetic, you would try to do without it, lest you should force the intestines down too violently. You therefore see that you are not to be satisfied when you have given a name to a disease. It is a great point to make a good general diagnosis, but that will not do alone; you must make a more accurate diagnosis still; you must ascertain its minute variety, individual peculiarities, and all the other circumstances that I have just now mentioned.

In regard to the things indicated, they are divided generally into two classes—*regimen* and *pharmaceutical* means; the former consisting of injunctions in regard to the temperature of the place, to food, to exercise, and rest, and every thing of that description; and the latter respecting drugs. In prescribing medicines, it is necessary to recollect that, however excellent they may be, they are frequently rendered abortive by our want of attention to something else. There can be no doubt of the use of a large number of remedies, both drugs of the Pharmacopœia, and other remedial means; but I have seen them fail, not through any fault in themselves, but through something else that was not attended to when they were employed: for example, if mercury be given to arrest a violent acute inflammation, you cannot expect it to have that effect if you neglect to bleed the patient; and if, in addition to giving mercury, you do bleed him, you cannot expect to succeed unless you are particular with regard to diet. So, in diseases of the skin, there are many remedies which have a particular operation which we cannot explain; they are not exactly specifics, but they do great good; yet the skin, in the midst of these diseases, may be in an inflammatory state, and if you do not bleed and enjoin low diet you cannot remove the affection. As no one symptom can be depended upon, so it is with remedies; and when you prescribe a remedy for a patient, you must take into consideration all the other points on which it is necessary to give directions.

Notwithstanding, however, the most scrupulous attention to all the circumstances I have just enumerated, you will often be disappointed in the cure of a disease; but you are not to be dismayed on that account. Our profession is capable of effecting the greatest good, chiefly in the way of preventing disease. By improvements with respect to cleanliness, with respect to air, with respect to food, and every thing else of that description, diseases are now scarcely known which formerly prevailed to a great extent; and we can do absolute good to a large amount in curing disease when it has occurred. A great many diseases are inflammatory; and over inflammation, by means of bleeding and other remedies, we have the greatest power. When we cannot eradicate a disease, still it is of great importance, and it is also a great blessing, to be able to mitigate pain; and it is to be remembered, that although we fail in the cure of many affections, yet, if we make the attempt, by degrees more knowledge will be attained, and those who follow us will be able to do what we cannot.

ON THE TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

Should you deem the following hasty remarks worthy of insertion in your valuable publication, they are at your service. E. Stanley, M.D.

Brighton, Nov. 2, 1831.

In the treatment of cholera, it appears to me that sufficient stress has not been laid on the suppression of urine, and state of the urinary system, as shewn by dissections. If we consider the baneful effects caused by the constituents of urine being thrown back on the system, we shall, I think, be enabled in a great measure to account for some of the worst symptoms of the disease. Again, those remedies which at times have proved successful, such as diluents, calomel with opium (a combination which, by preventing the former from running off by the bowels, may have operated on the urinary system,) hot baths and emetics, (by either of which a vicarious outlet by the skin was established,) &c. may have owed their efficacy to their directly or indirectly obviating that suppression. Besides, a return of the secretion and evacuation of urine being one of the most favourable prognostics, I should direct my immediate attention to a vigorous employment of diuretics:—oil of turpentine, in doses of ʒj.—ij. repeated at short intervals, perhaps in combination with other essential oils, tinct. canthar. and other active medicines of that class; hot fomentations, blisters, or boiling water, to the perinæum; embrocations of oil of turpentine and cajeput, &c. to the spine, loins, and pubes, with a turpentine clyster, might, as far as practicable, be employed in conjunction with means calculated to cause a determination to the surface, (such as vapour or hot bath, &c.); and general bleeding, if not evidently counterindicated, or, as the immediate abstraction of blood from the vena portæ system would seem desirable, leeches to the anus and right hypochondrium, or cupping to the latter. In some cases, especially where there is little or no vomiting, I should feel inclined to try the powerfully relaxant and revellent effects of tartarized antimony, in large doses, at shorter or longer intervals. From the colour of the blood, I am induced to believe the inhalation of oxygen would prove beneficial, and the depression of the vital powers might, perhaps, be relieved by the inhalation of nitrous oxide gas. As one of the remote causes of cholera may be a change in the electrical qualities of the atmosphere, electricity or galvanism, particularly the latter, might prove valuable auxiliaries. For the purposes of fumigation, the muriate of soda and sulphuric acid are, perhaps, the most convenient articles.

With respect to the question, whether cholera is contagious or not, I think it difficult in the extreme to come to a decisive conclusion; perhaps, it is idle to attempt it. The effluvia on which contagion seems to depend, probably vary in their quality and tendency to reproduce the disease, as much as symptoms do in number and degree; and again, the susceptibility of those exposed to contagion, may be diminished or increased by numerous circumstances. Facts have almost indubitably shewn the disease, in many instances, to have been propagated by contagion; other more recent facts have proved that numbers have been exposed to it without becoming infected. This ought to convince the unbiassed, that the only conclusion we can arrive at is, that cholera may be either contagious or not, according to circumstances. Hence, while we ought not to neglect measures of precaution, we should at the same time use every means in our power to divest this fatal scourge of some of its terrors, and consequently to render individuals less susceptible of it, by dwelling on the fact, that many have escaped infection under very unfavourable circumstances, as well as on the probability, that if it should reach our shores at all, it may in this latitude appear in a milder form, both as to its contagious and other characters.

NON-MEDICAL TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

As all facts relating to the treatment of cholera are useful, whether derived from medical sources or otherwise, I send you the remedies resorted to by the Jews of Wiesnitz, who suffered less during the existence of the disease there, than other sects. My information is from a private letter to me, from one of the principal bankers and merchants of Frankfort S. M. and I give you the quotation *literatim*:—

"One pint of strong Spirits of Wine;
One pint of good White Wine Vinegar;
One ounce of powdered Camphor;
One ounce of flour of Mustard, or bruised Mustard Seeds;
Half an ounce of ground Pepper;
A full teaspoonful of bruised Garlick;
Half an ounce of powdered Cantharides;
Mix well in a closed bottle, and expose it to the sun, or some equal warmth, for 12 hours, frequently shaking it.

"Let the patient be immediately put to bed, under warmed blankets (cover-lids,) and let his hands and feet be rubbed powerfully and uninterruptedly with the above lotion, after it has been made warm. During this operation, let the patient take a glass of strong drink, composed of two parts of camomile flowers, and one part of balm of mint," (I suppose, of the infusion.) "Persevere in this course, and at the end of fifteen minutes, at the utmost, he will break out into a profuse perspiration,—the patient's head and body being kept well covered beneath the bed clothes. The patient must be kept in this state between two and three hours, and must not fall asleep. After this, remove the extra covering from the bed, and he will fall into a slumber, which may last from six to eight hours, and be accompanied by a gentle perspiration. When he awakes, he will feel weak, but the disease will have entirely left him, and he will require but rest and moderate diet to restore him.

"After rubbing, the patient must not have a single finger above the clothes,—the slightest chill would cause his death.

"When the cramps in the stomach come on, we apply very hot dry bandages of bran and ashes to the pit of the stomach, and, when necessary, a bladder of hot water to the region of the navel.

"The great point is, to produce strong perspiration, and to restore the circulation, which is drawn from the surface of the body, and thrown with frightful virulence on inward parts."

Note.—I have learnt, also, that these intelligent people used camphor extensively about their persons and apartments long before, and during the raging of the disease about them.

I send you this non-medical communication without comment; the medical reader will know how to appreciate it, and the general reader will derive advantage from its perusal.

I have, &c.
JOHN S. GASKOIN.

Clarges-Street, Nov. 5th, 1831.

REMARKS
ON
INJURY OF THE LENS.

By R. T. Hunt,
Assistant Surgeon to the Eye Institution, and Surgeon to the Lying-in Hospital, Manchester.

As the cases of injury of the lens, related by Mr. Mackenzie, comprise facts which are peculiarly illustrative of the advantages of extraction, in such injuries, I trust the following observations will not be considered entirely inapplicable.

The extraction of the lens when dislocated from its capsule, and lying in the anterior chamber, is a practice which has generally been advocated by those who have particularly attended to ophthalmic surgery, and indeed extraction of the cataract seems to have been first attempted in consequence of Petit's extracting an opaque lens, which had become so displaced as to occupy the anterior chamber. This plan of treatment appears, however, to have been confined to those cases in which dislocation of the lens and its consequences were the only injuries the eye laboured under. I am not aware that any writer had noticed the utility of early extraction in injuries of the eye, until the appearance of Mr. Barton's paper on that subject, in the Medical Gazette of March 20th, 1830. Since that period, a considerable number of complicated injuries of this organ have been thus treated at the Manchester Eye Institution, and the general result is such as will at least entitle this method to further consideration. It may be right to mention, that nine of these cases were published in the 4th No. of the North of England Medical and Surgical Journal, in a "Report of the advantages of extracting the Lens in such injuries."

When a considerable period has elapsed after the accident, previously to the examination of the patient, there is much difficulty in determining which of the diseased changes in other parts have been produced by injury simultaneous with that of the lens, and which are to be considered as consequences of the lens' displacement. For instance, a blow on the eye dislocates the lens without immediately affecting the retina; this latter structure, however, gradually suffers, in consequence of the lens remaining dislodged, and amaurosis is the result. In another case, the blow which dislocates the lens causes concussion of the retina, and instant amaurosis. Suppose they are examined six weeks afterwards, when the injured eyes of both patients have become amaurotic; how can an observer then distinguish one case from the other? But let the lens be extracted, in such an instance as the former, as early after the accident as is possible; this will certainly prevent that injury of the retina which the displacement of the lens from its natural situation is stated as having produced. I have stated that amaurosis may occur, not only as an accompaniment of dislocated lens, but also as a consequence of such an injury. This is rendered more than probable by the frequent supervening of this disease upon the operation for depression, in which the lens is dislodged with much less injury to other structures than when its displacement is caused by a blow. After the operation, patients frequently possess good vision for a time, which subsequently deteriorates, the case terminating in amaurosis. When, therefore, greater violence causes the dislocation of the lens, it is not assuming too much to state, that there will be at least an equal risk of the retina becoming affected. The lens was extracted in three of Mr. Mackenzie's cases; but although its removal appears, in every instance, to have relieved the pain and irritation which had previously existed, yet very little improvement of vision resulted, owing probably to the length of time which had elapsed before this practice was resorted to; for as the lens produced such severe symptoms, after the eye had been so long accustomed to its new position, it is evidently reasonable to conclude, that an equal, if not greater, degree of irritation must have existed soon after the occurrence of the injury. The author recommends extraction, not only in cases of dislocation of the lens, where other parts are uninjured except by its displacement, but also where other structures (the cornea, for instance) are implicated in the injury; but this recommendation is not clearly stated; it is merely implied in the treatment adopted, particularly in Case III., and in the observation, that "The lens was extracted by Mr. Rainy with the same striking relief which extraction generally affords in such cases." Upon carefully examining Mr. Mackenzie's Practical Treatise on the Diseases of the Eye, in which no such observation occurs, I conclude that the practice has been adopted since his publication of that useful work; and it would be a great addition to the evidence, of the advantages of extraction in such instances, already before the profession, if one, who has long possessed and still retains such ample opportunities for the observation of diseases and injuries of the eye, would make public his favourable report of such treatment.

Whilst writing the foregoing observations, a very unusual case of dislocation of the lens came under my notice, viz. that which Mr. Mackenzie calls the sixth variety of this injury.

Thomas Siddal, a weaver, ætat. 66, received a severe blow on the eye on October 18th, which caused much swelling of the eyelids, &c. After this had subsided, he found his vision injured, and applied at the Institution on the 24th. There is a semi-transparent tumor at the upper part of the eyeball, of a roundish form, about the size of a large bean, apparently produced by some body lying between the conjunctiva and globe, immediately behind the ciliary ligament. The cornea is perfectly free from injury; the iris detached from the ciliary ligament on the temporal side, and drawn upwards towards the swelling already noticed, so as to distort the pupil, which is rendered still more irregular by effused blood adhering to the lacerated iris. There is a slight degree of intolerance of light, and considerable vascularity of the sclerotica and conjunctiva; but the pain is much less severe than might be expected from the extent of injury. There is no constitutional irritation.

Considering the tumor as formed by the dislocated lens, I passed a cornea knife through the distended conjunctiva, when the lens was easily removed entire; it was perfectly transparent, and of the yellow colour usually found in advanced age. Its removal was not attended by any escape of either vitreous or aqueous humour, and the lids were kept closed by court plaister, in the usual manner.

Oct. 28th.—The progress of the case, up to the present time, is very favourable. He can now distinguish objects, and there is no appearance of opacity in the pupil; but when the extent of injury and the age of the individual are considered, it would not be judicious to expect that even the present degree of vision will remain. I shall reserve a more accurate report of the case until I am enabled to state the condition of the eye when a considerable period has elapsed since the occurrence of the accident.

OBSERVATIONS

ON THE

DEAF AND DUMB.

By John Harrison Curtis, Esq.

The condition of the deaf and dumb has of late become a subject of more than usual interest. An opinion having long prevailed that such afflictions are of rare occurrence, the public sympathy has been powerfully excited by the correct statement promulged respecting the comparatively large proportion of individuals labouring under partial or total privation of hearing, and of speech, in the united kingdom. For the reception and maintenance of sufferers, so strongly entitled to the compassionate aid of the benevolent, many institutions have been founded, but they have all proved inadequate to prepare the deaf and dumb for general intercourse with society. Another disadvantage attending them is, the regulation by which no applicant is admitted before the age of nine years, for it not unfrequently happens[*] that four, five, and even seven children in one family are so afflicted as to possess equal claims on these specific charities.

Having been led to consider the great number of sufferers, in comparison with the establishments provided for their reception, I ventured to propose a plan, which was first suggested by me in 1817, to the Governors of the London Deaf and Dumb Asylum. On that occasion I recommended, as a measure of primary importance, a minute examination of all children born deaf and dumb, with the proviso, that for every child presented for admission, there should be produced the report of a surgeon upon its case, as being curable or otherwise, and I advised that none but incurable cases should be admitted. Under this regulation, it appeared to me that the worst cases would be most certain of reception; and that in all curable cases an opportunity would be given for trying such methods as might appear best calculated to afford relief, if not effect a perfect cure.

From the abstract of a report made by M. Husson to the Royal Academy of Medicine in Paris, on the method adopted by M. Itard for the cure of the deaf and dumb, it appears that M. Itard has presented to the minister of the interior *three memorials;* the first relating to the various methods hitherto employed for the cure of congenital deafness, including those pursued by the author himself during a long course of practice; the second, containing a detail of experimental treatment adopted in nearly two hundred cases, with the view of determining the advantages and disadvantages of injections through the eustachian tube into the internal ear, an expedient apparently recommended to public confidence in a recent report of the institution; and the third, adducing arguments against the last-mentioned process, and proposing one which M. Itard considers as exclusively entitled to preference, being a medico-physiological method, calculated in his estimation to afford relief in very many cases of congenital deafness. To the last of these alone the present address refers, for according to M. Itard, absolute deafness is extremely rare; he admits not more than one-fifth of the cases that occur to come under that definition. Every endeavour, then, should be made, to relieve any curable defect or obstruction, before a case is abandoned as hopeless. It should be recollected, that to restore to society one who suffers from accidental causes, and to render him useful by the scientific development of his natural powers, is much more satisfactory and laudable than to lead him through the indirect and exclusive forms of an artificial and symbolical education, however expedient this may be with regard to desperate cases, for which all practicable resources have been tried, and have proved unavailing. In offering these observations, as confirmed by daily practice and experience, I have no hesitation in declaring, that the more I see of these afflicting cases, the more I am convinced that much relief may be administered, and that not one should be relinquished as desperate until all means have been tried to remove the defect. In very young children I have found the most frequent causes of deafness to arise from obstructions of the eustachian tube—from various affections of the membrane of the tympanum—from viscid mucus impacted in the bottom of the meatus, and, not unfrequently, from those herpetic eruptions which often afflict infants during dentition. Notwithstanding the general advancement of medicine and surgery, and the improved methods of cure for various diseases, it is to be regretted that those of the ear, though not numerous, are attended with more inconvenience than any others; and hence, until of late years, little attention has been paid to their relief, or, at least, little skill or ingenuity has been employed in multiplying and varying the expedients likely to attain that object. In considering the chief obstacles that present themselves, it is to be observed in the first place, that the structure of the ear is extremely complicated, the parts that compose it are principally internal, and the diseases that assail it chiefly concealed from view: hence, in very many cases, arise the uncertainty of diagnosis, and the difficulty of adopting a proper course of practice. In the second place is to be noticed the extraordinary prejudice which, ascribing all congenital deafness to malformation of the organ of hearing, denounces the disease as incurable, and depreciates all attempts at relief as at once nugatory and fallacious, or, at least, painfully uncertain. A third objection, which has been often urged, is the difficulty of applying remedies immediately to the parts affected. Yet means of relief may be employed without inconvenience, and often with success. Congenital deafness, and that which is observable soon after birth, frequently depends upon casual circumstances. In the ear, as in every other organ of the body, cases of defective formation may present themselves; and in these, deafness is incurable. But the obstruction of the eustachian tube, the most frequent cause of this disease in young children, is generally removed without difficulty. At the Royal Dispensary for Diseases of the Ear, as well as in my private practice, I have had opportunities, which have not occurred to any other practitioner in the metropolis, of examining different applicants, for, perhaps it is not generally known, that the governors, in order to afford more prompt and extensive relief, have often, with the most benevolent views, ordered that all deaf and dumb patients should be admitted without recommendation; I have thus been enabled to try every mode of treatment, and none has appeared to me so efficacious, after having had the ears well syringed, (an operation rarely performed on deaf and dumb children,) as blisters and gentle emetics. The following I have generally found most successful:—

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[*] Vide Report of the London Deaf and Dumb Asylum.

℞ Antimonii Tartarizati, gr. ij.
Pulv. Ipecac. gr. xxiv.
Aquæ Distillat. ℥vj. M. fiat mistura.

In order to suit the medicine to the patient, a dessert spoonful is given every five minutes, until it operates.

To illustrate these observations, the following cases are given:—

Mary Haines, aged four years, was admitted as a patient at the Royal Dispensary. She has been deaf and dumb from her birth. She was the only one of her family afflicted with these defects, though her parents had five children besides herself. She was much emaciated, and in such a state of general debility, that I had considerable doubts whether she would survive. On examining the ears, I found a great deficiency in the ceruminous secretion. Her general health being so deranged, the first indication was to endeavour to invigorate the constitution, and this, by proper means, was happily accomplished. The usual remedies, such as blisters, injections, and other topical applications, were then employed, with the occasional use of purgatives. By the prompt and judicious adoption of these means, I am happy to state, she has acquired the inestimable faculties of speech and hearing.

Mary Anne Hague, on recovering from a severe fever which had endangered her life, became deaf and dumb. She was brought to me when she was seven years of age; her case seemed of the most hopeless kind, and nothing but the strong importunities of her mother could have induced me to make attempts for her relief, and persevere in the treatment. After a number of ineffectual essays, and various modifications of my plan of proceeding, I had the satisfaction to restore her to hearing and speech. This case strongly exemplifies the advantages of perseverance in the treatment of these complaints, and demonstrates the necessity of repeated and prolonged trials of the various remedies, even in the most discouraging circumstances, especially when it is considered, that the faculties which we seek to restore, are of primary importance, and will qualify the patients to become incalculably more useful to themselves and to society. In regard to this child, I may also remark, that her speech was very probably improved in consequence of her being at a common school, for we are well aware how forcibly the principle of imitation acts upon children, and what great efforts they will make to perform whatever they see done by others.

Selina Hewitt was admitted a patient at the Royal Dispensary for Diseases of the Ear, afflicted with congenital deafness and dumbness. It was with some hesitation, in regard to success, that I undertook her case; but after commencing a plan of treatment similar to that adopted in the former instances, I soon observed indications of gradual amendment, and she has made such progress as to be able to hear and speak, much to her own satisfaction and to the heartfelt gratification of her friends, who awaited the result with great solicitude.

Thomas Mayo, aged two years and a half, had become deaf and dumb from accidental causes. When he was about a year old, the servant who had charge of him, having taken him out for an airing, imprudently detained him in the park too long on a very cold day, the consequences of which exposure were febrile symptoms, accompanied with sore throat. On examination, I found the eustachian tube obstructed, probably from inflammation when labouring under the sore throat; there was also a discharge from the meatus. I employed the emetic mixture, which was repeated several times, at intervals of a week. This completely removed the obstruction; the discharge was cured by means of injections, and a slight alterative course of medicine. The child is now able both to hear and speak.

James Lawlor, a boy about five years old, was sent to the Dispensary, from Ireland. His mother, who accompanied him, informed me that he was born deaf and dumb. He was also blind of one eye. Finding, on examination, an obstruction in the eustachian tube, I ordered the emetic. When I saw him, three days afterwards, his mother informed me that he could distinguish loud sounds. The emetic was consequently repeated, with even greater apparent success than before; and as he continues under my care, I anticipate the most favourable results. I adduce this case as a proof that slight affections, if early attended to, may be treated by this curative process with the fairest prospect of ultimately effecting the total or partial removal of the malady. It is requisite to repeat the emetics frequently, in order to remove the collections of mucus which lodge in, and obstruct, the tubes. The period required for the success of such means, must vary in different cases, according to the degree of obstruction. No remedy is so suitable for the purpose of removing these obstructions as emetics; of this every well-informed practitioner must be convinced, when he considers the nature of their action. The operation of emetics is not, as is imagined by the vulgar, confined to the evacuating the contents of the stomach; these remedies produce great increase of the secretions of the mouth and throat, as well as of the juices of the stomach; by their action the biliary and pancreatic ducts are emulged, the secretions of the liver and pancreas are augmented, as well as those of the glands in the vicinity of the mouth and throat; stagnant fluids are put in motion; morbid congestions and accumulations in various parts of the system are removed; the excretion of mucus from the lungs is promoted; the general absorption of the system is increased; the circulation is rendered freer; the blood is diffused more equally over the system, and topical determinations are, in many instances, lessened or removed; the articular and pulmonary exhalations are very sensibly promoted; an action is also excited in the nervous system, which is often highly salutary in nervous disorders. The excitement produced rouses the sensibility of every part, without producing debility; and thus the different minute parts of the organ are enabled to recover their energy, and structural mischief that may be forming is prevented.

Soho-Square, October 20, 1831.

BOTANY OF NEW ZEALAND;

BEING A

Description of Trees, Plants, &c. indigenous to that country.

By George Bennett, Esq. M.R.C.S. &c.

[With Engravings.]

Edwardsia Mycrophylla, Salisbury.
Sophora tetraptera . . Forster.
Kowhy, or *Kongia*, of the natives of New Zealand.

Natural Family, *Leguminosæ*.

Class, *Decandria*. Order, *Monogynia*.

This tree is the Kowhy, or Kongia, of the natives, and attains the height of from forty to fifty feet, but it rarely attains more than five feet in circumference; it is of very irregular growth, and is seldom seen straight; it is found in good soil. The wood is hard and durable, and is used principally by the natives in the manufacture of their paddles, and instruments for digging, &c. The flowering season is in September, and the two or three subsequent months.

There are two species at New Zealand, both of which have been correctly figured in several botanical works.

CRYPTOGAMIA.

Asplenium lucidum . . . Forster.
Uru-uru fenua of the natives of New Zealand.

It is difficult to assign a reason why certain trees and plants are held sacred by Pagan worshippers, as the misletoe (Viscum album) among the Druids; the Tamanu (Calophyllum inophyllum) and Aito or Toa (Casuarina equisetifolia among the Tahitans; the Ficus religiosa, and others among the Hindus; and the Fern now under observation among the New Zealanders. To queries on this subject, an answer is usually received of "its being the custom," probably originating in some lost tradition, or else some superstitious legend is found attached to them.

This species of Asplenium is a sacred plant among the New Zealanders, and is called by them Uru-uru-fenua; it is used by the Tohunga, or Priest, when praying over a sick person; he waves (during the time he is endeavouring to avert the anger of the gods by his prayers) a frond of this fern; if it breaks it is a sinister omen and the sick person will not be expected to recover.

When the Tohunga consults the spirits previous to engaging in war, he also waves a frond of this fern in his hand during the time he offers his prayers; if it breaks it is considered that the spirits are adverse to their engaging in war, and the enterprize is consequently given up.

It is also used by the natives as a badge of mourning. When a wife mourns for her husband, she sits wailing in her hut, with a frond of this fern, bound as a fillet around her head: a husband does the same when he loses his wife. They are careful not to burn, or use this plant in cooking their food.

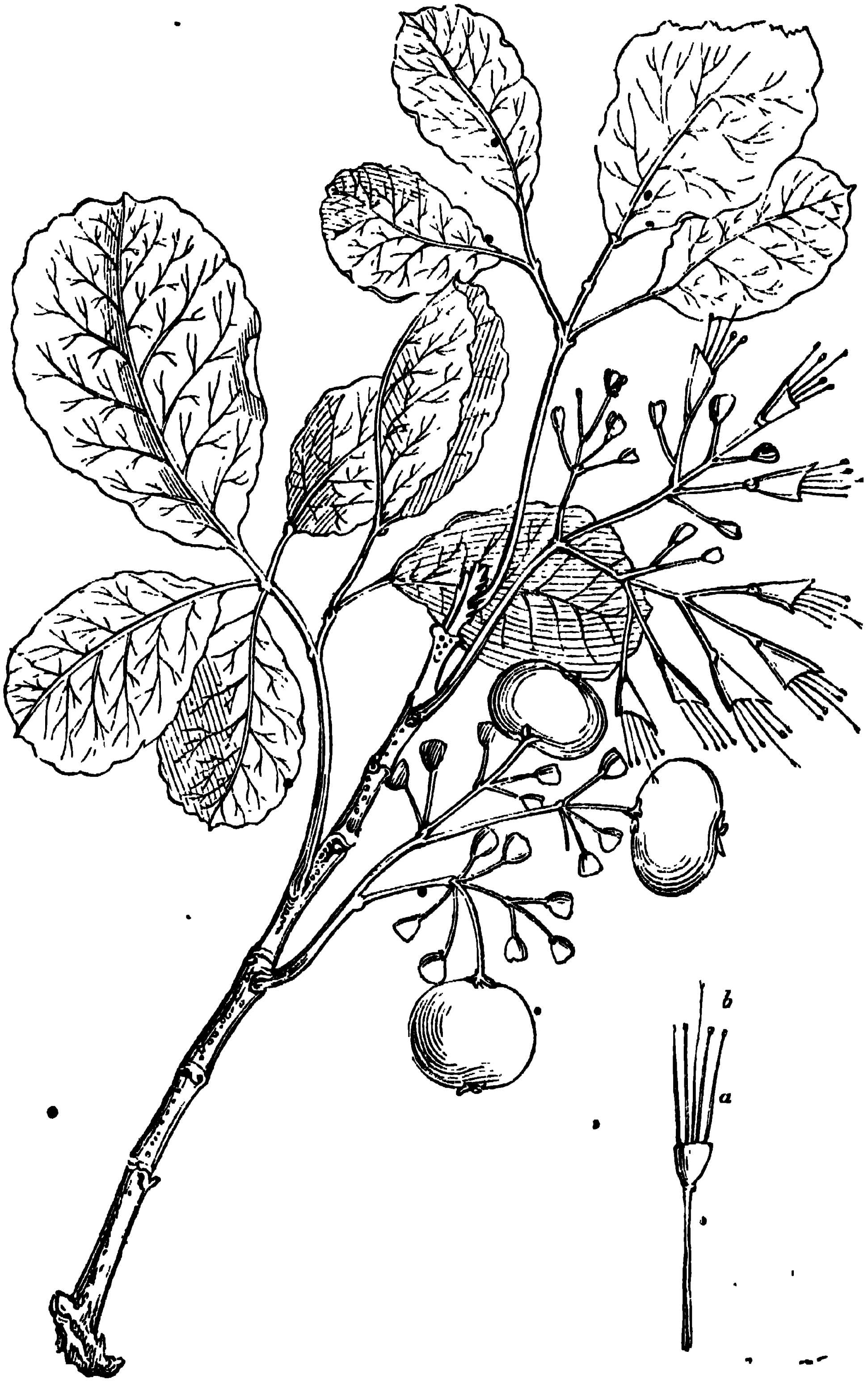
When a Chief has his hair cut, the Tohunga, or Priest, is present. After the operation is performed, the Chief holds a frond of this fern in his hand, during which the Priest prays over him, frequently taking the frond and shaking it; after which it is dipped into water, and shaken over the Chief; if it breaks it is regarded as a sign that the life of the Chief will be but of short duration; and if one of the leaflets only is broken off, it is expected that one of the family will soon die; if the frond remains entire, it is an indication of success and long life.

The fronds are usually two feet in length.

Ephialis Sp.

Puredi, or *Kauwere*, of the natives of New Zealand.

This is an unpublished species of Ephialis, and is figured in Parkinson's Botanical Drawings, made during Cook's first voyage, among the collection of Sir J. Banks now deposited in the British Museum. It is the Puredi, or Kauwéré, of the natives of New Zealand; seldom attains more than the height of forty feet, but in circumference is usually from ten to twelve feet. The wood is very hard, and is seldom used, as it injures the axes on attempting to cut it. The wood is of a yellowish colour, and yields a yellow colouring sap on being cut, and when washed. It is not used by the natives, however, as a dye, or for any other purpose. The flowers are elegant, drooping, and of a pink colour. The fruit is a drupe, the seed inclosed in which has on it numerous irregular projections; the fruit is of a bright red colour. The following drawing was made from a recent specimen at New Zealand. The fruit is delineated of the natural size. (*a*) The four stamina attached to the calyx; (*b*) the pistil.



Dacrydium cupressinum, BANKS and SOLANDER.

Remu, of the natives of NEW ZEALAND.

Natural Family, *Coniferæ.*

Class, *Diœcia.* Order, *Monadelphia.*

This species of Dacrydium is named Remu by the natives of New Zealand; it attains the height of eighty to ninety feet, but seldom exceeds fifteen feet in circumference. The timber is hard, of excellent quality either in plank or spar, and is considered the hardest timber of all the New Zealand pines. This species is not found so abundant as the other pines, growing only in particular districts. The wood in colour is an intermixture of white and red; no gum-resin exudes from it. The fruit is eaten by the natives, and was described by them as a small red berry, containing a black seed. This species of Dacrydium is the one mentioned by Captain Cook as the spruce fir of New Zealand.

It is figured in Parkinson's Drawings, made during Cook's First Voyage, in Sir Joseph Banks's collection, now deposited in the British Museum, from which the engraving in Cook's First Voyage seems to have been made.

Phyllocladus Species.

Tánakáá Táwai, or *Toatoa,* of the natives of NEW ZEALAND.

Natural Family, *Coniferæ.*

Class, *Diœcia.* Order, *Monadelphia.*

This tree, the Tánakáá, Táwai, or Toatoa, of the natives of New Zealand, is an unpublished species of Phyllocladus; it attains the height of sixty to seventy feet, but seldom exceeds fourteen or fifteen feet in circumference. The timber is heavy and hard, of good quality either in plank or spar, and of a white colour, but not equal in durability to the Kowri, and its weight renders it less valuable for spars. It sinks in water; a gum-resin exudes from it, but in very small quantity.

The bark of this tree is used by the natives of New Zealand for dyeing a red or black colour*; the mode of preparation is as follows. The bark is pounded, and then placed into a vessel of cold water, into which hot stones are placed until the water boils*. After the bark has been boiled for some hours, the liquid becomes of a dark red colour: it is then left to cool, the decoction strained, and is ready for use. The Flax (Muka) to be dyed is then placed into this liquid; and after remaining for some time, when taken out, is found of a red colour; it is then placed into the sun to dry. This red colour is not indelible, but will stand frequent washings before it comes out. When the flax is to be dyed a black colour, after having undergone the before-mentioned process, it is placed in mud, (usually from marshes, &c.) and remains in it for the space of twelve hours, after which it is taken out, dried, and is found of a shining black colour †. The bark of this tree is generally used in the recent state, but its virtues also remain when dried. The beautiful black colour of the flax used in the manufacture of some kinds of the New Zealand mats, is procured by the above-mentioned process. The bark of this tree is sometimes stripped off by the natives, the outer part removed, and the inner bark, which is of a dark red colour, is worn round the waist as an ornament, but is not of any durability.

Podocarpus Species.

Mai, or *Mátai,* of the Natives of New ZEALAND.

Natural Family, *Coniferæ.*

Class, *Diœcia.* Order, *Monadelphia.*

This tree, the Mai or Matai of the natives of New Zealand, is an unpublished species of Podocarpus. It attains the height of from eighty to ninety feet, and from ten to twelve feet in circumference; it is scarce in the vicinity of the sea, but is found abundant inland. The wood is of excellent quality, of a red colour, resembling cedar. One tree was cut down at Wyshaki Cove,

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* I believe that a black dye is wanted in this country, which, on exposure to sea air, will not lose its colour; if so it is be found in the black procured from the bark of this tree, as the New Zealand mats dyed with it do not fade even when washed in salt water, but it rather gives a greater brilliancy to the black.

* This is the native mode of heating water, as they have no vessels which can be placed on the fire.

† I know not, on analyzation, of what chemical properties (but probably salts of iron are held in solution) the marsh mud may be found composed, but it will be an interesting subject for further investigation. In Ireland it is customary among the poorer people to steep the undyed woollen cloths in the water of the turf-bogs, which dyes the cloth of a dark colour.

River Thames, from which I procured specimens; it measured thirty feet in height, and six feet in circumference. The torins, or New Zealand flutes, (which are tastefully carved,) are made from this wood; they are not hollowed out, but formed of two portions, which are accurately joined, tightly bound with cord made from the New Zealand flax, and the joined portion well luted.

London, Nov. 4th, 1831.

MEDICAL GAZETTE.

Saturday, November 12, 1831.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

MODE IN WHICH CHOLERA IS PROPAGATED.

THE only rational source of confidence and tranquillity under the existence of an approaching danger, is a thorough acquaintance with its nature, and a timely adoption of the measures requisite to avert its actual presence, or if this cannot be, at least to limit its extent, and controul its influence. On this ground it is that we think the public ought to be put in possession of all the most correct and useful information which can be obtained on the subject of Cholera. Now the two most important points to determine are, first, the manner in which the disease spreads, that we may know how to avoid it; and, secondly, the best mode of treating it, should our previous efforts at prevention fail.

The first of these—or the manner in which cholera propagates itself—is a consideration of vital importance to the interests of the community; and it is one with regard to which a large portion of the press has done, and is continuing to do, mischief which may prove irreparable: we allude to the efforts which are made to convince the public that the disease is not contagious. The motive of the writers we doubt not to be good, but they err lamentably in judgment, because the evidence against their views is of the most convincing description to an unprejudiced mind, and because the direct tendency of their doctrines is to lead those who adopt them into unnecessary danger. Let it not be supposed that we are among the "terrorists," or that we concur in the sentiments of the panic-stricken writer in the *Quarterly*, whose recommendations, if adopted, would sever every bond which links society—would cause the parent to abandon his child, and the child his parent—converting the land into a scene of distrust and desolation worse, infinitely worse, than has resulted from the disease itself even where its ravages have been the greatest. But, because others may have magnified the evil, is that any reason why we should shut our eyes, and refuse to look upon it as it really is? If small-pox were raging epidemically, should we not tell the public that the surest way to escape was to avoid the source of contagion?—precisely on the same principle, then, we urge the imperative necessity of their being made aware that cholera is propagated, and, consequently, may be avoided in a similar manner; and we pledge ourselves to prove in the sequel how small the individual risk is if proper precautions be taken, and how great the peril if they be neglected.

The first point, then, to determine is, whether it be contagious or not; and here we would premise that many of those who argue this point are wholly incompetent to give an opinion upon it at all*. But instead of interrupting our narrative by arguing with them here, we proceed to grapple with those who are of the medical profession, and who, better acquainted with such subjects, still prefer the fanciful and incongruous explanations, which their ingenuity enables them to offer, of the phenomena regarding cholera, rather than adopt the plain straight-forward path to which undeniable facts and common sense alike conspire to lead them.

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* See what we have said further on this subject at p. 214.

First, then, we hold that the great majority of intelligent persons, who have had opportunities of witnessing the disease, or of examining on a large scale the evidence afforded by others, are of opinion that it is propagated by contagion. This has not only been denied, but the very reverse has been asserted by the anti-contagionists to be the case; and as it is a question, not of reasoning, but of recorded facts, we shall proceed to prove that we are right in our assumption by quoting the opinions of the best authorities, as stated in various public documents.

The disease, it is known to all, whether it had previously existed or not, first attracted notice, and excited alarm, in 1817, when it broke out in the *Delta of the Ganges*, and thence gradually over-spread the whole of India. Now it has been said that the Anglo-Indian practitioners almost universally regarded the disease as non-contagious; and it is true that the majority, though by no means all of the regimental surgeons in India, came individually to this conclusion. It was deemed expedient, however, to appoint at each presidency a Board of the more experienced and intelligent medical officers, to examine and collate all the returns transmitted to head-quarters, and to sift the whole body of evidence upon the subject. The result was, that at Madras and Bombay the disease was unequivocally admitted to be contagious. At Calcutta, on the other hand, no report was ever drawn up by the Board collectively, but merely by one member of it, Mr. Jamieson, who gave his own individual opinion against contagion. While, however, he did this in one part of his paper, he expressed himself in another in such a manner as to shew, either that his opinion was not perfectly made up, or that he actually admitted the doctrine of infection as existing when emanating from a considerable number of persons. The following quotations will shew that we represent this matter correctly:—

Bombay Report. — " It appears, then, incontrovertible that this disease is capable of being transported from one place to another, as in cases of ordinary contagion or infection, and also to possess the power of propagating itself by the same means that acknowledged contagions do."

Madras Report.—After citing numerous illustrations of the infectious nature of the disease, and alluding to the objections of the anti-contagionists, it is added—" Diseases avowedly infectious, such as small-pox, measles, &c. have not at all times the power of spreading epidemically; for while it is certain that their exciting causes are never wholly extinct, it is only at particular periods that these diseases become epidemic. The same may be the case with cholera. . . . . The sudden appearance and disappearance of cholera, however, unlike the progress of known infectious diseases, is not admitted as being irreconcileable with the doctrine of infection, especially if the disease be of sudden invasion, after the application of the exciting cause."

Bengal Report.—" This much, however, may be affirmed, from a review of the whole progress of the epidemic in this quarter, that the infectious medium, in whatever it consisted, was limited within a very circumscribed circle, and was very slowly extended. If, setting aside the circumstances militating against it, we take it for granted that the infection was truly received by the centre and Hansi divisions, from the detachments above-mentioned, we must believe that the disorder, although not communicable by contact from person to person, was so from one large body to another large body; and that, when the poison got head amongst a number of men, it assumed some new quality, so as, when mixed with the atmosphere, to become infectious. What constituted this additional quality, we cannot pretend to tell."

When the disease at length made its appearance on the south-eastern frontier of Russia, medical commissioners were appointed at Orenburg, and other places, with whom its mode of propagation was a primary object of investigation, and the result was, that "the local boards, as well as the physicians, have been fully convinced that the cholera does, in fact, disseminate itself from one man to another, and by this means travels from place to place[*]." Still holding on its course, the cholera ere long reached Moscow, and here a considerable number of medical men adopted the anti-contagion side of the question; in consequence of which a special commission was appointed by the Emperor, and the evidence adduced before them led them to report, that they did not think the disease was transmissible *by goods*; but they said not a word at variance with the opinion previously given by the supreme Board at St. Petersburgh, that "it has resulted from observation that the contagiousness really exists," and that the contagiousness of cholera is "in some instances incontestible[†];" and we learn that "at a consultation of forty of the most respectable physicians of this city [St. Petersburgh], thirty-eight came to the conclusion, after mature deliberation on the documents laid before them, that the disease was infectious, and only two were of an opposite opinion[‡]."

To Moscow also repaired Dr. Albers, a Prussian physician, who stated to his government that the disease was contagious: the same view of the subject was taken by Dr. Walker, an English physician, resident at St Petersburgh, who had been sent to Moscow;—and MM. Moreau de Jonnés and Gravier transmitted to France a similar opinion.

When the disease manifested a tendency still to spread, other European governments, alarmed at its progress, employed commissioners to examine its phenomena, and to ascertain, if possible, its mode of propagation. Austria, Sweden, and Holland, sent intelligent persons to the spot where it was raging, and all with one voice have pronounced in favour of contagion. From England, Dr. Russel and Dr. Barry were dispatched; and if the opinion of the latter, who was previously known to be a contagionist with regard to other disputed diseases, may be received with some suspicion, no one can reasonably call in question his zeal in investigating, or his fidelity in recording facts. But even if his evidence were rejected altogether, no similar objection can be brought against that of Dr. Russel. Indeed his opinion, on the very principle which might invalidate the other, becomes of greater weight when we state that he had imbibed the doctrine of non-contagion in India, but that what he witnessed in Russia has removed his preconceived notions, and that he advocates this doctrine no longer[*]. At Warsaw it is said that the disease was regarded as not being contagious. By whom was it so regarded? By an English surgeon (Mr. Searle), who had already committed himself on the subject, having recorded his opinion in a work published some time before he went; and by a few Quixotic young physicians from Paris, who were so ignorant of the laws of contagion as to regard inoculating themselves with the blood, or tasting the evacuations of patients labouring under cholera, as an *experimentum crucis*; when it is notorious that no inference whatever can be drawn in favour of their views from these disgusting proceedings. Of these gentlemen we may remark that six out of about twice that number who went to the Polish capital, have perished*. Certain it is that both Diebitsch and Skrznecki, the commanders of the Russian and Polish armies, were satisfied that the pestilence was contagious: and the government at Warsaw, guided by the opinions of their most intelligent men, adopted the same view, as will appear by the following extract from one of their proclamations:—

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[*] Official documents published at St. Petersburg.

[†] Official Report, published at St. Petersburgh.

[‡] Private letter from Dr. Russel.

[*] At the very moment we are writing we have received wet from the press the second edition of Mr. Orton's work, decidedly the best which has yet appeared upon the subject of Cholera. Like Dr. Russel he had rejected the doctrine of contagion, but can no longer resist the evidence in its favour. The following passage is highly creditable to his candour:—"It was not without astonishment that many of the profession in India heard that the Medical Board of Bombay, in 1818, held the disease to be contagious. My feeble voice, in common with the great majority, was raised in opposition to the—as it appeared—monstrous dogma; but the march of time and events, the great accumulation of facts and gradual removal of prejudices, have wrought in my mind the same revolution they have in many others: the opinion of the contagious nature of the disease has been gradually gaining ground even in India, and seems to be the general one in Europe—*Magna est veritas et prevalebit.*"—*Orton on Cholera*, 2d edition, page 314.

> "Ever since the battle of Iguinie we have had to struggle against that contagion, which attacked us not only in the camp, but even in our capital . . . . . . We were already congratulating ourselves upon having deprived our enemies of one means of injuring us, and upon having rendered a fresh service to Europe, but on the 26th of May the battle of Ostrolenka brought the two armies into contact, and the cholera once more made its appearance, as well among our soldiers as among the inhabitants of Warsaw†."

So also with regard to Berlin, from whence we have the following account by Dr. Becker—himself, alas! subsequently a victim of the malady:—

> "It may finally be stated, that the manner in which the cholera has appeared and spread in Berlin, perfectly warrants the conclusion, that this disease is always produced by a peculiar human effluvium, which in its mode of generation and diffusion shews some analogy with that of the *common typhus fever of Great Britain*, but extends its deleterious effects to a limited number of persons only, who are rendered eminently susceptible by cold, intemperance, fatigue, fear and anxiety, diarrhœa, and other previous diseases‡."

And lastly, with regard to this part of the question, hear what an intelligent writer says, from a totally different quarter:—

> "*Valeni del Mount.*—Having observed in the public prints an opinion put forth by some physicians that the cholera morbus, even when epidemic, is not contagious, I think it right to state, that, in this country at least, it is undoubtedly communicated in the same manner as plague or scarlet fever—by contact or approximation. Thus the thirteen cases of that malady that have hitherto occurred in this place, have been all of them distinctly traced to the above source*."

But it will probably be asked, are there no places in which the local Boards regard the disease as non-contagious? There are. Opinions to this effect have been expressed at Riga and Dantzig; but we are inclined to place little reliance upon them; first, because no sufficient grounds of this exception to the more general sentiment are stated; and, secondly, because the extent to which both these places are dependent on their trade, renders some degree of distrust almost unavoidable as to the impartiality of their decision; just in the same manner as we have seen the existence of cholera at Sunderland denied, in letters from that place, published in the daily papers. Another non-contagionist presents himself at Dantzig, in the person of Dr. Hammet, who was sent there by the English Government. With regard to the communications of this gentleman, we can only say, that we think the Board of Health would have acted more judiciously if they had published them. We presume that they have been prevented from doing so by their extreme prolixity; still the illustration which they afford of the loose manner of reasoning adopted by the non-contagionists, would indirectly have strengthened the opposite side of the question, while their suppression may lead those who have not seen them to imagine that they have been withheld on account of evidence against contagion — which, in fact, they do *not* contain.

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* Viz. MM. Hugon, Collé, Michaud, Barrie, [illegible]rezes de Saurtours, and Le Gallois. We are unable to say whether they all died of the cholera or not.

† Circular of the Polish Government respecting the cholera morbus.

‡ Private Letter from Dr. Becker.

* Official letter from Mr. Blutte, the English consul, to Lord Cowley.

We have thus shewn, then, the utter groundlessness of the common assertion, that they alone think cholera contagious who have not seen it, and that, on the contrary, the immense majority—not of illiterate persons, but of those capable of forming a judgment on the subject—have espoused the side we advocate. Nor is it a trifling addition to the probability of this view being correct, that it has been unanimously adopted by the Board appointed in London, numbering as it does among its members some of the most eminent of our civil practitioners, as well as those at the head of the medical departments of the army and navy.

But in coming to the conclusion at which we have arrived, that cholera is a contagious disease, we have not been influenced by these authorities farther than as an inducement to examine with attention, at the same time with candour, the evidence which had led so many to an inference, which, we are free to confess, was at variance with our views with regard to certain other diseases, which by many are regarded as contagious; and the following are the circumstances which have decided our opinion :—

There are three modes in which disease may pervade a district—it may be endemic, epidemic, or contagious. Now if we apply the characters of the two former to cholera, they will not be found sufficient to explain its phenomena; or rather, they will appear manifestly to be inconsistent with them.

The extent to which cholera has spread, and the diversity of the localities which it has occupied, at once set at rest the idea of its being what is usually understood by endemic; and there remains only that we should choose between the two latter. Diseases which are epidemic, without being contagious, break out in distant places either simultaneously, or travel with a rapidity, and in a manner, that defies all calculation; cholera travels slowly and progressively. Epidemics march on the winds, and cannot be arrested; the progress of cholera has repeatedly been stopped. Epidemics generally prevail in tracts of country analogous as regards humidity, temperature, soil, elevation, or some other obvious similitude; cholera has exerted its dominion alike in the marshy jungles of Hindostan and the arid plains of Persia—the burning sands of Arabia, and the snow-bound provinces of Moscow. Epidemics, unaided by contagion, prevail for a time and disappear; cholera, like small-pox, scarlatina, and other undeniably contagious maladies, has never wholly left any country it once has visited ; it still lingers in Bengal, where it commenced, and, in fact, wherever it has laid its envenomed hand, though it may occasionally relax its grasp, it still retains its hold. These considerations render it, *prima facie*, improbable that cholera should be merely epidemic. Let us now see what evidence of a direct kind there is of its being contagious.

Before doing this, however, we must request our readers to keep in mind, that the evidence of any disease being contagious is derived from a consideration of many facts, with regard to the circumstances attending its extension from person to person, or from place to place; and that, though perfectly demonstrative to men of ordinary reasoning powers, and whose minds are unprejudiced, it never is of that overwhelming and intuitive nature which forces conviction by a direct impression upon the senses. So much otherwise, that Sydenham, who witnessed the ravages of small-pox on a great scale, seems to have entertained no suspicion that it was contagious; yet no reasonable man of the present day doubts that it is so. But though this be the mode of its propagation, and though it be thus *communicable*, it does not follow that it will in every instance be thus *communicated*. It is a remarkable fact, with regard to all the animal poisons, that some peculiar state of the individual is required in order to bring him under the influence of the disease: thus it is recorded by Dr. I. Hunter, that 21 persons were bitten by a rabid dog, and only one became hydrophobic. Does any one suppose that, because eleven escaped the effects of the poison, therefore the twelfth, who took the disease, did so from some other cause, and not from the bite? Yet this is precisely the argument of the non-contagionist with regard to cholera: the numberless cases in which the disease has directly followed exposure to some one having it are set aside, and some other explanation is offered as to the origin of the disease, because, in certain other cases, persons have been exposed to the same cause with impunity. How, then, is it with regard to complaints which universal opinion has now acknowledged to be *transmissible*—are they always necessarily *transmitted?* Take measles: probably there are none of our readers, who have had but a moderate share of experience, who have not met with this kind of case: measles breaks out in a family—it affects A, B, and C, successively, and, proving mild, the parents are anxious that all the children should have it; and, accordingly, a fourth child, D, is exposed to it by being allowed to eat, play, and even to sleep with the others; yet D refuses to take it, and passes the ordeal unaffected. Does any one thence suppose that measles is not infectious? On the contrary, we merely say that D was not at that time susceptible of the disease, and we know that, on another occasion, this same child may become affected with measles, from a degree of exposure not nearly so great as that which had previously been resisted. So also, with regard to small-pox, an immense proportion of mankind are incapable, from the protecting influence of vaccination, of having this disease communicated to them; but does any one thence doubt as to the mode of its transmission to those who do receive it? Our object is to impress upon the minds of those interested in this question, that the fact of a disease being transmissible by infection, and so communicated to those who have it, by no means implies that it must of necessity be transmitted to all who are exposed to it. There is no instance of any disease, however infectious, in which this is the case; and therefore the argument against the transmissibility of cholera from man to man, founded on the notorious fact that many men are constantly exposed to it with impunity, is a complete *non sequitur*—absolutely illogical; and which, if admitted, would prove, by parity of reasoning, that neither small-pox, nor scarlatina, nor measles, nor any other disease, was either infectious or contagious. The kind of evidence, therefore, by which any disease can be shewn to be infectious, is not that every, or nearly every one exposed to it becomes affected, but that, on the one hand, its appearance in individuals, or in places, so frequently follows approximation to those who have it—or on the other hand, the exemption from its attacks, while it rages around, is so frequently preceded by a careful avoidance of the presence of those affected, as to render it impossible for us to give any rational explanation of the phenomena, without looking upon exposure to the presence and the subsequent occurrence of the disease, or the exclusion of its presence and the continued exemption from the disease, as standing in the relation of cause and effect.

Now, keeping in mind that the infectious nature of a disease is to be deduced chiefly from the multiplication of probabilities, rather than from proofs approaching to mathematical demonstration, the first point to which we would direct attention is the mode in which cholera travels. We have already said, that epidemics overrun whole countries, either travelling with the wind, or at least with equal rapidity; but cholera journeys more leisurely. It proceeds from place to place successively; and, being at any given spot, it never jumps over any great intervening space to another similar locality, but, be the adjacent district moist or dry, high or low, healthy or unhealthy, if only there be men in it, and these have any intercourse with their fellows where it then is, the disease is sure to visit them. On the great scale it moves along the principal roads and navigable rivers, first affecting the towns and villages by the wayside, and subsequently extending to those more remotely situated. "Change of air and climate has apparently no influence on the progress of cholera*." "When once established in a marching regiment, it continues its course in spite of change of position, food, and other circumstances†." In short, it travels as man travels, stops where he stops, and proceeds again at the time, and in the direction, in which he resumes his journey. Moving along with the caravans of the east, it was approaching the city of Teheran, in Persia, when the Shah resolved to shut his gates against it. The travellers were obliged to make a circuit, in order to avoid the place; and thus did this sagacious Persian protect his domain from the visitation of this scourge for no less a period than eight years, though it raged on all sides around him; an example which shews what might have been accomplished had any precautions been taken in India to arrest its progress; but unfortunately none whatever were adopted. The instance of Teheran is, we believe, the first in which the quarantine was fairly tried;—it was commenced in 1821, and it was not till 1829, owing to some remissness, probably engendered by this long exemption, that the disease at length gained admission. The Russians, too, arrested it on its first irruption at Astrachan, but allowed it to spread on its reappearance in 1830; and, indeed, after this no effectual attempt seems to have been made to stop it on the eastern border of Russia. Not only were travellers allowed to proceed, but "they who could flee the city fled, and as the malady was not considered contagious, servants, labourers, Tartars and Russians, were permitted to rush into the country*." From this time it branched off in various directions, according to the routes of the different fugitives; and there are some circumstances connected with its course which strikingly illustrate its disregard of every thing except the path followed by men. From Astrachan it proceeded up the Volga, but without touching the Don, till a cossack, who had been sent from the station named Katchalinskara on this river, to buy provisions at Doubooka on the Volga, died of the cholera on the 7th of August, after his return home. From that time the malady spread successively through the different Cossack villages along the Don†, and arrived at Azof in October. We request our readers to attend to this circumstance. The cholera travelled up the Volga a certain distance—was carried across to the Don, where it was till then unknown; and while on the former river it continued its course upwards, running nearly north-west; on the latter it held its way down (having first joined this river near its source), running in a direction nearly south-east. Thus during the month of September 1830, cholera was marching on two nearly parallel lines, but in opposite directions—on the one from south to north, and on the other from north to south—an occurrence easy of explanation, if we admit that it was carried by the persons travelling in these directions, but totally inexplicable on any other principle.

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* Professor Lichenstein.
† Madras Report.

* Letter from a clergyman at Saratov. Quarterly Review, November.
† Crichton, &c.

Again, in every instance where cholera invades a new country, it does so on a frontier next some district already infected; or, if it attack an island, it does so at some sea-port. Thus in Ceylon it first broke out at Jaffnapatam and Columbo, in Sumatra at Acheen, in Java at Batavia, in the Isle of France at Port Louis, in Bourbon at Port St. Dennis, *in Britain at Sunderland*; and, in fact, the rule is universal; nor can any rational explanation be offered of the circumstance except that of the disease being imported. Persons on shore, in the neighbourhood of vessels having the disease, never take it unless some intercourse has taken place with the crew; and the converse holds equally true, that vessels arriving off an infected place remain free till they have had some communication with the shore. The illustrations of both these facts are too numerous to admit of any doubt. By sea then, or by land, we still find cholera accompanying man; and is not this a strong presumptive proof that it is carried by man?

But are there no exemptions from its so general influence—have no favoured places escaped its visitation? Yes, there are such exemptions—there are such places; and their history proves that there is one method, and only one, by which it may be turned from our doors. Exclude men, and you exclude this pestilence. To one example of this (that of Teheran) we have already alluded; but the point is too important to be left without farther proof. Let any unprejudiced man read those which follow, and we think he will see in them powerful evidence in favour of our argument:—

"Many gardens and farms in the neighbourhood of Astrachan remained exempt from the epidemic, having broken off all intercourse with the diseased districts. In many villages, too, where similar measures of security were taken, the issue was equally fortunate, although the cholera raged all around them; for example, in the lordships of Smirnov, Beketov, and Prince Dolgoruki, in Sarepta, eight miles from Zaritzin, and some other places*."

"The Moravian colony on the right bank of the Wolga, and several German colonies in the government of Saratov, around which the disease was violent, adopted the system of exclusion, and were also unhurt. At Caramala-Gubeewa some Russian peasants, living together, scarcely a hundred yards from the village, shut up their hamlet on the first report of the disease having appeared in their vicinity, and by enforcing a strict quarantine during the prevalence of the epidemic, remained in health†."

"Mr. de Lesseps, the Consul of France at Aleppo, when the cholera approached that city in 1822, retired, in company with all who wished to be of his party, to a garden at some distance from the city. His asylum was inclosed with walls, and was surrounded by a large fossé; there were only two doors, one for entrance, the other for going out. As long as the malady lasted, he admitted nothing from out of doors without submitting it to the precautions observed in lazarettoes. His colony comprised *two hundred* persons, and consisted not only of Franks more or less acclimatised, but also of several natives. *Not a single individual contracted the disease;* while, at the very same time, within the city, four thousand beings perished in the space of eighteen days‡."

"The cordons around Zarcozelo and Peterhoff were removed last week. We immediately visited these places, and saw, for the first time, Sir William Crichton and Sir James Leighton. Both these gentlemen separately and positively asserted, repeated the assertion, and permitted us to note it, that no case had occurred within the sacred precincts of either cordon since their establishment, though the circle of demarcation was completely surrounded with the disease, and though the enclosure around Zarcozelo contained from 8000 to 10,000 souls[*]."

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* Hawkins.
† Ib.
‡ Ib.

"Ever since the epidemic, the thoroughfare to Moscow has been through these little villages, which are separated by about half a verst, and contain about 200 souls. Yet, though at the village of Yshora, two versts further on, there have been several cases and deaths, and though there were so many at Colpina, there has not been a single case amongst the colonists up to this date [Aug. 8.] .... The authorities say, that no precautions of any kind were taken; *but it is remarked by Dr. Bowmann, that travellers to and from Moscow never halt at the colony;* Yshora on one side, and Colpina on the other, being so much better resting places[†]."

"The large establishment composing the Academy of Military Cadets, at Moscow, was preserved by a similar plan from the scourge which was so active on all sides of it[‡]."

"There were 150 pupils on the officers' side (Cadets at Cronstadt), which is kept perfectly distinct from the school, for petty officers and sailors. The gates were shut on the 19th June, and as strict a quarantine as possible maintained to the 6th August (O.S.) No case amongst the pupils, who are from nine to twenty years of age[§]."

But if the effects of rigidly avoiding those labouring under the disease be thus remarkable, the manner in which it spreads when once introduced is not less striking. This can only be shewn by examples; and first, as to places.

"The cholera was brought to Astracan by ships, and it has spread itself over Russia from Astracan by the emigration of the inhabitants, principally those of the lower orders. This is the chief cause of its propagation in Russia; it has never shewn itself in any place except where it has been brought by travellers, who came from infected places. *We have not a single instance* of a town, or of a village, which, without communication with houses or persons affected, has contracted the disorder. Several places surrounded by the disease have preserved themselves from it by a rigid insulation[*]."

"From every thing we have been able to learn as to the progress of cholera morbus in the north of Europe—from its first appearance in the towns and villages of this country having been generally, if not always, preceded by the arrival of persons, or vessels, or both, from infected places—from the manner in which the disease has now broken out in this city [St. Petersburgh], we see no other mode of accounting for its sudden appearance here than by concluding, that the barks from places on the Wolga, where the disease prevails, have brought something with them, which, disseminated in this atmosphere, has been the immediate cause of the eruption of cholera, which has just occurred[†]."

"The first cases of cholera in Berlin occurred among the skippers on the boats lying *on the river Spree*, which flows through the town, and in *houses in the immediate neighbourhood of the river*. The disease has prevailed to a considerable extent in all those streets which lie along the navigated branch of the river, and whose inhabitants at the same time live in frequent intercourse with the skippers and fishermen. On the fourth and fifth days cases appeared in other parts of the city, and, in many instances, they were those of individuals who were known to have had intercourse with cholera patients, or at least with the boats lying on the river, and with the streets first infected[‡]."

Nor is the evidence at all less convincing if we direct our attention to individual examples of the mode of propagation. In the above illustrations, and in those which follow, we have preferred selecting instances which occurred in Europe, because, what is nearest us, both as to time and place, naturally makes the greatest impression on our minds.

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[*] Drs. Russel and Barry.
[†] Ib.
[‡] Crighton.
[§] Drs. Russel and Barry.

[*] Extract of a letter from Dr. Rehman, Physician at St. Petersburgh, communicated to the Academy of Medicine, at Paris, by Dr. Marc.
[†] Drs. Russel and Barry.
[‡] Private letter from Dr. Becker, of Berlin.

" *City Prison.*—From the moment that the disease was proclaimed, the strictest precautions were adopted; no person was admitted without medical examination; rooms were set apart for a cholera hospital, and persons of both sexes appointed to attend the cholera cases, should any occur. Dr. Bish. who resides within the walls of the jail, and who, it must be observed, *was* an anti-contagionist, as acknowledged by himself, showed us a plan of the prison, illustrating the introduction of the disease amongst the prisoners, led us round the whole building (July 30), and communicated to us the following information from his journal, which had been most accurately kept:—' A woman had been sent out some weeks before to be treated for a syphilitic complaint[*], in a public hospital. Her husband was also in confinement at the time, in a different part of the building, but remained. The woman was returned to jail, on the 23d June, O. S., with a diarrhœa upon her. She saw and embraced her husband for a moment, as she passed on to be placed in the room of observation. In a few hours she was seized with true cholera, and died that night. This was the very first case. The next persons attacked in the prison were three women in the same room with the former, one of whom had rubbed the deceased. These three died all within three days after the first. The next prisoner attacked was the husband of No. 1; he lived in a separate part of the jail. After this man, others in his room, all numbered on the plan, and registered in Dr. Bish's journal. In short, of twenty-seven attacked (fifteen dead), there is but one to whom communication cannot be traced. He was confined for a capital offence, and had less liberty than the others. There were about four hundred prisoners and attendants: the former are all well kept, and treated with great indulgence. *None of the noble class, who are lodged in a separate part of the building, were attacked*[*].'"

Pensa.—" ' In a village of the government of Pensa, where this medical officer was sent in consequence of the breaking out of the cholera, to trace its origin and to afford medical aid, he learnt the following circumstances, which are attested by all the village authorities, and of which we are promised an authenticated copy, signed by himself:—The son of a villager, who was coachman to a nobleman, at fifty versts distance, died of cholera; the father went to the place to collect the effects of the son, and brought home with him his clothes, which he put on and wore a day or two after his arrival at his native village. He was shortly thereafter seized with cholera, and died of it: three women, who had watched him in sickness, and washed his body after death, were also seized and died of the disease: the doctor arrived in time to see the fourth case, and, finding that it spread on that side of the village, he had the common street barricaded on the side where the disease had not reached, and interdicted all communication to the two sides of the village, even for the purpose of going to church. In that side in which the disease first broke out, upwards of 100 cases of cholera occurred,—of whom 45 died, but the disease did not appear on the other side of the barricade[†].' "

" ' The Navarino corvette, Captain Nachinoff, 200 men. She had been placed two miles to the eastward of Cronstadt, during the epidemic, to question and examine all craft from St. Petersburgh. She had eleven severe cases of cholera, of whom eight died. Her first and second cases occurred on the 26th June, O.S. These two men belonged to the boat that examined the vessels coming from St. Petersburgh, on board many of which they had been. The next men who fell ill were of those who carried the two first cases to the hospital in town[‡].

" *Colpina.*—The first patient was a

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[*] " It is a rule, that this disease shall not be treated in the prison hospital."

[*] Drs. Russel and Barry.

[†] Private letter from Dr. Russel.

[‡] Dr. Russel and Barry.

non-commissioned officer of a ship's crew, who had arrived from St. Petersburgh a few days before. No. 2 was an invalided workman; this man was noted for his bad conduct, and had been ill for at least a day before he was brought to the hospital. No. 1 had been in company with him, and had been drunk the day previous to his being taken ill. The woman, No. 3, had nursed No. 2 while at home; so that there appears a strong case of communication of the disease, which cannot be traced further[*].”

“A physician (Dr. Calow), who had attended cholera patients, being at the time in bad health, and labouring under diarrhœa, died of cholera; on the day following, his landlord (Mr. Steibelt), died; a day afterwards, two children of the landlord died, and the servant-maid was taken ill of cholera, and recovered. The landlord's wife had been removed to quarantine. There have been no more cases of cholera in this street or its neighbourhood[†].”

Instances of this kind might be quoted without number; but if what we have given do not convince our readers that the disease is transmitted from one person to another, probably no multiplication of the same kind of evidence would avail.

There is, however, one point connected with this part of the question which it may be necessary to set right before we proceed farther. It has been said, that if cholera were contagious, a larger proportion of medical men than of others ought to take it. Hear what happened at St. Petersburgh:—

“Fifteen hospital physicians had been attacked, and six had died of cholera up to the morning of the 13th [July] instant, N. S., out of 264 medical practitioners of all descriptions, who were in St. Petersburgh at the breaking out of the present epidemic[‡].”

Was it by chance, we ask, that the first fifteen medical men affected with cholera were hospital physicians[§]? But again—

“Though we have not yet obtained official returns of the number, we are satisfied, from the statements we have personally received in the numerous hospitals we have visited, that the proportionate number of attendants, of all descriptions, on the sick, who have been taken ill with cholera, is fully greater than that of the medical men[*].”

Besides—

“There were 70 attendants of all classes employed about the cholera hospital of the Foundling Hospital, of whom 15 were attacked; two prescribing physicians, one attacked. Dr. Duppé [the resident physician] was seized on the 22d [June, O.S.] He had seen no sick before the first case of the *garde malade* already mentioned[†].”

Such are the chief considerations which, after the most scrupulous investigation, and in opposition to preconceived opinions, have convinced us that cholera is contagious. Several very important questions connected with the circumstances which facilitate or retard its propagation remain untouched; but we have already trespassed too long on the patience of our readers, and must leave further observations for another opportunity. We shall therefore add but one sentence more. The first step towards arresting a disease consists in detecting its mode of spreading—if Cholera does so by contagion, as we hold to be the case, the means of checking it are obvious; if it depend on any thing in the air around, or in the earth beneath us, then no human means will avail aught in arresting it;—therefore our view, as it is the more rational, is also the more safe and consolatory; and let it be kept in mind that, even if we be wrong, still no danger is incurred to the public safety by adopting our opinions; but if the anti-contagionists be wrong, and their principles followed, the peril is imminent, the evil incalculable, and the extension and permanent residence of the malady in this country, its naturalization among us, will be the inevitable result; just as the same ignorance, or disregard, of the mode in which the disease is transmitted, led to its overrunning the whole of India, in which country, at the end of fourteen years, it still continues to abide.

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[*] Private letter from General Wilson.

[†] Private letter from Dr. Becker.

[‡] Drs. Russel and Barry.

[§] Ten other medical men became affected in a few days after.

[*] Drs. Russel and Barry.

[†] She was taken ill on the 19th.

[See Postscript, page 214.]

COMPARATIVE MORTALITY OF CHOLERA.

The mortality of cholera has been very different in different places. According to an account given in the Russian State Gazette, the number cut off by the disease out of every 1000 inhabitants, during the first 48 days of its visit, has been:—at Lemberg, 51; Mittau, 34; Riga, 31; Posen, 16½; Petersburgh, 12½; Konigsburgh, 11½; Elbing, 9½; Dantzic, 8½; Stettin, 5½; Berlin, 4.

CHOLERA AT BERLIN.

If it be recollected that the capital of Prussia contains 225,000 inhabitants, besides a strong garrison, the ravages of cholera in that city will not seem to be so very frightful. From the 31st of August to the 10th of October, the daily number of persons attacked was from thirty-five to forty; and the whole number during that period amounted to 1100, of whom 233 were cured, and 665 died—the rest still remaining under cure. Of the garrison, no more than ten soldiers were attacked, but eight of them died. The physicians and hospital attendants have been generally safe.—*French Paper.*

PREMATURE INTERMENT IN CHOLERA.

The bodies of persons; apparently dead of cholera, have been in some instances observed to move. M. Londe, President of the late Warsaw Commission, has expressed his belief that many have been buried alive in the complaint.

SINGULARITIES.

Among the correspondence read at a late sitting of the Academy of Sciences, in Paris, were the following singular items:—1. A cure for the cholera, by a M. Dutertre, who offers to lose his head at the Place de Grêve if his remedy be not found superior to all that has ever yet been tried; 2dly, a letter from M. Garnier, announcing his discovery of the quadrature of the circle; 3dly, a letter, accompanied by a pretended *head of a flying dragon*, which M. Cuvier at once recognized to be the *head of a pig*; and, 4thly, a copy of verses on a new remedy for cholera!

ANALYSES & NOTICES OF BOOKS.

> " L'Auteur se tue à allonger ce que le lecteur se tue a abréger."—D'Alembert.

Reports of Medical Cases. By Dr. Bright. Price 9*l.* 9*s.*

[Continued from page 158.]

PRESSURE OF THE BRAIN *resumed.*

The next case is remarkable for the unusual rapidity with which it proved fatal. Mr. N——, aged 57, of spare habit and anxious disposition, enjoyed good health up to July 1st, 1830. He had been much fatigued, and went to bed at half-past ten, having taken a moderate supper. About half-past one he awoke sick, but seemed to be relieved by vomiting. His wife, however, was alarmed by his mode of breathing, and rang for assistance, when he was found in a deep apoplectic seizure; and before medical assistance could be procured he was dead—his decease occurring within twenty minutes from the commencement of his attack. The lateral ventricles were found distended with a large quantity of semifluid blood, which raised up the velum interpositum from the thalami. From this membrane the hæmorrhage seemed to have taken place, as it was rent at one part. The corpora striata seemed to be broken down in their substance to the extent of a fifth of an inch, and beyond this depth spots of ecchymosis were perceptible. The commissura mollis was torn, and the blood had descended into the third and fourth ventricles: the entire quantity might be above three ounces. The action of the heart had always been very forcible, but that organ appeared healthy in its texture, with the exception of a slight deposit in the aortic and mitral valves, but not to such an extent as apparently to interfere with their function. The coronary arteries were much enlarged.

In the preceding case the death took place with unusual rapidity for apoplexy, though still it was not so instantaneous as in rupture of the heart, or great vessels.

In the case which follows, death took place in an hour and a quarter from effusion of blood near the corpus striatum, tearing the texture of the brain and bursting through the arachnoid. The hæmorrhage appeared to have been copious and unrestrained, owing to the softened condition of the brain at the point where it occurred. Numerous other cases are given of apoplexy dependent upon effusion of blood, but in none did the fatal termination take place so soon, and in some it did not occur for several weeks. These cases were, in general, instances attended with some disease of the vessels. In two instances, where the patients had suffered repeated attacks of hemiplegia, affecting articulation and deglutition, and proving fatal at the end of several months, disease was found at the posterior part of the corpora striata. In another there was hemiplegia of the left side, and the cerebral lesion was not on the opposite but on the same side. As the case is short, we shall insert it.

Hemiplegia on the left side, with Cerebral injury on the same side.

"John Walker, aged 38, was admitted into Guy's Hospital October 14th, 1829, labouring under a severe attack of bronchitis; his countenance purple, his breathing very difficult, and his legs œdematous. He had considerable hesitation in his speech, owing to a paralytic affection of the mouth and tongue; and his left hand and arm were weak and obviously paralytic, though not to such an extent as to be useless. On inquiring from his relatives, we learnt that the whole left side was paralysed about a year ago, that the affection was sudden and for some time severe, but that he gradually recovered the use of his limbs to the present time. He survived only three days after his admission, and his death was quite sudden, so that he had spoken to the nurse within a few minutes of his dissolution.

"*Sectio Cadaveris.*—The dura mater was perfectly natural. The arachnoid and pia mater shewed a good deal of vascularity, but of purple blood, arising from congestion; very slight effusion beneath these membranes, which were stript easily from the convolutions, except on the lateral and outer part of the left hemisphere, where it was found impossible to detach them without lacerating the brain, to which they were glued by a thin opake yellow flake of deposit, like fibrin: immediately below this adhesion, and extending to the middle of the corpus striatum, was an opake portion of the brain considerably harder than the brain itself, plainly shewing in its centre that it was the scar of an apoplectic cyst, surrounded by hardened parietes. The cavity in this was scarcely so large as half a small French bean, containing a little softened curd-like substance; but the derangement from the injury evidently extended from the middle of the corpus striatum, the structure of which was much disturbed, quite to the surface of the middle lobe.

"The whole of the remaining parts of the brain, and more particularly those of the right side, were most carefully examined, but without finding the slightest trace of disease. The medulla oblongata was so cut into, that we did not ascertain whether there were any peculiarity in the decussation of the fibres."

There is every reason to believe, with Dr. Bright, that the diseased portion of brain above described was constituted by the remains of the apoplectic cyst, which had undergone a kind of cicatrization. A case in which the lesion and paralysis were on the same side, is recorded by Morgagni, Lib. I. Epist. 2. § 16.

The author next introduces some cases, illustrative of the effects of pressure acting suddenly, yet not proving immediately fatal. It is upon cases of this sort, which fortunately are not infrequent, that all our hopes must be founded of being able to prolong life, and often to secure even a pleasurable existence after the occurrence of severe attacks and of very unfavourable symptoms. Sooner or later, however, if their subsequent history be traced, it will be found that the disease has recurred in most instances, and ultimately proved fatal.

A woman, aged 33, had experienced, during some weeks, headache, vertigo, and occasionally dimness of sight, for which she was bled, and blistered on the neck, with very little relief. Upon awaking one morning, she found she could not move her left leg and arm as well as usual, and, in short, she appeared to be afflicted with all the symptoms of partial hemiplegia. She suffered much from pains in the side affected, particularly in the joints of the leg and arm. Pulse 108; tongue covered with white fur, moist, and slightly drawn to the left side. She attributed her illness to exposure to cold and wet. She got better in the hospital, but her partial paralysis was by no means cured when she was discharged. Those pains in the affected limbs, sometimes (and with correctness) ascribed to rheumatism, were also observed in two other cases; they appear, however, to be rather connected with the affection of the nerves on which the paralysis depends.

A man was taken to Guy's Hospital, who, while rowing the day before, was suddenly seized with insensibility, and, on recovering himself, found that he had lost the power of his right side, and was unable to articulate. He had been labouring under severe cough and dyspnœa for six weeks, and there can be little doubt but that this pulmonary disorder laid the foundation for the effusion of blood in the brain, which was immediately induced by the exertion he was making at the moment. By cupping in the neck, keeping the bowels open, and by the use of a combination of squill, grey oxide of mercury, and a little opium, he was enabled to go home in a few weeks much relieved.

Another instance of paralysis from cerebral congestion, arising from obstructed circulation in the lungs, but augmented by habitual constipation, is given by Dr. Bright, in the case of a woman who had been long accustomed to a most severe asthmatic affection, which used very often to prevent her lying down at night. She was also subject to occasional pain in the head, giddiness, and sickness. While suckling a child, some six weeks before she came to the hospital, she suddenly heard of an accident having happened to her son, upon which she fell into a sort of hysteric fit. This was followed by a loss of power and sensibility in the left thigh, and the arm of the same side was similarly affected. A few days after, the right leg became numb and powerless, so that she could no longer support her weight. By the use of the aloes and myrrh pill, and attention to her bowels, which were habitually confined, she was so much improved that, in about ten days, she could rise from her bed and walk some distance; and she went on improving to such a degree that, though not completely restored, she was allowed to leave the hospital, at her own request.

We shall just give an abstract of one case more before we proceed to the author's general inferences A thin, middle-sized man, 46 years of age, whose occupation was to work the steam-engine of Barclay's brewery, was admitted into the hospital, affected with hemiplegia. For some time previous he had suffered from occasional headache and vertigo; but it was not above two hours before his admission, that, as he was walking, he felt his right foot become very heavy, and then his right hand; his articulation also became very indistinct. Sensibility was very obscure on the whole of the affected side; indeed it might be traced accurately to the middle line of the body and head, including the ear and cheek. The mouth was drawn to the left side; the speech indistinct; the pupils acted naturally when light was brought to them; his intellects quite undisturbed; bowels costive; head more free from pain than for a long time previous to the attack. He was bled and purged freely, cold applications put to the nape of the neck, blistered, &c. Under this mode of treatment he gradually improved, and at the end of ten weeks left the hospital pretty firm on his feet, and with some power of his arm. It was ascertained, however, that some weeks after his return home he had another fit of apoplexy, in which he died. The history of this case, as Dr. Bright observes, is not very mysterious: there was here clearly long irregular distribution of blood, probably accompanied by diseased vessels; extensive sanguineous effusion into the left hemisphere, perhaps injuring the posterior part of the corpus striatum. This effusion having ceased, the process of reparation went slowly on; some fresh excitement induced increased flow of blood; the injured vessels again gave way; the effusion became more extensive, and the blood probably burst its way into the ventricles; but the exact circumstances of the patient's death are unknown. "Had he been contented," says Dr. B. "to remain quiet for some months longer, it is very possible that the injury would have been more completely healed, and that, with care, he might have lived many years; but his arm would never have been perfectly restored."

General Observations on Apoplexy.

With regard to apoplectic attacks generally, it will be observed, and the observation is borne out by a copious induction from the cases adduced by our author, that, in the first place, neither age nor sex affords security: a majority, however, of those attacked have been males, and these chiefly above the age of forty, and many above sixty years of age. The spare and tall, as well as the short and robust, have been subject to the disease, but not in equal proportions; the plethoric frame certainly seems to predispose to that irregular action of the arterial system, and to that tendency to congestion, on which apoplexy and paralysis so essentially depend. Intemperate and irregular habits of living have a large share in preparing the way for an attack; and accidental blows appear frequently to act as strong predisposing causes. Anxiety, mental exertion, and sedentary habits, likewise decidedly predispose. Many severe attacks occur without any previous symptom sufficient to excite alarm; but much more frequently some subordinate seizures have been experienced—momentary loss of consciousness, giddiness, slight defects in the sensibility of parts, frequent irregular or convulsive actions; and at other times sensations of beating within the head, or peculiar noises, or defective vision. On inquiring into the previous history, we not unfrequently find that strong evidence exists of disease in the heart and large vessels, or of great obstruction in the lungs, or of such derangement in the kidneys as is calculated to interfere very essentially with the healthy discharge of other organs.

"When the decided attack has taken place, it varies greatly in extent and severity: sometimes it is attended with violent pain in the head; sometimes there is not the slightest pain; sometimes it assumes the form of complete apoplexy, the annihilation of all consciousness, the extinction of all sensation, the loss of all voluntary motion:—for a few minutes, or a few hours, the retarded pulse bespeaks the difficulty with which the heart and arteries maintain, by their involuntary actions, the labour of circulation, and the stertorous sound or impeded breathing betrays the inactive condition of those muscular parts through which the involuntary powers are called upon to force the air in the process of respiration; and these soon prove insufficient to maintain life. At other times the apoplectic condition, though well marked, gradually subsides; or frequently the state of insensibility continues for a few minutes only; while in some attacks the consciousness is never destroyed. But though the apoplectic state should not exist, or though the consciousness should have returned, yet if effusion of blood have taken place, paralysis will generally remain. The extent of this paralysis will vary almost indefinitely; it frequently affects both the motion and the sensation of the same part; sometimes, however, the motion, and sometimes the sensation, suffers in the greatest degree; and occasionally the sensation of one part and the motion of another are more strikingly influenced. Hemiplegia is by far the most common form which paralysis assumes from effusion of blood within the cranium. I have, indeed, never met with a decided instance of paraplegia from this cause: occasionally one leg or one arm will be affected, without the other limb on the same side suffering materially; but those forms of paralysis, which occupy almost exclusively the two upper or the two lower extremities, very rarely result from the sudden effusion of blood in the brain. Cases occur where paralysis of the two lower extremities has appeared to depend on other disease or injury in the brain; but of these we should always be somewhat sceptical, from the obvious sources of error to which they are liable; amongst which, the unobserved or the unsought diseases of the spinal cord and its membranes are the most to be suspected. Defective articulation and deglutition, either alone or as attendants upon hemiplegia, are likewise common results of apoplectic seizures. Occasionally great pain is experienced in the affected limbs, while at other times a sense of numbness alone is felt. The powers of the mind generally suffer in some degree, but this varies greatly:—sometimes the mind evinces great irritability, and sometimes a childish tendency to excitement, and a trifling turn quite inconsistent with the former disposition, or with the present situation of the sufferer:—at other times the patient falls into a dull state of imbecility; while cases occur in which the affection of the mind is so slight, that it is only by close examination it can be detected."

Treatment.—The most important of all remedies in apoplexy, is the judicious employment of bleeding. It is clearly indicated for the removal of congestion in the brain, or with a view of checking the hæmorrhage, or to reduce the general plethoric condition of the system. When apoplexy simply depends upon congestion in the brain, a large and free abstraction of blood almost immediately effects a cure of the disease. When rupture and effusion have actually taken place, and the slow and labouring pulse shews to what a degree the vigour of the nervous system has been depressed, it is still our duty to bleed, and to bleed largely. Small bleedings, however, will rather do mischief, by increasing the chance of hæmorrhage as the force of the circulation is increased. After bleeding, the pulse must be watched, and if it increase in rapidity, still maintaining considerable force, we must bleed again; but if we find that after two free bleedings, in which we have abstracted forty or fifty ounces of blood, no decided benefit is derived, we must remember how much it behoves us to husband the small remaining stock of vital energy, which, owing to the patient's habits of life perhaps, had been, even previous to this abstraction, greatly diminished. Cupping is to be employed only after venesection has been fairly tried; experience leads us to think favourably of the practice, and perhaps by applying the glasses either to the temples or behind the ears, we may more immediately, and often very expeditiously, draw blood from arterial branches of considerable size with the most decided benefit. How far the application of cold to the head is generally useful, admits of some doubt, as a means of checking hæmorrhage when it is taking place from parts which have strong sympathy with the skin, or are almost continuous with it, we have the strongest proofs of its efficacy. Epistaxis and uterine hæmorrhages are good examples of this; though in apoplexy the circumstances are not precisely analogous. There is much danger, too, it ought to be observed, in a long-*continued* application of cold to the head in apoplexy, when there is reason to apprehend a tendency to delay in the circulation, or probable congestion. Where congestion exists, the *sudden* application of cold is most desirable—and sometimes even when hæmorrhage is taking place within the substance of the brain; but it would undoubtedly be hazardous, as Dr. Bright very properly observes, when employed with this view; for it might throw the impulse from the external to the internal circulation, and thus rather increase than diminish the extravasation of blood.

We give the remainder of Dr. Bright's treatment in his own words, without abridgment:—

"Purgatives are of great importance in every stage of these diseases. It very frequently happens that the immediate cause of the attack is the neglected state of the bowels, and nothing tends more to favour cerebral congestion than a loaded condition of the alimentary canal. When the patient lies under the first impression of the attack, he is often quite unable to swallow medicines, in which case the oil of croton is the most active and appropriate remedy of this class. We should be careful not to administer calomel before the powers of deglutition are sufficient to ensure its being swallowed. I once saw most serious consequences result from this; for having put five grains of calomel on the tongue, and attempted to wash it down with a cathartic draught, the calomel, instead of passing into the stomach, remained, moved about by the tongue, and produced in a few hours the most alarming ptyalism, in which the tongue was forced out of the mouth, and it was necessary to scarify it deeply before it could be returned within the teeth. If, however, the patient can swallow well, a dose of calomel with extract of colocynth, followed by castor oil, is a very proper purgative. Cathartic injections may also be used with much advantage. In some cases, where the paralysis has been less complete, where paraplegia has occurred, or where there has been a combination of hysteric irritation with the paralytic affection, purging has sometimes been the chief means of cure.

"Diuretics are likewise of great utility; indeed the importance of attending to the action of the kidneys may easily be inferred from the perusal of the foregoing cases, in which the connexion between the apoplectic state and their defective action has been so frequently traced: and those who have seen much of diseases cannot be ignorant, that even the undue retention of the urine in the bladder is liable to produce great cerebral disturbance; a fact which is proved continually during the prostration of fever, and in a still more striking manner by the state of coma in which strictures of the urethra and retention of urine often terminate.

“ Blisters to the nape of the neck and the counter irritation produced by the liniment of tartarized antimony, or the continued discharge from a seton, often promote the cure when the first severity of the disease is passed away, and when we have reason to suppose that morbid action is interfering with the healing processes of the system.

“ The time, however, soon arrives when it is necessary to give tone and even to stimulate the torpid nerves, which appear in a certain degree, even after they may be considered as organically capable of resuming their functions, to continue inactive from habit; and this is very much the case in the subordinate attacks of paralysis, where no very extensive organic lesion has occurred. It is at this period that the balsamic preparations are of utility, and that the mineral tonics are employed with advantage: the sulphate of zinc and the arsenical solution act occasionally very well. The nux vomica is a remedy which has long been employed during the state of nervous torpor which attends the tedious convalescence after paralytic attacks, and of late the more active principle of that drug, the strychnia, has been substituted: it tends to excite an action in the nerves, which, though not in itself a healthy action, may counteract the morbid state to which they are reduced. In a case of local paralysis, I have applied this powerful remedy in doses of the eighth, the quarter, or half a grain, to a blistered surface, with the effect of producing spasmodic action through the paralysed muscle, and I have sometimes administered it internally with advantage. But cases of hemiplegia from the rupture of vessels are not those in which this remedy holds out the greatest prospect of success, though with caution it may be employed in the advanced stages of convalescence, with safety at least, and sometimes with benefit. Exercise of the affected parts, and friction, are of use; and, by degrees, moderate and cheerful occupation of mind, avoiding excess and anxiety, will rather promote than retard recovery.”

Warning to the British Public against the alarming approach of the Indian Cholera. By Sir Gilbert Blane, Bart. F.R.S.

The benevolence which dictated the publication of this little tract will, we have no doubt, be generally appreciated. The venerable baronet, filled with just apprehension for the safety of his fellow-countrymen, lifts his pen for the most praiseworthy of all purposes, and imparts his advice with the earnestness of a dying injunction. He endeavours to impress upon them the horrible nature of the malady with which they are threatened; gives a short sketch of its history; distinctly expresses his belief of its contagious nature; and concludes with some plain practical suggestions for its treatment when it is among them. As we heartily concur with Sir Gilbert, in thinking this to be one of the most alarming visitations with which England has had to contend for many years, and that too much pains cannot be taken to impress this opinion upon the public mind, accompanying that impression with salutary advice, we will not stay to find fault with some minor points which struck us in the perusal, but recommend the “ Warning” generally, to the notice of all.

Cholera, its Nature, Cause, Treatment, and Prevention, clearly and concisely explained. By Charles Searle, Esq. of the Hon. East India Company's Madras Establishment.

Without attending to the rather uncomplimentary reflection on medical critics, thrown out by Mr. Searle in his Preface, we proceed to notice the substance of his pamphlet as practically as he could wish, premising merely, that, concise and clear as his little work, on the whole, undoubtedly is, it could be rendered still much more so by leaving out the third and fourth chapters, which profess to treat of the “ cause of cholera,” and of the “ operation of the cause in producing the phenomena, &c.” We have not patience for dwelling on those pages of a writer professedly practical, which treat *de non apparentibus vel non existentibus;* suffice it, for the benefit of those who may not see the work, to mention what the author holds to be the "one essential agent, and immediate cause of the disease;" which is, "*a mephitic vapour or miasm,* of the nature of, if not identical with, *malaria,* in the common acceptation of the word, which being received into the system by respiration, contaminates the blood with its poisonous agency, and, in common with the other poisons of a *sedative* class, of which it is one, operates immediately and specifically upon the capillary vessels, in depreciating and arresting their functions; and hence, as they enter into the composition and structure of every organ and part throughout the system, and as their function is nutrition, secretion, calorification, and, in short, all that appertains to the life and health of the part and system in general, it necessarily follows that the depreciation of their action is attended with *debility* of all the functions, &c. &c.". We fear this will not pass for a good specimen of the author's ability in getting up a useful popular pamphlet; we therefore turn to something really more valuable, or at least less questionable and obscure.

Mr. Searle divides the symptoms of cholera (the cholera, as it appeared in Poland, we presume—the pamphlet is dated from Warsaw) into three stages. In the first, or that of invasion, the patient feels giddiness, and disturbance of the stomach and bowels; then vomiting comes on, with evacuations from the bowels of a sero-mucous or barley-water-like appearance; prostration of strength, twitchings, pain, numbness and cramp in the extremities; sunken dusky countenance, ringing in the ears, cold skin, and feeble pulse. In the second stage, or that of excitement, inward burning heat at the pit of the stomach, attended with great thirst; pulse sharp and frequent; head and trunk somewhat more warm; tonic spasms gradually involving the muscles of the belly and chest. The third stage, or that of collapse—which often follows the first stage, without the intervention of the second, or that of excitement—is characterized by the cessation of the spasms; livid extremities; cold clammy skin; countenance death-like; coma and dyspnœa—with death ensuing in ten or twelve, but generally within eight-and-forty hours from the period of invasion. There is considerable variety, however, in the symptoms, as they appear in different individuals and under different circumstances: the spasms are sometimes not distinguishable; sometimes a mortal coldness comes on from the beginning, and the patient dies without a struggle.

We have, on a former occasion, given some of Mr. Searle's peculiar opinions with regard to the exhibition of *common salt* in this disease: we may now add, on this head, that he continues to recommend that practice most strenuously, as the best beginning of the treatment.

"As a deranged state of the stomach is, I am of opinion, no unfrequent exciting or predisposing cause of the attack, or if not, as indigestion invariably succeeds to it, as a general rule, I think it always advisable to commence the treatment by evacuating the stomach, whether the patient has vomited before or not. With this intention let the patient drink freely of warm water, or (which I am of opinion is better, as it operates almost instantly, and more perfectly accomplishes the purpose, and is always available, and from its stimulant and mild aperient qualities is useful also with these intentions)—a *large table spoonful of common culinary salt,* (muriate of soda) dissolved in half a pint of hot water, and drank as warm as it well can be: should it fail in producing the desired effect, which however is but seldom the case, a second dose may be repeated five minutes afterwards. Prior to the emetic, I think it however advisable, whenever the state of the patient admits of it, particularly if there be pain in the head, or oppression about the præcordia, that the patient be bled from a small orifice, in the recumbent posture, to a moderate extent, as this tends immediately to the relief of these symptoms, which are dependent upon accumulation of blood in these parts; and the emetic following the bleeding to the restoration of an equilibrium of the circulation."

We need not follow the author in the other particulars of his practice in cholera, which are those generally recommended by most writers, and already detailed with sufficient minuteness in our pages. Calomel he prescribes in large doses, because, says he, "in effect, in this disease large doses are but equal to small ones under ordinary circumstances, from the diminished absorbing power of the stomach and susceptibility which assists to its influence."

Twelve grains, he thinks, ought to be the first dose, "which being simply placed upon the tongue, should be washed into the stomach by a cordial; and a very suitable one is a dessert or table spoonful of brandy, with double the quantity of hot water; and the same dose of calomel should be repeated every hour or two, according to the urgency of the case, and commensurate with the improvement of the patient in smaller doses, should it be continued, till bilious stools and urine are restored, when it may be given in conjunction, or alternately with some mild aperients, till healthy secretions are established."

In his directions with regard to articles of diet and sustenance craved by the patients in cholera, we fear Mr. Searle is too indulgent in consulting so freely the wishes of the sufferers, notwithstanding that he pleads the observation of twenty years practice of medicine to bear him out in the proceeding; but we highly approve of the greater part of his treatment, and more particularly of the sensible remarks which he offers for the benefit of those who are wise enough to take timely precaution. We need scarcely add, that he advises temperance and moderation above all things; and the following hint ought not to be neglected:—

"In cholera times, it is particularly advised that you should not leave home in the morning with an empty stomach; a cup of coffee, with or without a teaspoonful of brandy in it, and a biscuit, is indispensable, if too early for breakfast. And a light supper, with a little spirits and water, is no bad thing before going to bed, which should be early."

We bar the supper, however; the cup of coffee and the biscuit, as a stay before breakfast, may do very well.

The pamphlet concludes with (we think very unnecessarily) a reiteration of the author's continued adherence to his opinion that the disease is not contagious. We take leave, with regard to this point, to refer him to our leading article in the present number.

Practical Remarks on the Disease called Cholera, which now exists on the Continent of Europe. By John Goss, M.R.C.S. and late Assistant-Surgeon in the Hon. East India Company's Service, Bombay Establishment.

On Cholera Morbus. By Medicus.

In the first of these productions, the only thing we discover worthy of notice, is the author's announcement of the strong acidity of the contents of the stomach when ejected in cholera. "I was sent for," says Mr. Goss, "to an old native woman, who was labouring under all the symptoms of the second stage of the disease. I applied carbonate of soda to the fluid she vomited, which made it effervesce most powerfully; I *therefore* administered about a drachm of the soda, with ten or fifteen grains of the carbonate of ammonia." The woman recovered, and the author, of course, thinks himself justified in giving the soda plan the strongest recommendation. But the means on which he would place his whole reliance are these—bloodletting to the fullest extent that the system will bear—constant frictions, with a camphor cantharides and turpentine liniment—a mustard poultice to the abdomen—soda and ammonia as above mentioned—and, finally, calomel and jalap. Some absurd arguments are ventured in favour of the non-contagionists.

The *brochure* of *Medicus* is a catchpenny—ill written and ill got up. In that part where the writer proposes his treatment, he says, he *should not be disposed to cavil with* the universal hot bran cataplasm of the Russians!—though perhaps a vapour bath might be a *more elegant* substitute; nor does he think that in cholera *the bowels* ought *to be neglected*, for which reason he prescribes opium; and "for the safe administration of that drug," combines two grains of it with "a scruple of calomel, washed down with a camphor draught."

Remarks on the Cholera Morbus; containing a Description of the Disease, its Symptoms, Causes, and Treatment, &c. By H. Young, M.D. formerly of the H. E. I. C. Medical Service in Bengal.

At the present moment our readers will naturally expect us to take notice of all most works on the engrossing subject of cholera. The one whose title we now give presents no particular claims to their attention. The history and symptoms are described with tolerable perspicuity, but these are now familiar to all, and there is nothing in the details before us of any novelty. The author supports the doctrine of contagion, and recommends the same general methods of treatment as the rest of his Indian brethren.

THE LATE DR. BECKER'S ACCOUNT OF THE CHOLERA AT BERLIN.

Copy of a Report of Dr. Becker, of Berlin, to Mr. Chad, his Majesty's Minister in that capital.

Oct. 12, 1831.

THE comparatively small number of persons attacked with cholera in the city of Berlin, during the first month of the prevalence of this disease, seems to be owing chiefly to the following causes:—

1. The manner in which Berlin is *built*, compared with other populous cities. It has few crowded and narrow streets, and a great proportion of the lowest classes of the population inhabit the outskirts of the town, where the streets are large and distant from one another.

2. The attention which has before and since the appearance of the disease been directed to the necessity of *avoiding cold and intemperance*. The advice given on this subject by the civic authorities, as well as by the medical men, which, as everybody can read here, must have reached all classes of the population, has been practically assisted by food and clothing distributed among the poorest inhabitants. The almost complete immunity of the garrison of Berlin from cholera is chiefly to be ascribed to the pains taken to give the soldiers additional warm clothing, and to force upon them a healthy diet.

3. The measures taken to prevent the spreading of the contagion. The method adopted is, when a case of cholera occurs in a family, to put those who have had intercourse with the patient, as well as the patient himself, under a sort of quarantine: this lasts for five days after the patient has died, recovered, or been removed to an hospital. It is left to the choice of the friends whether they will pass this time in their habitation, or go to houses appropriated to the purpose of receiving them (*contumaz*, anstalten). During the time the rooms in which the cholera has prevailed are cleaned, and '*disinfected*' with chlorine*.

All these measures are directed by the local committees, of which there are 61 in the city and suburbs, consisting of respectable inhabitants, medical men, and a police-officer, and having in their service men who devote themselves to nursing the sick, and preventing intercourse with other persons. These servants of the committee are generally lodged in particular houses, where they are always to be found. It cannot be said, however, that these regulations are, in all instances, rigidly adhered to; they are often neglected, owing to the prejudices of the patients and their families, who conceal the disease; the remissness or connivance of the medical men, who do not wish to cause inconvenience to themselves and their patients; and the inactivity of some of the committee.

Experience has shewn that the spreading of the cholera has been most efficaciously prevented when the patients have been *immediately removed to an hospital*, and where those who had lived in the same room with them also left the infected house, and returned to it only after it was cleaned and aired. This measure having been adopted wherever circumstances admitted of it, may, in fact, be regarded as the chief cause of the very moderate extent to which the cholera has till now prevailed.

In reply to the other queries put, the following answers may be given:—

1st. What description of persons has most suffered from the disease at Berlin?

The great majority of persons attacked with cholera in Berlin consists of those who, on the one hand, are exposed to the usual causes of disease, viz. cold, fatigue, and particularly intemperance in food and drink, or are labouring under previous disease, particularly diarrhœa; and, on the other hand, are, by their business, or by chance, brought into intercourse, direct or indirect, with those already attacked by the disease, and *with their dead bodies.* Of course the greatest number of patients occur in the lower orders, who form the bulk of the population; but there have not been few instances among persons in easy circumstances; in them also, previous disease, intemperance, fatigue, or anxiety of mind, have generally, although not uniformly, preceded the disease.

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* Of the superior efficacy of this substance, however, there is no evidence.

2dly. Whether the disease was confined, for any length of time after its appearance, to one street or one district of the city?

The first cases of cholera in Berlin occurred among the skippers on the boats lying *on the river Spree*, which flows through the town, and in *houses in the immediate neighbourhood of the river.* The disease has prevailed to a considerable extent in all those streets which lie along the navigated branch of the river, and whose inhabitants at the same time live in frequent intercourse with the skippers and fishermen. On the fourth and fifth days cases appeared in other parts of the city, and, in many instances, they were those of individuals who were known to have had intercourse with cholera patients, or at least with the boats lying on the river, and with the streets first infected. In the part of the city chiefly inhabited by people in easy circumstances (Friedrichstadt) such cases generally remained solitary, and the disease did not spread in the streets where it had thus appeared: in those districts, on the other hand, which are peopled by the labouring classes (in the centre of the town, and in the eastern suburbs), the disease once having occupied one house of a street, was observed to attack other houses successively. Three weeks after its commencement the cholera had spread in all directions, without any regard to situation, high or low, damp or dry, or to exposure northern or southern, eastern or western, but occurring in solitary instances only in the Friedrichstadt, whilst it found numerous victims in four or five other quarters remote from one another, and inhabited by the poor.

3dly. Whether, when the disease attacked one member of a family, the other individuals in the same family suffered from it?

This is the case so frequently, that it may almost be considered as the rule, and the contrary as the exception, unless the patient be removed from his family. It is not possible to give any numeric returns of the recurrence of the disease in *families*, but the following is a statement of its re-appearance in the same *houses* where it had shewn itself:—

From August 29th to September 26th, there have been reported cases of cholera in Berlin, 770. During that period, a second case has happened in the same house where one case had been reported:—After one day, 65 times; two days, 34 do.; three days, 23 do.; four days, 16 do.; five days, 21 do.; six days, 7 do.; seven days, 3 do.; eight days, 2 do.; nine days, 0 do.

In order to illustrate this point, it may not be superfluous to mention some instances of the recurrence of cholera in the same families and buildings:—

1. A physician (Dr. Callow), who had attended cholera patients, being at the time in bad health, and labouring under diarrhœa, died of cholera; on the day following, his landlord (Mr. Steibelt) died; a day afterwards, two children of the landlord died, and the servant-maid was taken ill of cholera, and recovered. The landlord's wife had been removed to quarantine. There have been no more cases of cholera in this street and its neighbourhood.

2. A family, living on the river side, consisting of husband and wife, four children, and a servant-maid, were *all successively* attacked with cholera; only the husband and one child survived.

3. In a house (Alte Jakobstrasse, 66,) the following cases of illness have occurred:—1. Sept. 7th, W. M. 27 years of age, seized by cholera; recovered. —2. 8th, Miss M. 32 years of age, with vomiting and purging; recovered after twelve hours.—3. 11th, A boy of two years and eight months, son of a cutler, cholera; died on the 12th, after eleven hours' illness.—4. eod. A journeyman of the cutler, vomiting and purging; recovered. — 5. 13th, The child of a tailor, aged two years and nine months; died of cholera after nine hours' illness. —6. 15th, An apprentice of the cutler, vomiting and purging; recovered.—7. eod. The man who had been in attendance on No. 5 took the cholera, and was sent to the hospital.—8. 16th, Anothera apprentice of the cutler, vomiting and purging; recovered.

4. The *workhouse* (arbeitshaus), a large building occupied by a numerous

poor population, had a cholera patient, on Sept. 3d, *five* days after the appearance of the disease in the city: the following cases successively occurred in this building:—Sept. 3d, 2 cases; 5th, 2 do.; 11th, 1 do.; 15th, 1 do.; 16th, 5 do.; 17th, 7 do.; 18th, 6 do.; 19th, 9 do.; 20th, 4 do.; 21st, 6 do.; 22d, 2 do.; 23d, 6 do.; 24th, 3 do.; 25th, 2 do.; 26th, 4 do.—60 cases.

Immediately beside the workhouse there is another building (Familienhaus), inhabited by a great number of very poor families, who there find employment of various kinds: here the disease did not show itself till September 8th, *five days after the first case in the workhouse:* from that day to the 26th Sept. 27 cases occurred in this house.

It may finally be stated, that the manner in which the cholera has appeared and spread in Berlin, perfectly warrants the conclusion, that this disease is always produced by a peculiar human effluvium, which in its mode of generation and effusion shows some analogy with that of the *common typhus fever of Great Britain,* but extends its deleterious effects to a limited number of persons only, who are rendered eminently susceptible by cold, intemperance, fatigue, fear and anxiety, diarrhœa, and other previous diseases.

(Signed) F. W. Becker, M.D.

Berlin, Oct. 5, 1831.

PARISIAN BOARD OF HEALTH.

Extract from the Instructions given by the Central Commission of Health, in Paris, to the Minor Commissions of Districts and Parishes (d'arondissement et de quartier).

In the event of the arrival of this awful epidemic, it will behove all good citizens to combine their efforts with those of government to purify the city as perfectly as the localities will permit.

The commissions are composed of medical men, persons of professional distinction, so selected as that each may be employed in that line for which his talents and his experience adapt him. To these are added persons unconnected with the profession, but whose standing in society is most respectable, and who have more time and local knowledge to assist and direct the proceedings of their colleagues. The civil power has received orders to second the business of the commissions.

The parish commissioners are to visit *every house* in their parish, and point out to the inhabitants whatever they find amiss, warning them of their danger and of the propriety of setting all to rights before the police interfere. In these visits it will be particularly necessary to ascertain the state of the privies, leads, gutters, and channels into which the foul water of the house flows; also the wells and cesspools; and to look to the stables, that their floors have the proper inclination and allow the fluids to run off from the dungheaps which are often suffered to accumulate.

Establishments—such as schools, nurseries, *maisons de santé*, and places inhabited by dog-keepers, pig-feeders, rabbit-dealers, pigeon-fanciers, who seldom remove the offal; also the dwellings of rag-men, graziers, stable-keepers, bathmen, tanners, gut manufacturers; and, in short, all workshops which become nuisances by bad smells and want of cleanliness—will have need to be examined with the greatest care.

The commissioners will also see that the public streets in their respective parishes are properly paved and in good order, well swept, with the channels running free and cleanly washed; that the reservoirs of the fountains are in good repair, and sufficiently numerous; that the places of public convenience are well kept, and as many as need be; and whether there should not be new *urinoirs* established in certain situations.

The reports of the parish commissioners are to be made to the commissioners of districts, which last are to communicate with the central commission, and also to give their personal attendance in visiting those places from which they have not received satisfactory reports. The district commissioners are also to call upon the principal shopkeepers and artisans, and to solicit their exertions among their class for the more perfect purifying of the city; to insist upon the necessity of neatness in their houses and dress, and of temperance, the want of which produces such aggravation in the progress of an epidemic.

The business of the central commission is to discuss and digest the reports received from the district commissioners; to form the grand point of re-union of every thing concerned in these works; and to act immediately in connexion with government.

Members of the central commission engaged in drawing up this document, MM. Marc, J. Pelletier, Girard, Darcet, and Lucien Delamorliere.

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

BY

DR. ANTHONY TODD THOMSON.

LECTURE IV.

November 4, 1831.

Acute Hydrocephalus—Epilepsy.

GENTLEMEN,—The principal object of this lecture is, to direct your attention to the very interesting and highly-instructive case of acute Hydrocephalus, which was admitted on the 27th of last month, and which has terminated fatally—the too frequent result of cases of this formidable disease. As the little patient was necessarily confined to bed, and visited at home, a few of you only have had an opportunity of observing the progress of the malady; I will therefore enter a little more minutely into the history of the case than would be otherwise necessary.

George Harker, three and a half years of age, a fine intelligent boy, with light hair and blue eyes, was stated to be too ill to be brought to the Dispensary; he was, therefore, visited at his father's house. On inquiring into the history of the case, we were informed that, a year and a half ago, this child fell down an area, and struck the back part of his head with violence; he lay for two hours in a state of insensibility, with the eyes fixed. Since the occurrence of that accident he has been less playful than children of his age usually are; occasionally subject to attacks of dizziness; and, more than once, has suddenly fallen down. Six weeks since, when in the country, he caught a severe cold, was brought to town, and soon afterwards placed under the care of a medical gentleman, who lanced the gums under the impression that his complaints arose from dentition. On Friday, the 21st of October, he was seized with a violent pain of the head, which continued without intermission. The cough and difficulty of respiration, which were brought on by the cold, still remained, but only in a slight degree, having diminished from the time that the pain of the head came on. The bowels were relaxed, and the stools dark-green and offensive. Such was the account given by the mother.

At the time of taking the report the boy was lying on his back, the head a little inclined to one side, and the eyes closed: he expressed great intolerance of light. His mother told us that he cried for many hours together, sometimes uttering a loud and prolonged scream. He was very restless, especially at night, rolling his head on the pillow, and sometimes jumping up in bed. He was perfectly sensible, recognizing every one of his family; and put out his tongue when ordered to do so. There were frequent flushings of the cheeks, followed by dampness of the skin. The pulse was 116, full and soft; the skin was dry; the temperature moderate; the tongue little furred; the urine scanty, high coloured, and offensive to the smell. He refused food. On examining the head, it appeared larger than in children of a similar age, measuring 21¾ inches round, and 12 inches across the head, from ear to ear, in the direction of the coronal suture. Both parents are healthy, and not subject to complaints of the head. Two other of their children, however, died with symptoms closely resembling those of this boy; but neither of them had met with any accident. Ten Leeches were ordered to be applied behind the ears; and a powder, consisting of six grains of Calomel and eight of Jalap, was directed to be taken after the bleeding, and the head to be kept moist with a cold evaporating lotion.

On the second visit we found that four Leeches only had been applied; but the mother assured us that much blood had flowed after they dropped off. The same degree of restlessness, the rolling of the head on the pillow, and the screaming, continued; the pulse was 120, thin, and hard; and the stools had assumed the slimy, glazed appearance, which is so pathognomonic of acute Hydrocephalus. The pupils were not dilated; the tongue was still but little furred, and moist. Four grains of Calomel and three of Colchicum, were ordered to be taken every eight hours, and two table spoonfuls of a solution of six grains of Tartar-emetic in six fluid ounces of distilled water.

On the 1st of November the pupils were much dilated; he squinted, and complained of not seeing distinctly; there was some degree of delirium, and the pupils were occasionally turned upwards. The stools had lost their green colour and glazed appearance, and were pale and pasty; the tongue had become more furred, but was still moist; and this, you will find, is frequently the case when the brain is affected. The same plan of treatment was continued; and, as the head was hot, the application of the lotion strenuously enjoined. On the afternoon of this day convulsions supervened, and continued with slight remissions until ten o'clock on the following morning. The pulse was 148, and bounding; the pupils were still sensible to light, but the patient appeared incapable of recognizing any one. At noon (November the 2d), the whole face was suffused with a deep scarlet blush; the eyes were protruded, the pupils much dilated, and the teeth fixed as in trismus, whilst the breathing was stertorous. The thumbs were thrown across the palms of the hands, and the two first joints of all the fingers rigidly bent inwards; the pulse was hard, bounding, and 150; the temperature high; and there was an evident tendency to opisthotonos. He had passed scarcely any urine since yesterday, and only one stool, which was dark-green and pasty, but not glazed. The head was very hot, although the lotion had been sedulously applied. The temporal artery was ordered to be opened, but, before five ounces of blood flowed, a state of collapse came on, which seemed to indicate a rapid fatal termination of the case; nevertheless the trismus did not yield. The breathing, however, lost the apoplectic character, but he became torpid, and continued so until he died. Enemas, composed of small quantities of White Wine Whey and four drops of Tincture of Opium, were directed to be administered every hour; and a large blister was applied to the nape of the neck. In seven hours afterwards he died.

Post-mortem Examination of the Body.—On laying bare the cranium, the sutures appeared more than usually consolidated in so young a child; and the adhesion between the dura mater and the bone was so great, that the skull cap was with much difficulty raised. The vessels on the surface of the brain were greatly congested; as were those also running through its substance; the medullary part displaying an infinite number of large bloody points. The whole surface of the brain was bathed with serum, and about two ounces of fluid were collected in the base of the cranium, passing down into the cavity of the spinal column. The appearances of inflammation were most obvious on the medulla oblongata, and in the sheath of the spinal marrow, particularly in the pia mater, which was beautifully injected. The quantity of fluid in the ventricles was comparatively small. The substance of the cerebellum was healthy. On opening the abdomen, the liver was found in a diseased state, studded with dark patches, which penetrated the substance of the viscus; several small tubercles, not larger than millet-seeds, were also observed in it. The spleen and the pancreas were healthy; but the mesenteric glands were enlarged, and some of them contained suppurating tubercles. The rest of the abdominal viscera were in a natural state.

Few cases, gentlemen, will come before you more fraught with important matter for remark, both in a theoretical and practical point of view, than that the report of which I have just concluded. In tracing the origin of the disease, we should be tempted to refer it to the accident which occurred to the child eighteen months ago; but I am of opinion that that concussion impressed upon the brain a predisposition to take an inflammatory action, rather than that the inflammatory state of the menengies then commenced. It is true that the child displayed less sprightliness than usual in young children after the accident, but it is pretty evident that it was not until after he caught cold, six weeks ago, that the inflammatory action was set up; and there seems to have been a translation of diseased action from the lungs to the head, as the cough abated when the head became affected. How far the state of the brain, induced by a severe blow on the head, resembles that which may be denominated *hydrocephalic*, and which seems hereditary in some families, it is difficult to determine; but that such a state exists there can be no doubt, and there is as much propriety in talking of a *Hydrocephalic* as a Scrofulous, or any other diathesis: the constitutional disposition to many diseases is obvious, long before the habit is actually under the influence of the disease to which the diathesis is supposed to contribute. In this state of the brain, however it may be brought on, a diseased condition of the liver, or some other of the abdominal viscera, not unfrequently disposes the meningeal vessels to take on that action which terminates in effusion of serum, either upon the surface of the brain or into its ventricles. In our little patient's case, the primary diseased action was evidently in the lungs.

The knowledge of the fact, that, in families in which the hydrocephalic diathesis prevails, a transference of diseased action from any part of the body to the head is always likely to occur, points out the importance of early attention to even the slightest inflammatory attacks in such individuals; and more especially when the abdominal viscera are affected. In such a case as that now under consideration, I am of opinion that much security may be reasonably anticipated from establishing an issue, so as to divert the determination of blood to the head, and from constant attention to the bowels. In the present case, both the liver and the mesenteric glands were diseased; but it is probable that these were consequent on the state of the head, arising from the fall: and daily experience will teach you, gentlemen, that, in cases of cerebral derangement, conjoined, as they almost always are, with disease of the hepatic and chylopoetic organs, nothing is so difficult as to determine whether the brain or the abdominal viscera be the primary seat of disease. The dizziness and falling down, which occurred before our little patient caught the cold which proved the exciting cause of this affection of the brain, indicates a state of the organ similar to that which sometimes precedes attacks of Epilepsy; and if the life of the patient had been spared, he probably would have suffered from that disease.

The next observation to which I have to direct your attention, is the position of the patient on our first visit. He was lying on his back, with his eyes closed, to shade them from the painful stimulus of light. This affords me another opportunity of making a few remarks on the physiognomy of disease. When the brain is affected, the power of volition over the muscles is much diminished, sometimes wholly suspended; consequently, the position on the side, which is natural to us during rest, in a healthy state of the body, cannot be maintained, and the patient turns on his back. By observing this position, therefore, when it is attended with screaming, rolling the head on the pillow, and jumping up in bed, you may always form a pretty accurate idea of the state of the brain. These symptoms, however, mark a high degree of irritation rather than one of acute inflammation; but if the means taken to obviate this condition prove insufficient, the acute inflammatory action is rapidly set up, and runs its course to a fatal termination. It is true that the previous symptoms often so closely resemble those connected with Worms, or with Dentition, that the diagnosis becomes extremely difficult; but when we see the patient always lying on his back, and rolling the head on the pillow, we can scarcely be mistaken in pronouncing the disease to be *Acute Hydrocephalus.* In employing this name, gentlemen, I do it merely in conformity to established custom, for the Hydrocephalus, or Dropsy in the Brain, is certainly not the disease, but the consequence of the disease.

The thumbs thrown across the palm of the hands, and the clonic spasm of the flexor muscles of the two first phalanges of the fingers, with the accompanying trismus, indicate a high state of cerebral excitement, particularly on the base of the brain, involving the origin of the motor nerves within the cranium; and this the post-mortem dissection demonstrated to be the case, in a very striking manner. The crossing of the thumbs is not an unfrequent symptom of visceral irritation affecting the brain and medulla oblongata, in severe cases of dentition, and occasionally in irritation from worms: in these cases, the spasm is generally resolved by correcting the condition of the abdominal viscera by active purgatives, so as to make some impression on the brain; but when purging fails, bloodletting, followed by calomel and opium, must be resorted to. Trismus is a very uncommon termination of Acute Hydrocephalus, and can only be accounted for by the extension of the inflammation of the pia mater to the medulla spinalis; for, in this case, there was nothing in the state of the abdominal viscera to which it could be referred. Late investigations, particularly those of the continental pathologists, have also discovered an increased vascularity of the spine in all cases of tetanus; indeed, when we consider that, besides the fixation of the jaws, the diaphragm, and abdominal muscles, the trapezius and the serratus magnus auticus are thrown into spasmodic contraction, we can only trace these effects to inflammation affecting the origins of the par vagum, the spinal accessory nerve, the phrenic and external respiratory nerves, or to those nerves affected by some other powerful irritating cause. That the former is the most probable cause, may be inferred from the permanent or clonic nature of the spasm.

The turning up of the pupils has been commonly regarded as indicating the effusion of fluid within the cranium; and, as it always appears towards the close of this formidable disease, the vulgar say "that children, with water in the head, are looking to their final home." It is said, that vulgar sayings are generally founded in fact, and this is true as far as this turning up of the eyes is the prognostic of great debility: but it is only when it occurs in our waking hours that it is so, for in sleep it is always present; and, therefore, we may all be said to be looking towards our final homes during sleep. You will read in Dr. Good's Study of Medicine, and other medical writings, that both the turning up of the pupil and strabismus is the result of spasmodic action, but the discoveries of Sir Charles Bell have demonstrated that this is an erroneous opinion. The turning-up of the pupil is merely an indication of great debility extended to the voluntary muscles of the eye, leaving the obliqui to act with relatively greater power, and it is by their operation that the pupil rolls upwards. The same thing, as I have already said, occurs in sleep, but it is not observed, owing to the eyelids being closed. When any circumstance occurs, however, to prevent the eyelids from closing, then it becomes visible. This is well described in a letter from Mr. Jackson, of Sheffield, which Sir Charles Bell has published in his admirable work on the Nervous System. The patient, owing to partial paralysis of the face, could close the lids of one eye only in sleep, whilst the other remained uncovered. "The right is closed," says Mr. Jackson, "and the upper eyelid on the left side remains as in the state of ordinary vision, whilst the inferior margin only of the cornea is visible; thus simulating the appearance on the paralytic side of a person in the act of dying."

No feature, nevertheless, is so indicative of the state of the brain as the eye. In the early or inflammatory stage of Acute

Hydrocephalus, they are generally suffused, watery, and intolerant of light: when the state of oppression supervenes, the pupils become dilated, the eyelids are not closed even in sleep; strabismus, the rolling of the eyeball in its socket, and various contortions are observed. Sometimes in the commencement of the disease we find the pupil much contracted, with knitting of the eyebrows; but these symptoms rather indicate the existence of nervous irritation than inflammation in the membranes of the brain, although it is often mistaken for Arachnitis, into which it may decline, if not properly managed. It may be removed by a course of brisk purgatives, followed by the judicious and cautious administration of opium and light aromatics: if blood be abstracted, it should be in small quantity, and by means of leeches on the temples, or behind the ears.

The green, slimy, glazed looking stools, which are so characteristic of this disease as to be designated *Hydrocephalic*, demonstrate the powerful influence of the nervous energy on the secretion of the liver: instead of the ordinary secretion of the viscus, these green slimy stools are produced. It is of importance not to confound these stools with the dark green alvine discharges which usually follow the administration of Calomel in children; and which seem to depend on the mercurial stimulating the gall-ducts, and thus exciting the gall-bladder to pour out its contents, which, from having been pent up for some time, have become viscid or inspissated, and acquired a much darker green hue than recent hepatic bile. *Hydrocephalic* stools are distinguished from these by being very little acted upon by water, which is scarcely coloured or rendered turbid by them, and by possessing little or no offensive odour. We know too little about the manner in which the liver, or any other gland, separates its secretion from the blood, to explain the nature of the change which produces Hydrocephalic stools; but we know that it is intimately connected with the state of the brain; and one fact of practical utility is worth a score of conjectural theories. If no change occur in this state of the alvine evacuations, a fatal prognosis may be pronounced; but the contrary does not always authorize us to venture on hazarding a favourable opinion; this is demonstrated in the case before us: the stools on the fourth day of the attack lost their hydrocephalic aspect, and became pale and pasty, whilst the disease was rapidly gaining ground. It was almost immediately after this change that the patient became insensible, and incapable of recognizing those about him; and it might be expected, that, as the fluid was poured out within the cranium and the inflammatory action was partly suspended, the function of the liver would become more natural and, consequently, the bile reassume an appearance more consonant with its usual state.

The frequent fatal issue of attacks of acute Hydrocephalus has led to a too general opinion, that it is an incurable disease; and has, unfortunately, paralyzed those efforts that would otherwise have been exerted to trace and avert the causes of the evil. Let me warn you, gentlemen, against entertaining such opinions: every disease, which has a tendency to destroy a vital organ, becomes an incurable disease in the hands of the ignorant and the negligent; but Hydrocephalus, even in its advanced stage, after strabismus, blindness, and convulsions have taken place, as long as the pulse remains moderately steady, and the breathing continues natural, must not be contemplated in inactivity, as if the fatal event were inevitable.

In regarding the disease as an inflammatory state of the cerebral vessels, excited by external injuries, or more frequently by diseased states of the abdominal viscera, sympathetically affecting the brain, peculiarly predisposed by hereditary organization or accidental causes to take on diseased action, we are led to consider two distinct kinds of treatment, according to the period of the disease at the time our advice is demanded: the one *prophylactic*, the other *curative*. It falls to the lot of the practitioner to put the former in practice only when he is aware of an hereditary predisposition to Hydrocephalus, existing in a family, which has that implicit confidence in his opinion that would insure attention to it, even when the reasons for following it are not very evident to common observers; in exercising the second, it frequently happens that the disease has gained too great an influence before his efforts to overcome it are commenced.

As a prophylactic measure in those families in which the Hydrocephalic diathesis is suspected, the closest attention should be bestowed on the state of the abdominal viscera. In the slightest deviation from the healthy state, the hypochondria should be examined; if any tenderness on pressure be felt, leeches should be immediately applied; and if the stools be unnatural, Calomel, and Tartar Emetic, or James's Powder, in conjunction with Jalap, or Scammony, should be administered. But you must also bear in mind that when intestinal irritation is great, drastic purgatives frequently tend to increase it; and, therefore, instead of Calomel, combined with Jalap or Scammony, you will find the Hydrargyrum cum Creta, with the powder of Colchicum, better adapted to subdue irritation and promote healthy secretion. I have found the most obvious advantage to result from a long-continued course of the Wine of Aloes combined with

Alkalies. If, however, notwithstanding the open state of the bowels, headache, or any other symptoms of morbid excitement of the cerebrum, display themselves, then the employment of more active purgatives should be resorted to; but these ought not to stand in the way of local blood-letting, for it is only by subduing vascular excitement in its earliest stage and correcting the derangement of the digestive organs, that we can expect to ward off the influence of those causes, which have called into activity the morbid tendency of the habit, and to aid the powers of the constitution in restoring its healthy functions.

After the attack has really commenced, the depletion should be more active, but nevertheless limited. In the case under consideration, the number of leeches applied was inadequate to the effect, and it is probable that the account of the quantity of blood lost was exaggerated. Although aware of this, yet I was hopeful that, by purging actively with Calomel and Colchicum, with the addition of Tartar emetic in doses sufficient, by its nauseating influence, to abate arterial action, I should be enabled to diminish the general morbid irritability of the nervous system, without depressing too much the powers of life. The low state of the habit previous to the attack rendered much depletion by blood-letting a doubtful measure; and having had many opportunities of witnessing the beneficial effects of active purging in Acute Hydrocephalus, the leeches were not ordered to be repeated. It may be thought that the nature of the purgative to be employed is of little consequence; Calomel, in combination with Colchicum and an antimonial, not only stimulates the torpid bowels, and, by acting on the orifices of the gall-ducts, empties the liver, but it also aids in altering the abdominal secretions and exciting the languid absorbents; consequently it ought always to be preferred. I recollect Dr. Denman having informed me that he had ordered Elaterium in one case; the effect of which was a violent diarrhœa that continued for several days, during which the symptoms of the disease disappeared, and the child recovered. My own experience, also, induces me to place more confidence in purging than in bloodletting in Hydrocephalus. When, however, the symptoms of Apoplexy, with sterrorous breathing, and the scarlet, flushed state of the face supervened, no alternative remained but opening the temporal artery; but, could I have anticipated the sudden collapse which followed, I think I should not have ordered it.

Some benefit might have possibly resulted from the substitution of Croton oil for the lancet; and as the fixed state of the jaws prevented medicines from being exhibited by the mouth, its effects would have been obtained by applying it to the surface, in combination with Opium. As the collapse immediately followed the arteriotomy, the only chance that then remained, to avert the fatal issue, was to arouse anew and to support the powers of the habit. I cannot, gentlemen, urge too strongly the necessity of making yourselves familiar with the distinction which exists between Coma and Exhaustion; and warning you, immediately on the appearance of the latter, to throw aside Calomel, Antimonials, and every other depressing agent, and to have immediate recourse to White Wine Whey, or Brandy and Water, Ammonia, and Opiates, in adequate doses, frequently repeated, until evident symptoms of re-action display themselves. These are readily recognized by the pulse, which will become less frequent and fuller; by the warmth of the extremities returning; and, with the colour of the cheeks, the restoration of the natural expression of the countenance. As soon as this takes place, the bowels should be regulated by gentle aperients, such as Rhubarb and Magnesia, and the stimulants gradually exchanged for light nutritive diet and appropriate tonics. Were it necessary, I could detail to you several cases, in which I have snatched children, as it were, from the very jaws of death, by exchanging the depleting for the cordial plan of treatment, under which every symptom of threatened effusion ha vanished, and the smile of returning health gradually returned to a countenance, which not twenty-four hours before displayed the *facies Hippocratica*—that fatal index of complete exhaustion, and, frequently, too certain harbinger of final dissolution.

It is unnecessary to make any comment on the appearances of the brain and the spinal marrow displayed in the examination of the body; they were such as satisfactorily to account for the symptoms that characterized the disease, and to add another proof, were any wanting, of the importance of morbid dissections.

Epilepsy.

I have no doubt, gentlemen, that you have observed, with satisfaction, the success of the plan of treatment adopted in the case of Leni. He has had no fit since the 27th of last month; but, nevertheless, although the headache has completely left him, yet he still complains of dizziness, and there is a constant winking of the eyes, with suffusion of these organs, and an expression of great anxiety. The pulse is 72, small, and irregular, and the tongue furred. The bowels are open, and the urine has lost its high colour. He has been ordered to continue the use of his pills, and mixture, with the addition of ♏xxiv. of the Arsenical solution. I shall say nothing regarding the reasons for prescribing this tonic, until we shall have seen something of its effects.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

GENERAL DISPENSARY.

Stricture of several years standing, Extravasation and Death—with Clinical Remarks.

James Finlaison, aged 50, residing at No. 112, Aldersgate-street, was seized with shiverings, which came on once in twenty-four hours, and were so severe as to compel him to apply at the General Dispensary. The shiverings were speedily removed by the use of quinine; but Mr. Coulson was requested to see him for a difficulty which he had in passing his water. The patient said, that for twelve years past the size of the stream of urine had been diminishing, and that he had experienced at times great pain in passing his urine, and felt frequent desire to make it. These symptoms had of late very much increased. At this time he could scarcely pass any water, and what came away was passed in drops; there was great pain and a sense of scalding in the urethra, and on examination a swelling was found in the perineum. There was also great constitutional disturbance. It was very clear that some portion of the urethra was in a very diseased condition, and from the pain and scalding in the part, and swelling in the perineum, Mr. C. apprehended that ulceration of the urethra, and extravasation into the perineum, had commenced. A middle-sized catheter was introduced, without the least difficulty, as far as the membranous part of the urethra; but here a firm obstruction was met, and the instrument was instantly withdrawn. A small-sized catheter was tried, and with very little difficulty introduced into the bladder: about half a pint of dark-coloured fluid came away, and the patient felt much relieved. Mr. Coulson wished to make an incision into the perineum, but the patient would not consent. A gum elastic catheter was kept in the bladder; warm fomentations were applied to the part; an opiate was given immediately, and some aperient medicines ordered in the morning.

The patient had continued easy until the middle of the night, since which the pain had returned; the swelling had extended to the scrotum, and the prepuce was also œdematous. The necessity of giving relief by an operation was again strongly urged upon the patient and his friends, but the proposition was met by a decided negative. As the bowels had not been opened, a clyster was given; the pulse was small and quick, (120); and the countenance pale and anxious. The extravasation extended during the course of the day to the lower part of the abdomen, and on the penis there were vesicles filled with black fluid; the penis itself was semi-erect; the pulse 140, and very small; hiccup; now free from pain. In the evening he died.

The body was examined within 24 hours after death. The whole cellular tissue of the perineum, scrotum, penis, and lower part of the abdomen, were loaded with a dark-coloured fluid, having a strong urinary smell; the urethra was sound in every part except at the membranous portion, which was completely destroyed by ulceration, and the opening communicated with a cavity in the perineum of sufficient size to hold an egg; the prostate was enlarged, and the bladder inflamed.

Nov. 4th.—To-day, Mr. Coulson made the following clinical observations on the above case:—Gentlemen, this case of extravasation of urine with stricture, which occurred some time ago in this institution, shows the rapidity with which the mischief extends, and even death ensues, unless the most prompt and efficient measures be resorted to. The giving relief to the extravasated urine by free incision might have afforded the patient a chance of recovery, and ought in no case whatever to be neglected. Extravasation of urine may occur in consequence of pre-existing mischief in the urethra, (and is then frequently, though not always, preceded by retention,) or in consequence of violence, either external, as a blow, fall, &c., or internal, as the too forcible introduction of instruments. The symptoms, however, of the disease from either cause are pretty much alike: there is pain and a sense of scalding in the urethra, increased by any attempt at making water, swelling and tenderness of the perineum, which, in severe cases, generally extends to the scrotum, prepuce, and lower part of the abdomen, and the skin over the part where the urine has extravasated ultimately becomes gangrenous. When only a very small quantity of urine has escaped, a hard circumscribed swelling occurs, which terminates in an urinary abscess; the constitutional symptoms are very severe; fever of a typhoid character generally supervenes; and if the urine has not been evacuated by free incision into the part, the patient rapidly sinks: if, on the contrary, this plan of treatment has been adopted, the chances of recovery are much increased. The course which the urine takes is a point worthy of your attention, and depends on the attachment of the perineal fascia. The urine seldom makes its way down the inside of the thighs, or in the neighbourhood of the anus—a direction which, from the position of the body, you might naturally expect the urine to take. The urine, however, makes its way upwards, where the perineal fascia is loose, or has no fixed attachments, being there continuous with the fascia covering the anterior parietes of the abdomen. Laterally this fascia is connected to the rami of the ischia and pubes, and posteriorly it dips behind the transverse perinei muscles, and becomes connected with the anterior layer of the triangular, or, as it is sometimes called, Camper's, ligament. Thus you will see that, unless the fascia is destroyed by ulceration or external violence, the urine cannot escape in these two latter directions. Sometimes the urine makes its way into the cells of the corpus spongiosum, and then the penis attains a large size, and in general becomes gangrenous; the penis also in these cases is in a state of erection. Mr. Brodie, in his valuable lectures on diseases of the urinary organs, (*vid.* Medical Gazette, vol i page 7,) says, that " a black spot may be sometimes seen on the glans of the penis; it is a most fatal sign, for I never knew one to recover in whom it appeared: it indicates that the urine has been effused into the cells of the corpus spongiosum." The urethra also gives way sometimes more anteriorly about an inch or so from the external orifice; in this case the prepuce will be most œdematous. I will now show you a curious case of this kind, in a man fifty-three years of age, who is at present under my care, and whose urethra has given way in this part. You perceive that when he is making water the anterior extremity of the penis, particularly the under surface, swells to a great size; the circumference of this part is seven inches. When he has emptied his bladder, he then empties this pouch, or second bladder, if I may so term it, with his fingers; it gives him little or no inconvenience.

The treatment in cases of extravasation of urine consists, first, in introducing a catheter down to the stricture, or, if you can, into the bladder—then place your patient in the same position as if you are going to operate for stone, and make a free incision in the perineum down to the ruptured portion of the urethra. If you have succeeded previously in getting a catheter into the bladder, you have nothing to do, when you have completed the incision, but to let the instrument remain: if, on the other hand, you have only succeeded in getting an instrument down to the ruptured portion, you must then, taking the point of the instrument in the urethra for your guide, endeavour, from the external wound, to pass a gum catheter into the bladder, and keep it there two or three days: if you cannot get a catheter into the bladder from the external wound, you will not have much to apprehend, as the urine will make its escape, if there be a free external opening: also make free incisions wherever the urine is extravasated. If the urethra is ruptured just behind the scrotum, this part attains a great size; and in this case you must make a very deep incision, guided by the middle line of the scrotum down to the ruptured part. I attended a case, not very long ago, with Mr. Lawrence, where the urethra had given way in this situation; we were obliged to make a free division of the parts in the manner in which I have now mentioned; the patient, however, died. Locally, apply warm fomentations and poultices, to promote the coming away of the sloughs, and when they have come away, apply some mild ointment to the wounds; internally, you must give some saline medicines till the sloughs have come away, and opiates, if at bed-time, and afterwards support your patients' sttrength with tonics, wine, and strengthening diet.

On examination after death, we in general find ulceration of the urethra to a considerable extent, the cellular tissue of the perineum, scrotum, and lower part of the abdomen loaded with urine, or in a state of gangrene. If there has been stricture of long standing, the prostate, bladder, and sometimes the kidneys, will be found diseased. Extravasation of urine from the urethra, as I said before, is frequently preceded by retention, or some such marked symptoms of disease in the canal, as to lead the practitioner, at a very early stage of the complaint, to detect its nature, and to adopt at once the proper means of relief: this, however, does not invariably happen. A case of extravasation came under my care in this institution, in 1828, where there had been but little previous disease, and at the same time no such marked symptoms as to lead me to detect the complaint. The case had been entered under the physician, but I was requested to see the patient for a pain and a scalding in the perineum. I at first suspected that there might be extravasation, and I asked him to make water, which he did in my presence, in a very good stream: at the time of making water, however, the pain was increased; he assured me also that he had never any difficulty in voiding his urine. I then thought nothing more of extravasation, but treated the complaint with the usual antiphlogistic means, as leeches to the part, and warm fomentations. The pain was followed by redness of the lower part of the abdomen, but there never was much swelling. The patient, however, rapidly sunk, and the post mortem examination disclosed the real cause of the mischief.

There was a great deal of urine lodged in the cellular tissue of the perineum and lower part of the abdomen. There was no extensive ulceration of the urethra; the urine had escaped from three or four minute openings (of a size sufficient to admit a small silver probe) in the membranous portion of the urethra. This part of the urethra was also narrower than natural. I thought that it would be instructive to you to mention this case, for it made a deep impression on my own mind, the man's life having been lost from the complaint not being understood. I would advise you never to conceal an error in practice; if the only effect of concealment were to deprive our professional brethren of an ample source of instruction (for errors will occur to all), this of itself ought to form a sufficient inducement for us not to practise it. But this is not all—by keeping secret our errors in practice, we run the risk of bringing ourselves into a state to commit them without remorse, by deadening those moral feelings which the commission of errors in practice ought to excite in every honourable mind, and which, if properly cherished, form at the same time the strongest stimulus to avoid them.

THE COURIER *versus* THE MEDICAL GAZETTE.

The Courier has thought fit to allude to the observations contained in our last number on the opinions of Dr. James Johnson: the protocols from Suffolk-Street have been interrupted, but another writer has adopted the same sentiments and the same language; and the Editorial writers have again put forth their own most dangerous doctrines on the subject of Cholera, which they assert not to be contagious, "as the Board of Health pretend." It is of importance to the public that all points of this nature should be placed in their true light, that the extent to which those, who assume the guidance of public opinion, are entitled to confidence, may be at once determined, and on this account we feel called upon to make the observations which follow.

It is a mere mockery to urge persons to the observance of the quarantine, and in the same breath to ridicule the authority by whose advice such precaution is adopted; and to concur in the sentiments, and praise to the very echo, the reasoning of one who holds (or at least held) the disease to be non-contagious, and the danger to be imaginary: this, we say, is contradictory and inconsistent, yet it falls short of the plenitude of absurdity which some recent writers have displayed.

Thus the Editor of the Courier, about a fortnight ago, in commenting on certain letters published in his paper, says, "We insert to-day a letter with which we have been favoured from Dr. James Johnson, one of the King's physicians, on the subject of cholera. It is with great satisfaction that we lay before our readers the opinions of a gentleman so competent to the task which he has undertaken." What, then, were the opinions to which the Editor thus confidently directed attention? Why these—that the precautions against cholera were perfectly useless. The following are the doctor's words. After ridiculing the idea of keeping out the disease by measures of this nature, and speaking in irony of having ships ranged bow and stern along the coast, he adds, "I will hold it (*i. e.* the quarantine) perfectly guiltless of neglect of duty, should the pestilence pass this boundary by day or by night, and effect a landing on our shores." Again, said Dr. Johnson, in a former letter inserted in the same paper, "I have no hesitation in reiterating my opinion, that if cholera comes to these shores, it will come shorn of its fatality, and coercible to a degree that will make the terrorists ashamed of their ominous predictions and visionary speculations." Here the *Courier* also takes up the subject, and congratulates their readers on the opportunity "of removing the alarm, caused by the notices inserted in the *Gazette*, at the instance of the Board of Health;" and he adds, "It is satisfactory *to see that folly completely exposed by medical authority of real weight.*" Again, says he, "every thing in reason and in fact seems to show, that it is not contagious."—(*Courier, Oct.* 29.) Very well: here is the opinion of the writer distinctly enough expressed, whatever may be its value. What, then, will our readers think of the reasoning powers of this gentleman when we inform them that within a week afterwards he denounced in the strongest terms those who had been weak or culpable enough to act upon the doctrines he had taken so much pains to inculcate. He alludes to cholera as having been imported into Sunderland from

Hamburgh, and then, evidently not aware that the admission of this fact was fatal to the doctrine of non-contagion, he adds, "The crews of several vessels from that port (Hamburgh) are stated to have been suffered to land. * * * If it be true that such negligence on the part of the authorities really did take place, we know of no terms of reproach and censure sufficiently strong to bestow upon them. * * * Is it conceivable that they could have allowed the crews of vessels from Hamburgh to have had communication with the shore merely because they gave no external proof of sickness?"—*(Ibid, Nov.* 4). In taking leave of this sagacious personage, we are fain to apply to him his own words, because "we know no terms of reproach and censure sufficiently strong to bestow upon those" who presume to utter *ex cathedra* opinions on a subject of the very rudiments of which they thus shew themselves to be totally ignorant, and who use whatever influence they possess over the public in the inculcation of a doctrine which, according to their own shewing, is not more erroneous than it is pernicious.

They (says the Times of Thursday, most truly) have much to answer for who would throw a whole nation off its guard, and from stubborn adherence to a gratuitous dogma, persuade men from taking simple and easy (provided there be time) means of repelling or mitigating a pestilence to which so many human lives have already fallen victims.

PRECAUTIONS AGAINST CHOLERA.

Cholera has been imported into England by a vessel which left Hamburgh after the disease had broken out there, and which was suffered to come up the river at Sunderland. The first person affected was a "keelman" named Stroud, and the next in succession to whom it spread were members of his family. It is remarkable that Sunderland has for some time been looked upon with suspicion, in consequence of some doubts as to the strictness with which the quarantine was there carried into effect. The accounts received in town have hitherto been of the most unsatisfactory nature, being lamentably deficient in particulars. After this, however, each practitioner is to send a daily report to Dr. Daun. Our accounts on Thursday gave fifteen new cases, and ten deaths; all among the lower orders. The time for deliberation, however, is gone by, and that which imperatively demands active exertion, has arrived. The possibility of its importation, and the fact of its being infectious, are no longer matters of doubt with reasonable men; and it behoves the authorities of every town and village in the empire to make preparations for the unwelcome visitor. London, we understand, is to be divided into districts; houses are to be prepared for the reception of the sick; and medical men are to be appointed, who will devote their whole time to the fulfilment of the practical measures deemed requisite. These, of course, will not be composed of the present Board of Health, who have been a deliberative rather than an executive body; and we repeat, that now is the time for action. Whether the present Board will be continued we know not, but the new parts of the machinery will, we believe, be immediately put in operation. We understand that army surgeons, and others over whom the government have power, are every where to be put in requisition; and we would advise farther—that, inasmuch as the disease may be communicated by one who himself has it not—or in other words, as medical men may carry it to their patients—so, as a general rule, those who are in frequent intercourse with persons labouring under cholera, ought not to visit others, or go into society—at all events, without changing their clothes and having recourse to copious ablution. We are convinced that, in other countries, the neglect of this precaution has contributed to the extension of the disease. We shall return to this subject next week: meantime we take leave to ask, what is the use of stopping the vessels at the Nore, while the mail is allowed to come up in two days?

The new Board consists of Colonel Rowan, of the police department, Sir W. Pym, Hon. Edward Stuart, Drs. Russel and Barry; the latter gentleman having just returned from the Continent.

A FALSE ALARM.

A PERSON, walking in Regent-Street, had an epileptic attack, on Wednesday: he was taken into a shop close by, at the instance of the Solicitor-General, who was passing at the moment, and Mr. Arnott, of New Burlington-Street, was sent for; by whom the nature of the attack was at once pointed out. Nevertheless, the circumstance got wind, and was magnified into a case of cholera; the government was informed of it, the Board of Health consulted, and the practitioner above-mentioned examined, all within an incredibly short period. This shews, at least, how much all are now on the *qui vive* on the subject.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Nov. 5th.

The Cholera.

IN our last number we gave some account of the proceedings of the Saturday evening previous, considering them as preliminary to a grand discussion which was to take place here on the Saturday evening ensuing; but in this we have been disappointed, and we regret to say that we feel it our duty to give *no* report this week. It would be far better, we should think, not to raise public expectation, nor appoint a night for discussing the subject of cholera at all, than for gentlemen to stand up unprepared with the grounds of their opinions, making vague assertions, and wasting the time of the meeting in desultory, inconclusive, and unprofitable chat. We except, of course, two or three of the members, who acquitted themselves on this occasion with their usual ability; but the debate, on the whole—if debate it can be called—does not deserve to occupy any portion of our columns.

MEDICO-CHIRURGICAL SOCIETY.

THIS society commenced its meetings for the season on Tuesday last. Mr. Lawrence, President, in the Chair.

Want of space prevents us from noticing the proceedings this week. The prohibition of reporting has been removed.

NOTE FROM MR. COULSON TO MR. S. COOPER.

To the Editor of the London Medical Gazette.

SIR,

You will oblige me by inserting in your next number the following note to Mr. S. Cooper.

I am, Sir,
Your obedient servant,
WM. COULSON.

Dear Sir,—I take the earliest opportunity of acknowledging that I committed an error in my clinical lecture in stating that there were no observations in your Surgical Dictionary on the treatment of patients previous to the performance of the operation of lithotomy. The passage to which you have called my attention shews that my observations were too unqualified; but I still think that less of your valuable article on lithotomy is devoted to the particular subject I alluded to than its importance demands. Permit me to add, that nothing was farther from my intention than to make any observation calculated to convey an impression that I undervalue a work which I have always considered of high merit and great professional utility.—I remain, dear Sir,

Yours, very faithfully,
WILLIAM COULSON.

Charter-house Square,
Nov. 7, 1831.

S. Cooper, Esq.

NOTICES.

We cannot attend to the *reclamation* of a dissatisfied author: as he has not liked our first, perhaps he would be still less obliged to us for a second notice.

Mr. Lee will find that what he requests was done in No. 2.

In No. 3, we gave eight additional pages without any extra charge; but as we are this week compelled to add an entire sheet, to meet the great press of matter, the price is necessarily raised *on this particular occasion.*

W. WILSON, Printer, 57, Skinner-Street, London.

Spasmodic Cholera.

THIS much dreaded disease has commenced its ravages on our shores, and nothing is more certain, than that the Medical Profession are not yet prepared with any consistent plan of preventive or remedial treatment, for a complaint, which, it is now admitted, " has successfully baffled the skill of the faculty throughout the continent." In this state of fearful uncertainty, and whilst every drug having the slightest claim as an anti-cholera remedy, has risen in estimation, the attention of the profession is respectfully invited to an invistigation of the merits of the

BARBADOS NAPHTHA.

This singular natural product comes forward with no doubtful pretensions, it is *actually* in high repute and constant employment in the West Indies, in the Cholera and Spasmodic Tetanus, so common in those latitudes; and is popularly held there, to be much more efficacious than the Cajeput Oil in the above cases. It is used externally as an embrocation, and internally in doses of half an ounce, or even more, mixed with a little rum, often repeated; its properties being those of a mild and very diffusible stimulant, speedily affecting all the secretions of the body. The powers of Barbados Naphtha, not only in Spasmodic Tetanus, but in Chronic Coughs, and Leprous and Scorbutic cases, have been attested by the Medical Staff at Barbados, and it has lately been determined by the Legislature to erect an hospital for the reception of patients from the adjoining islands. There seems every reason to believe that the present impending malady, assuming an epidemic form, is of the same nature as the West Indian Cholera, and under these circumstances, the sole contractor for the Barbados Naphtha, feels himself called upon to apprize the public of its more than equal claims to a fair trial; leaving it to the profession finally to decide upon its merits. The scientific Dr. Wilkinson of Bath, in an appendix to a pamphlet, published by Ridgway, entitled, " An Enquiry into the Chemical Properties and Medical Virtues of the Mineral Naphtha of Barbados," has already stated his opinion, that, in Combination with Ammonia and Opium, it will eventually be found the best remedy in the Cholera. Happily for us, the experience of the faculty has hitherto been too limited to warrant our placing faith in any particular remedy; but the Barbados Naphtha, it must be bserved, is not a quack medicine of empirical character and occult pretensions; but a simple, natural product, which has been excluded from this country for the last forty years, by an extravagantly high duty. It was this circumstance that led to the substitution of a spurious article, *manufactured* at Wolverhampton and Colebrookdale, and falsely called *Barbadoes Tar,* which has no medical power whatever, and has therefore been justly discarded by the enlightened practitioner. For the future protection of the public, (Government having reduced the import duty) the genuine Barbados Naphtha is now, on its re-introduction, consigned by the owner of the estate to one individual alone, who will only dispose of it in its pure state, as collected from the springs. It is sold in pint and half-pint bottles, for 4*s*. 6*d*. and 2*s*. 6*d*. each, with the signature of

Charles Clark

on the label and seal, at 24, STAMFORD STREET, LONDON; also by J. Bell and Co. Oxford-street; S. F. Gray, 97, New Bond-street; E. Smith and Co., Haymarket; Waylat and Co. Finsbury-pavement; Norton and Lowe, Great Surry-street; and at APOTHECARIES HALL, London: Campbell and Co. Knightsbridge; Scott and Tylee, Bath; Cope, Birmingham; Harris, ditto; and J. Sims, Norwich.

The Barbados Naphtha has been analyzed by Mr. Faraday, and by Dr. Wilkinson, and samples are deposited with the Lecturers on Materia Medica in the Metropolis. It is found to consist of almost a simple compound of Carbon and Hydrogen, decomposing at a temperature as low as that of the animal body; and its singular diffusible and penetrating qualities have been accounted for on a reasonable supposition, that the Hydrogen acts as a conducting medium to the Carbon, and thus, by means of the absorbent vessels, conveys the Antiseptic and strengthening powers of the latter throughout the system.

NOTE.—Since its re-introduction, the Barbados Naphtha has been proved to be a most efficacious remedy in Chronic Coughs of long standing and obstinate character; and externally applied as a dressing in Malignant Ulcers; and also in all Cutaneous diseases.

THE
LONDON MEDICAL GAZETTE,
BEING A
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OF
Medicine and the Collateral Sciences.

SATURDAY, NOVEMBER 19, 1831.

PUBLIC LIBRARY 1859

LECTURES ON THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

By Dr. Elliotson.

Lecture VII.

Nosological Arrangements, their inutility—Inflammation, sometimes a salutary process, frequently a dangerous disease—The Symptoms—Redness—Heat—Swelling—Pain.

I have now, gentlemen, made the chief prefatory remarks which I deemed necessary before entering upon the consideration of particular diseases,—upon *special pathology.* I have confined myself to an exceedingly superficial view of the subject of *general pathology*, and to the explanation of a few of those terms which I shall most frequently have occasion to employ.

More minute remarks I shall reserve for various parts of the course: for example, when I come to speak of the causes of inflammation, I shall particularly dwell upon the *modus operandi* of various changes and degrees of temperature in producing disease; and when I come to speak of diseases supposed to be contagious, I shall enter upon the subject of contagion. I think it much better to treat these matters in this way, than to treat them in the abstract before entering upon particular diseases; by this means they will not only be better understood, but we shall be relieved from the tediousness of dwelling long at a time upon general subjects.

Now in the first instance I mentioned that one part of general nosology, or one part of general pathology, and indeed one branch of that part of pathology which is called nosology, is the *arrangement* of diseases—one branch, if I may so speak, of general nosology is, *methodical nosology.* You will frequently hear the arrangement of diseases spoken of simply as nosology, but that is incorrect; the arrangement of diseases is not simply nosology, but *methodical* nosology—*nosologia methodica.*

It was long before an attempt to arrange diseases was made. I believe Felix Platerus was the first who struck out the idea of arranging diseases methodically; he was followed by Baghvi, an Italian physician, and Sydenham. However, it was a French physician who first actually arranged them systematically—Sauvages, who published his work in 1762. It was in that year that the first methodical nosology was presented to the world. After him came the celebrated Linnæus, who arranged every object in nature, and published his nosology at Sweden in 1763. Another physician, Vogel, published one in 1764. No sooner was the thought put in practice than every body went to work. The arrangement of Cullen was published in 1769; another was published by Dr. Macbride, and one by Sogar, at Vienna, in 1771, so short a time elapsed; then one by an author named Vitenus; one by Dr. Thomas Young, the celebrated English philosopher; and one still more recently by Dr. Good. There have been others, but these are the chief.

Now, however useful it may be to arrange diseases slightly, for the purpose of memory, and for the purpose of general views, I think it must be confessed that all these various *methodical nosologies* only perplex and encumber the mind. I studied the arrangement of Dr. Cullen formerly very minutely, and had great part of him at my fingers' ends, but I confess that my knowledge of it now is out of a very superficial kind, and that it was never of any use to me. The arrangements of Drs. Young and Good appear to me just as useless; and I would not, if I advise you honestly—and I hope I always shall do so—I would not advise you to plague yourselves about nosological arrangements. My own experience tells me that it is a much greater plague to recollect the arrangement and all the hard words than to recollect the things for which the arrangement is made. I never found it of the slightest use, any more than the barbarous jargon of the *Propria quæ maribus* and *Quæ genus* of the Latin grammar.

On the inconsistencies and defects of these various arrangements, I will not dwell; for if you turn to any methodical nosology you will find them in sufficient abundance. You find exceptions made to the plan which the author has laid down in his arrangement, and you find the most gross absurdities. If you turn to Cullen's arrangement, for example, which is one of the best, you will find a large number of diseases daily occurring that have no place in his nosology. Such was his arrangement, that there was not only no place for many most important diseases, but he arranges itch and fracture together. In speaking thus of Dr. Cullen, allow me to say, that although I think little of his arrangement, there can be no doubt that it was far better than that of any of his predecessors; and I must be allowed to pay a tribute of respect to his memory, as one of the most sagacious and one of the best of men who ever joined himself to our profession. I should be very sorry to endeavour to captivate your minds by attempting to lower any one in your estimation; nothing can be more contemptible; and as it regards Dr. Cullen, nothing could be more unjust. You find in his work the utmost candour, and the soundest information; and although he indulged in hypotheses to which some Scotch physicians have been prone, yet independently of that his descriptions are elegant and simple, and you will find his definitions admirable. It is to be remembered that his work on the Practice of Medicine was not considered by him a perfect work, and he modestly called it "First Lines"—a mere outline; but such as they are you will find them, according to the information of that day, exceedingly useful; and it is on account of the excellency of the original work that I should perhaps prefer recommending to your study an edition of Cullen, with notes, to the work of any other individual. I think the advantage of having Cullen's work, with notes, although it should not contain more information, is, that you have the work itself of Dr. Cullen, which was not surpassed in his day, and has not since been surpassed, as far as respects the knowledge of that period. I think you cannot do better than study the edition by Dr. Gregory. There are many other excellent works, Dr. Gregory's, Dr. Macintosh's, Dr. Good's, any one of which is worth possessing, and would form a good basis for study, but I think altogether you will find Cullen's, with notes, the most profitable to you as *students*.

The most natural mode, in my opinion, in which we can attempt to arrange diseases in our mind, that which serves best for the purposes of recollection—for an arrangement is certainly useful—although I am not an advocate for a methodical one, so called—that which we most naturally fall into, is a two-fold arrangement; first, as to the nature of affections in general, whether they are inflammatory, structural, functional, mechanical, or parasitical; for whenever we see a case we immediately consider what is the kind of affection; and then, secondly, as to which is the part in which the affection occurs. This is the arrangement which I shall follow in these lectures. I shall first consider general diseases, such as affect every or most parts of the body—inflammation, scrofula, and various other organic diseases; and afterwards, having considered all the affections which may attack any part of the body, I shall proceed to consider those affections and all others, functional, mechanical, and parasitical, as they attack the body from the head downwards—*a capite ad calcem*. I think we all make two inquiries in considering any case; the one is the nature of the disease, and the other is the situation of it. This is the utmost assistance, I think, that the memory can have from arrangement, and this is the plan which is continually followed by *practical* writers. They write on particular diseases, whatever they may be—inflammation, cancer, &c., and of the whole diseases of particular organs, and sometimes of particular regions. You have one writer publishing a work especially on diseases of the urinary organs; another, entirely on diseases of the nervous system; another, entirely on diseases of the head; another, on diseases of the chest; and this course we naturally fall into. But, independently of that, we must make observations upon the general affections to which all parts of the body are liable. I am quite satisfied that this sort of arrangement—by which we consider general affections which may attack any one part of the body, and then consider the affection as seated in this or that part, will afford us all the assistance that arrangement can; and I hope you will never plague yourselves farther with methodical nosology.

Inflammation.

The first disease of which I will speak is the most general of all affections, one which attacks almost every part of the body, at least all vascular parts, and which is the most frequent of all diseases—*Inflammation.*

In the first place, this is the most general of all diseases; it will attack any part of the body that is vascular: in the second place, it is one of the most frequent affections. It is a disease which scarcely any person escapes; it is a disease which is seen every day. It occurs simply by itself, and it is the concomitant of a variety of other diseases. Many diseases are always inflammatory, though they are not simple inflammation—though there is something more than inflammation, yet inflammation is united with them; and many other diseases are frequently inflammatory, but not always. Besides, inflammation is continually occurring as a means of benefit to the body; it is not only a morbid process, but a process frequently set up by nature for the purpose of restoration, relief, and prevention. It is a disease, too, which proceeds from a vast number of causes—causes to the action of which we are continually exposed, whether we will or not.

Again, it is one of the most dangerous diseases when it affects certain parts, and any part when it proceeds to a certain extent; and it is always liable to be carried to this dangerous point. However slight an inflammation may be, it is always liable to become aggravated to a dangerous point, and it continually does actually attack the most important organs, and hence we may be justified in saying, that the consideration of inflammation is more important than that of any other affection of the body.

When it occurs not as a disease, but for the purpose of restoration—for the purpose of health, if I may so speak—the circulation of the part becomes increased, the quantity of blood becomes augmented, and more nourishment is afforded than before. If a part be destroyed, or divided, a degree of inflammation is set up, and fresh matters are deposited, so that the part is more or less renovated:—when a bone is fractured, a certain degree of inflammation occurs for the purpose of pouring forth a fluid, which afterwards becomes solid, and produces union.

In the next place, inflammation is frequently employed by nature to prevent the escape of matters into parts where violent inflammation would be produced; and is set up by nature to prevent its own occurrence in another spot. For instance, when an ulcer takes place in the stomach, or in the intestines, if it proceed unchecked, the contents of the stomach or the intestines are poured forth into the cavity of the peritoneum, and a violent inflammation is set up—an inflammation, in the greater number of cases, fatal. Nature, however, very frequently sets up a little inflammation outside the organ, exactly at the part corresponding to the ulceration within, by which means it is glued to the surrounding parts, fibrin is poured forth, the fibrin becomes vascular, the parts become still more firmly glued, and then the stomach or the intestines cannot ulcerate through into the peritoneum. Thus, a slight inflammation is set up in one spot to prevent an inflammation which would be fatal to the patient. You will see the stomach sometimes attached to the peritoneum, and sometimes to the liver.

Then, again, nature employs inflammation frequently to give an exit to something which is injurious to the body. When a biliary calculus is impacted in the ducts, and is too large to escape, it sometimes happens that these ducts become attached by inflammation to the intestines. The inflammation, after it has excited adhesion, proceeds, perhaps, to ulceration, and when the latter occurrence takes place, the calculus escapes through the opening into the intestines. The inflammation, in the first place, is set up to prevent the communication which would afterwards take place into the peritoneum, so that the bile and the calculus shall not escape into it; and when all these parts are firmly glued, and the danger prevented, then, at the particular spot, inflammation goes on to ulceration, ulceration takes place into the intestines, and in that way an exit is given to the foreign body.

When a part of the body mortifies, whether through inflammation or not—yet when a part has become dead, and useless to the body, and injurious to it from its presence—then inflammation is employed for the purpose of separating it. At the boundary of the living part, inflammation is set up; a red line appears at the extremity of the healthy part, where the dead part begins, and that red line is a line of inflammation, which inflammation proceeds to ulceration. A furrow of ulceration takes place, and goes down till the part is ulcerated through, and the dead part falls off, having been separated by the ulceration of the living disease.

You see, therefore, that inflammation, although a most fatal disease in many cases—although conjoined with the most frequent and violent diseases—is sometimes a most salutary process;—sometimes employed by nature to produce an union of parts; sometimes to prevent the great mischief which arises from an effusion of fluids into parts where they ought not to go; and sometimes for the purpose of giving exit to foreign bodies, or separating dead parts from the living.

The general definition of inflammation is, "redness, swelling, heat, and pain;" that is to say, *morbid* heat, *morbid* redness: swelling and pain require no definition, but redness is common to some parts, and therefore we must understand morbid redness, and with respect to heat, morbid temperature. These are the four chief symptoms of inflammation—those which make the definition in general—in the words of Celsus, "*rubor et tumor, cum calore et dolore.*"

Redness.—These are the general constituents of inflammation, and if they do not all exist in all cases, still usually the greater number are present. Sometimes one is absent, sometimes another, but the redness is never absent; the redness is certainly indispensable to our idea of inflammation, and it will sometimes exist without swelling, without heat, and without pain; without any of these individually being induced, without any two of them; and now and then the redness will exist alone without any of the three. Whether the state should then be called inflammation or not, may give rise to a difference of opinion. But frequently you have inflammation without any perceptible swelling; you must conceive the part to be a little increased in size, because there is more blood than natural, but without a sufficient enlargement to justify you in using the word swelling—without much increase of heat, and sometimes certainly without pain; and it is to be remembered that both the increase of temperature and redness, by which we understand a morbid degree of them, may arise from other circumstances than inflammation, as may also the swelling and pain.

Still, although redness is indispensable to inflammation, it is not every redness that we have a right to call inflammation; a certain degree of intensity is necessary to the idea of inflammation. How much cannot be specified; but, like sensations in general, it must be experienced to be learned. The requisite intensity will vary in different parts, according to their natural colour. A blush, or momentary rosiness, is not considered an inflammation, and every part is liable to this momentarily in health. As regards the cheeks, a degree of redness there would not be considered inflammation, which we should be fully justified in calling so if it affected other parts of the body. The most intense redness, that lasts only for a moment, we should not call inflammation. It must be in the cheeks—supposing the person has a natural colour—a very deep red, indeed, and more or less permanent, at least not a transient redness, though intense, for us to say that the cheeks are inflamed.

Again, the same degree of redness, the same intensity of it, would not be considered inflammation if alone, which would be considered so if there were also pain and morbid sensibility. If we saw in the cheeks a great redness, we should not hesitate to call it inflammation if we found the part were morbidly sensible, or if the patient complained of it smarting, and being painful independently of pressure. Although the redness might be less intense, and might not continue so long as alone to justify us in calling it inflammation, yet provided it be in a part which is not naturally red, like the cheeks—not liable like them to occasional flushing and rosiness, a much less degree of it, and a much shorter continuance, might give us the idea of inflammation. You see how many things are to be taken into the account. A knowledge of these things cannot be gained at once, and many circumstances must be called in to aid us in our judgment. When persons are speaking of redness in connexion with inflammation, instead of the mere expression *redness*, we ought to say *morbid*, or, perhaps, *preternatural* redness; because what is morbid redness in one part is not morbid redness in another—redness preternatural to the part affected.

Now redness is not only of various degrees of intensity in inflammation, but of various hues, being sometimes of a bright scarlet, sometimes rather purple, and of all intermediate shades. Besides varieties of hues in redness, the redness may be distinctly circumscribed in patches, or it may be insensibly lost in the natural colour of the surrounding parts, so that we cannot exactly say where it ends.

Heat.

We will now proceed to the consideration of the other symptoms, and first of *heat*. It is right to say, that for the same reason which makes one adopt the epithet preternatural or morbid, in regard to redness, we should likewise affix the same epithet to heat, because the body, in this country at least, and in most others, is hotter than the surrounding temperature, and therefore it is always hot. A preternatural heat, if not very great, is not of necessity inflammatory; and if it be very great and yet last but for a short time, even then it is not inflammatory. A certain intensity, and a certain duration, are necessary to constitute inflammation in the case of heat, as well as in the case of redness. The greatest intensity of heat, if transient, would not be considered inflammation. The burning heat upon the cheeks, the palms of the hands, and the soles of the feet, in hectic, is never considered inflammation, even though it is combined with preternatural redness. The most intense heat, and also the most continued, is not of necessity inflammation, unless it be united with a pretty permanent preternatural redness. The whole body may be of a burning heat for weeks, as hot as an inflamed part, and yet the skin is not said to be inflamed. The example of hectic shews that even a combination of heat, with intense redness alone, is not sufficient to give the idea of inflammation; for in these intense spots in hectic there is no pain, nor are the parts most affected tender on pressure. Besides that, I need not say that morbid temperature only comes on for a few hours in the day, and is a mere concomitant in hectic from another disease. In addition to this there is no swelling in this affection, and therefore we have still more reason for saying that it is not at all inflammatory.

In regard to heat, the temperature of the affected part appears much higher to the patient than it really is, which arises from the parts being in a state of morbid sensibility. In palsy it is not very uncommon, when there is no increase of temperature at all—no inflammation—for the patient to feel every thing hot, so that if he sit on any thing without his clothes, it strikes hot to him, or if he put his hand on any substance, whatever it may be, it appears as if it were heated. This is a peculiar state of the system, and not inflammation. There is in inflammation a general morbid sensibility of the part, and therefore the degree of heat which actually occurs produces greater effects than it would in any other part of the body. But although, from the morbid sensibility of the part, the increase of heat appears greater to the patient than it really is, there can be no doubt that in inflammation the temperature is raised. When the nostrils are inflamed, or the bronchial membrane, or the throat, the air that we expire is so heated in passing over the parts, that, as it comes out at the nostrils and lips, it is felt to be burning. If you apply your hand, which is in a state of perfect health, to the inflamed part, you feel that it is hotter than usual; and if you make use of a thermometer you ascertain the point to a nicety—you continually find the parts inflamed hotter by many degrees than other parts of the body; it is not uncommon for them to be 107°. John Hunter made experiments to shew that the temperature of inflamed parts was but little augmented—at least not higher, he said, nor even so high, as the circulation at the heart and the chest; that if the parts had naturally a lower temperature than the centre of circulation—as for instance the hands and the feet—and they became inflamed, their temperature never rose so high as the centre of circulation. The general temperature is said to be 98°, but it varies in different parts; and the farther you proceed from the heart the lower it becomes, because there is less circulation there, and the parts are more exposed to the temperature of the surrounding atmosphere. There is a great mass of substance at the trunk—there is less on the arms—still less at the extremities of the fingers, at the nose and ears, and there is more exposure.

Now John Hunter produced inflammation in the thorax, in the peritoneum, in the rectum, and in the vagina, very cruelly, and he found the heat but one degree and a half higher than before the inflammation, and he frequently found it exactly the same. He says that the temperature of the vagina, at the utmost, increased to 101½°, but he found the temperature of the tunica vaginalis only 92° in health, whereas, when inflammation was produced in it, it rose to 98¾°, 98° being about the natural temperature of the body. It is to be remembered, however, that the scrotum is a part very much exposed to the surrounding air—a depending part, and therefore as much exposed to the surrounding atmosphere as the hands, feet, ears, or nose; and he infers that the heat is never raised above the natural standard at the centre of circulation. If the natural temperature of the tunica vaginalis be 92°, certainly the increase to 98¾° is very considerable. He says that he once found the abdominal fluid to be as high as 104°, and he says that if inflammation attack any part with a temperature of 98°, the heat may proceed beyond that of a healthy person. Now, however correct these experiments may be, you have only in the case of erysipelas to take a thermometer, lay it upon the inflamed part and cover it up, and you will then see the temperature, although it be erysipelas of the leg, often raised to 104, 105, 106, or 107 degrees.

That the temperature of the body may be increased very considerably under various circumstances, is shewn by the following facts: for example, Sir Everard Home observed the oviducts of a frog when about to spawn, and when a great local activity of circulation was going on, to be two degrees higher than the temperature of the heart. Now there was no inflammation here, but merely a great activity of circulation approaching to inflammation, such as occurs in the generative process, and the result was as I have stated; and if a mere natural process short of inflammation will raise the heat above that of the centre of circulation, you may well suppose what inflammation will do. Even in vegetables, when a certain process is going on with great activity, a very considerable increase of temperature takes place. When the arum cordifolium and the arum maculatum are about to burst, the temperature, by placing a considerable number around a thermometer, has been such as to raise it very considerably. Twelve were placed around a thermometer, and so high a degree of heat was developed in the physiological process of bursting, that the instrument was raised from a temperature of 70° to 143°. Even in fever, by placing the thermometer under the tongue, you find the temperature raised many degrees; and the same occurrence takes place in acute rheumatism. In one case of phlegmasia dolens, on placing the thermometer on the thigh, and covering it with the hand or bed-clothes, I found it rise to the same degree of heat that is frequently observed in fever and in acute rheumatism—namely, to 107 degrees. Indeed one gentleman says that, during labour, he has observed the temperature of the uterus, having introduced a thermometer into the vagina, to be 120 degrees. In the violence of the spasm in tetanus, the temperature is frequently as high as 107 degrees. There can be no doubt, therefore, that inflammation is frequently attended by a great increase of temperature: I have seen it myself in various inflammations of the surface. This, however, is not peculiar to inflammation; for, as I have stated, it occurs in fever and rheumatism, where the thermometer has not been applied to the rheumatic parts, but put under the tongue and in various parts of the body; and it has been observed likewise in many affections of the system.

But as an increased heat may exist without any inflammation, so inflammation—that is to say, redness and swelling, of an undoubted inflammatory character, as proved by the terminations— may exist without any preternatural heat. You will sometimes have inflammation, or what, certainly, I think we are justified in calling so, —that is to say, preternatural redness and swelling, going on to suppuration—without any increase of heat; and this state of the parts has been called *passive* inflammation. Some object to the term, and say it does not deserve the name of inflammation—it is mere congestion of blood; but it will go to suppuration, and therefore I think we are justified in calling it inflammation, although there is no increase of heat. It is right, however, to tell you that some actually object to calling such a state inflammation, and they limit the term "inflammation" to an active state in which there is pain and all those symptoms which I mentioned;—and, to avoid any quibbling, Andral calls any state in which there is a morbid collection of blood, *hyperemy* —excessive quantity of blood in a part. and when it is actively inflamed, *active hyperemy;* and when this passive state occurs, he terms it *passive hyperemy.* Thus he gets rid of the difficulty altogether, by no longer using the word inflammation, but using a word signifying congestion of blood, and applying one epithet or another to it.

Swelling.

If we consider the third of these symptoms—swelling—we shall find that it also may exist without any inflammation. Swelling, although frequently a symptom of inflammation (more frequently than not), may of course exist, just like an increase of temperature—just like an increase of redness—without inflammation. Any thing, almost, will produce a swelling. The mere displacement of a part, the dislocation of a bone, hernia, the mere effusion of hæmorrhage, or a mere collection of water or any morbid growths, whatever their character, or the presence of air within the body, will all produce swelling;—swelling may be produced by many causes besides inflammation. For a swelling to be considered inflammatory, it must be united with pain or a preternatural redness; and even then it may be chiefly dependent on other causes. For instance, in dislocation you may have inflammation of the ligaments and integuments, producing pain; but the swelling arises chiefly from the displaced bone. Again, it is to be remembered that inflammation may sometimes exist without swelling, as is occasionally noticed in ophthalmia. In ophthalmia, the turgescence often gives rise to no perceptible swelling; sometimes there is a little swelling, but frequently this affection, and many superficial inflammations of the skin, are not attended by any swelling. Sometimes you will actually see a part inflamed in which there is scarcely any pain and no increase of temperture, but excessive redness and excessive swelling; the congestion is extreme, and gangrene, in such a case, is very likely to take place.

You therefore see that not only the symptoms will arise from various other causes than inflammation, but they are out of proportion frequently, when they do occur, to each other. Sometimes a little inflammation is attended by very violent pain, and sometimes a little inflammation, a little increase of heat, is attended by the most violent swelling. There is no necessary proportion between any of these symptoms.

Pain.

Then if you take the last of these marks of inflammation—pain, we shall find that it will arise from many other causes than inflammation. Pain will arise from a mere spasm; nothing is more painful, for example, than cramp in the legs. Pain arises frequently from scirrhus, or cancer: some call this "chronic inflammation," but certainly there is something more than that. Pain, too, is frequently neuralgic; a violent affection of the nerves, in which no inflammation can be detected during life, and no effect of inflammation discovered after death. Pain continually arises from inflammation, but it continually arises also from the other sources just pointed out; and sometimes it is absent in inflammation. You will see a part violently red, violently swollen, without any pain at all; and nothing sometimes is worse than this, for the part may fall into gangrene.

Just as there may be varieties of redness and various hues of redness, so there may be varieties of pain. The pain is sometimes tensive: if the part be one that will not stretch, then the pain is of a tense character, and the part feels as if it would burst. If it be inflammation inclined to suppurate, the pain is of a throbbing character. If it be inflammation of the skin, the pain is frequently of a smarting or of a tingling character. If it be inflammation of a mucous membrane, you frequently have it characterized by a burning and pricking pain. If it be inflammation of a serous membrane, a stabbing sensation will often be experienced, as though a knife were being plunged into the part. There are, therefore, great varieties of pain, just as there are a variety of hues of redness.

PESTILENTIAL CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

A PAPER having appeared in your last number from Dr. Stanley, on the treatment of cholera, I wish to state in the forthcoming number of your excellent work, that the remedies he mentions were previously recommended by me, in an article on the nature and treatment of this disease, published in the October number of the Foreign Quarterly Review. Indeed, an account of the means both of prevention and of treatment there detailed was sent, several months since, to the Russian Ambassador, with the view of having it transmitted to St. Petersburgh; but I doubt if my intention was complied with, for my communication was returned shortly after; but whether a copy had been taken of it or not, I cannot say.

Without noticing the facts from which I have deduced the inference respecting the infectious property of this pestilence, I will merely state the conclusions at which I have arrived regarding its nature generally. Your able and conclusive leading article on the infectious nature of the malady, must, together with what has been adduced elsewhere, particularly in the Review alluded to above, have convinced those who are open to conviction of the truth of our doctrine. More is not required on this topic.

Its Nature.

1. The pestilential cholera is propagated by an animal miasm or effluvium, of a peculiar kind, emanating from the bodies of the affected; and this effluvium being inhaled with the air into the lungs, paralyses these organs, and acts as a poison on the class of nerves which supplies the respiratory, the assimilating, the circulating, and the secreting viscera, vitiating also the whole mass of blood, and thereby occasioning a specific disease, which, in its turn, gives rise to an effluvium similar to that in which itself originated, which also, in like manner, perpetuates its kind, under the favourable circumstances of predisposition, aerial vicissitudes, temperature, &c.; and thus a specific state of disease is propagated far and wide, as long as predisposing, concurrent, and determining causes favour its perpetuation.

2. The morbid impression made by this effluvium, or poison, on the nerves of organic life, and most probably the effect of its introduction also into the current of the circulation, are of a sedative kind, rapidly destroying the vital energy of the former, and vitiating the latter, and thereby giving rise to the characteristic phenomena of the malady.

3. The impression of this effluvium on the organic class of nerves, and the vitiated state of the blood, may be viewed as the proximate cause, not only of the disturbance evinced by the respiratory, the secreting, the assimilating, and the circulating functions, but also of the morbid actions of the stomach and bowels, as well as of the muscular spasms, the sinking of all the vital and animal powers, of the shrunk and collapsed state of the surface of the body, of the black thick state of the blood, and of the rapid depression of the animal temperature.

4. The morbid state of the perspiration, and the peculiar appearance of the evacuations, proceed from the alteration produced in the condition of the blood; and it is chiefly through the medium of the cutaneous surface, and of the mucous membranes, assisted perhaps also by the other secreting viscera, that this morbid state of the blood is remedied, and its impurities removed, in those who recover; and that the morbid effluvium or poison, which propagates the disease, is formed on these surfaces and membranes during the elimination of the impurities generated in the circulation.

5. The consecutive symptoms of the disease, whether those chiefly depending upon the state of the nervous functions, or of the circulation within the brain, or proceeding from the condition of the abdominal viscera, arise partly from the shock received by the vital energy of the frame in the early stage of the disease, partly from the arrest of the natural secretions, and the unchanged and impure state of the blood; and partly from the congested condition of the large veins and important viscera.

6. That neither the causes which occasion this pestilence, nor the pathological states which constitute its various stages or grades of intensity, have any connexion with those of any of the forms of cholera—whether the common *bilious*

variety, or the more severe form usually denominated the *spasmodic*, the *mort de chien*, &c.; and that, therefore, this name should be discarded from all scientific descriptions of the present pestilence.

Such, sir, are some of the conclusions at which an attentive examination of what has been published respecting this disease, has enabled me to arrive. As to the particular denomination, which may be applied to it, I conceive that some one, pointing to its chief pathological states, and its prominent tendencies, ought to be preferred: the intense influence of its exciting cause upon all the respiratory actions and functions, as well as upon the actions of the heart and state of the pulse, and its marked tendency to propagate itself, and to terminate fatally, induce me to apply to it the name of *Asphyxia pestilenta*, or pestilential asphyxia; and I would venture to offer the following definition of it:—

NOSOLOGICALLY.—Great oppression and anxiety at the præcordia, with giddiness, faintness, and general depression of the vital energies of the frame; cold, clammy, purplish, and shrivelled state of the surface; cold and raw state of the expired air; ejection of the contents of the stomach and bowels, with general tremors, spasms, and distress; a sense of oppression, pain, and heat at the epigastrium, with rapid sinking of the pulse, and of the animal heat.

PATHOLOGICALLY.— Paralysis, more or less complete, of the lungs; arrest of the changes effected by respiration on the blood, and of all the natural secretions; the circulation of a dark and otherwise vitiated blood through the arteries, with congestion and stagnation of the fluid in the large veins, sinuses of the encephalon, and internal viscera; sudden depression and rapid sinking of the heart's action; at last, a black, glutinous condition of the blood, with stagnation of it in the large veins and cavities of the heart, followed by death.

The most important considerations connected with this subject, are such as respect the prevention of the disease: when it is fully developed, there is reason to believe that even the most energetic means of cure will entirely fail, in a large proportion of cases. The measures of prevention which I would especially recommend for adoption, are the following:—The use of sulphate of quinine, with camphor, or the aromatic spices, in moderate doses, taken night and morning, whilst the disease is prevailing in the vicinity; regular living, with a regulated state of the stomach and bowels, by means of tonics combined with gentle aperients: a firm state of the mind, and absence of all apprehension of the disease.

During the occurrence of the malady in our vicinity, or in our families, these means are more particularly required:—A free ventilation of every apartment ought to be constantly attended to, in conjunction with fumigations, by means of aromatic substances kept slowly burning, or by the vapours of chloruret of lime. The attendants on the sick should especially attend to the measures now prescribed, and ought never to exert their attentions on the affected so near their persons as to inhale the effluvium emanating from them, without fortifying the vital energies in the way pointed out; and they should carefully avoid entering upon those duties with an empty stomach, or when fatigued. Besides burning warm aromatic substances and odoriferous gum resins in the apartments of the house in which any person may be affected, a saturated solution of camphor, in aromatic vinegar, or in the pyroligneous acid, should be occasionally sprinkled on the floors of the apartments. These means, with a thorough ventilation and a due attention to cleanliness, will not only, I am persuaded, counteract the influence of the effluvium proceeding from the affected, and ward off its action on even the predisposed, but will also prevent the clothes, bedding, or furniture of the apartments of the sick from becoming imbued with it to such an extent as can communicate the malady. They are within the reach nearly of all; and, in the event of the extension of the pestilence to any considerable town or city, if care were taken to see them put in practice, under the direction of Medical Councils of Health, one of which should be formed in each district or quarter, much good would result from them.

Having been required, in the various fields of practice, to treat diseases sometimes as rapid in their progress and as fatal under the usual methods of treating them as the malady now the subject of consideration, I conceive that it will not be considered presumptuous if I recommend a trial of those means which, from my having employed them beneficially in these diseases, I would myself be induced to employ in the one now so much dreaded. When the usual measures fail, others which have succeeded in similar states of morbid action, particularly when they cannot prove detrimental, should be resorted to; and I am not aware that the following means, particularly as respects the combination of them, have ever been employed in this disease—at least at the time when I transmitted them, with more detailed remarks, to the Russian ambassador, or subsequently sent them to the Foreign Quarterly Review.

As I conceive that the exciting cause of the disease makes its first morbid impression on the nerves of the lungs, the propriety of directing the means of prevention to that quarter, and treatment also, will appear obvious. The means which I have noticed for the former purpose have been directed accordingly; but the morbid actions constituting the malady are of that kind that cannot be removed by medication administered in that quarter further than in a subsidiary manner. If, therefore, I were called to a severe case of this disease, besides directing blood-letting, if the circumstances and symptoms of the case appeared to warrant it, I would recommend the patient to have a bolus consisting of from ten to fifteen grains of camphor, an equal number of grains of calomel, one or two grains of opium, and ten drops of any essential oil—as of cloves, mint, cajeput, &c. with a sufficient quantity of conserve of roses. This should be administered without any regard to the presence of vomiting. If this be retained, another may be given, and repeated in from one to two, three, or four hours, according to the urgency of the attack; but if rejected, it should be immediately repeated, until it at last remains. At the same time external heat should be applied, and frictions, with a liniment composed of two ounces each of liquid ammonia, of olive oil, and of camphor, with three ounces of spirits of turpentine, and a few drachms (from three to six) of hard soap and Cayenne pepper, to which one or two drachms of cajeput and lemon oils may be added, ought to be assiduously employed. From two to four hours after the exhibition of the bolus, a draught, consisting of from two drachms to half an ounce of spirits of turpentine and the same quantity of olive oil, with a few drops of the above essential oils and forty grains of magnesia, should be taken in mint water; and if it be rejected from the stomach, another should be given, and repeated, if again rejected, in half an hour afterwards; if retained, not until from six to twelve hours, when another may be taken. I have seen cases where the most urgent vomiting existed, and yet the above remedies (although both the bolus and the draught were taken at the same time) allayed instead of aggravating this symptom.

In order to promote the influence of these means, a lavement, consisting of twenty grains of camphor, from half an ounce to an ounce and a half of spirits of turpentine, and an equal quantity of olive oil, in a suitable vehicle, should be administered, and repeated according to the circumstances of the case. Much will depend upon the succession in which these remedies should be given, the periods which should be allowed to elapse between their exhibition, on the doses, and the decision with which they may be prescribed. When the irritability of the stomach continues, and if the attack be severe, then flannels, wrung as dry as possible out of very hot water, and immediately soaked in oil of turpentine, ought to be instantly applied, as warm as possible, over the stomach and abdomen, and retained there, or renewed, until a decided effect is produced. This is the most powerful means I am acquainted with, and the most successful in procuring reaction and restoring the heat of the body. In aid of these means, and when reaction is either commencing or even expected, effervescing draughts, composed of the carbonate of ammonia, and the pyroligneous acetous acid, mint-water, and aromatics, either in the state of spirit or of essential oils, may be given from time to time. Having found the above treatment eminently successful in treating diseases of remarkable malignity and fatality—in rousing the energies of life, restoring the secretions, and removing internal congestions—more particularly in the treatment of puerperal and other fevers of an adynamic or malignant character, I have been induced to recommend it in the present pestilence.

JAMES COPLAND, M.D.

Consulting Physician to Queen Charlotte's Lying-in Hospital, Senior Physician to the Royal Infirmary for Children, &c.

Bulstrode-Street, Nov. 12, 1831.

CHOLERA—THE TOPAZE FRIGATE.

To the Editor of the London Medical Gazette.

SIR,

IN common with all my professional brethren, I feel deeply for the consequences of the invasion of the Cholera Morbus into this country; and likewise I feel deeply the awful responsibility to be incurred by those entrusted with the lives of their fellow-creatures. Under existing circumstances, therefore, the establishment of the truth, with regard to the contagious or non-contagious nature of this destructive malady, is of paramount importance; and to have it in my power to correct a misstatement on either side which might lead to erroneous conclusions, and not to do so, I should consider a dereliction of duty. It has been stated repeatedly that the Topaze frigate conveyed the cholera to the Mauritius, from Trincomalee, in the island of Ceylon; which has been denied in the newspapers, journals, and verbally at the Westminster Medical Society, on the 5th instant, by a gentleman whom the audience considered good authority, being informed by himself that he was in India at the time. This gentleman stated positively that cholera had existed at the Mauritius previous to the arrival of the Topaze; that this ship could not have conveyed the disease, for that in point of fact the cholera was not on board.

The subjoined extract from the journal of the surgeon of that ship, now an official document, written and lodged at a public office years before the present crisis was contemplated, will set this question at rest.

Part of the public have been prevailed upon to believe that the disease is not of a contagious or communicable nature. Should such an opinion, or reliance upon such opinions, lead to the neglect of any precautionary measures, it would, indeed, be lamentable, as thousands might suffer the penalty of the bad judgment of a few*.

I am, sir,
Yours, &c.
A. COPLAND HUTCHISON.

Duchess-Street, Portland-Place,
14th Nov. 1831.

Extract from the Journal of Mr. James Foy, Surgeon of His Majesty's Ship Topaze.

As many of the sick as could be accommodated were sent to the Naval Hospital at Trincomalee, on the ship's arrival there on the 5th September, 1819; they amounted to twenty-six, leaving forty-six on board. Four of the cases of dysentery that were sent to the hospital relapsed there and died. Whilst in this harbour Robert Thomas, sail-maker, who had been employed working on board the hulk previous to his illness, and Charles Plato, a marine, who attended in the purser's steward's room, and was exposed in that situation to sudden vicissitudes of temperature—both died on board of cholera Indica, after a few hours illness; the former on the 16th, and the latter on the 20th September. Some others were attacked with this disease, but they recovered. David Pearce, seaman, a man formerly much employed in the hold, also died at this time, having had a relapse of dysentery. Permission could not be obtained from the commanding officer to inspect the body of the latter.

The ship being ordered to the Mauritius, all but one of the remaining sick belonging to her were embarked again from the hospital, under the impression that the change of climate would be beneficial to them, for but little essential improvement, if any, had taken place in a great number of them whilst on shore. The situation of the hospital is not considered favourable to recovery. The ship therefore sailed in this state from Trincomalee on the 9th of October, having fifty-seven on the sick list; and immediately after cholera Indica broke out, and attacked seventeen, four of whom fell its victims, viz. William Smith and John Dixon, seamen, who had not completely recovered from dysentery; the latter was one of the men returned from the hospital, and who had been ill with hepatic and dysenteric symptoms ever since he was on shore watering at Singapore, three months before: James Cooper, seaman, an old man sent on board by the civil power, for smuggling, who was much depressed in mind, and had suffered from scurvy on the passage from Manilla: William Steward, seaman, lately entered from a merchant ship.

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* The official document was put into my hands by my friend, Sir Gilbert Blane, to whom it was furnished by Sir Wm. Burnett, the medical commissioner of the navy.

There died also on this passage to the Mauritius, Esau Sheldrake, ship's corporal, of acute hepatitis, and Charles Farr, marine, of dysentery; the latter had been received in a hopeless state from the hospital.

On the arrival of the ship at the Mauritius on the 29th October, all the sick that were confined to bed, in number thirty, fifteen of whom were of the number received from Trincomalee Hospital, were comfortably accommodated in the Military Hospital at Port Louis, under the care of the military medical officers. The remainder, with the convalescents, were placed in the quarters on Tonnelier Island, which were given up for their accommodation by the General commanding. Six of the men sent to the Military Hospital died there; viz. four of the sequelæ of cholera Indica, with which disease they had been seized on board; one of these four had brought on enteritis by inconsiderate exertion and exposure on board when convalescent: the other two were long standing cases of dysentery. The whole six were of the number received ill with dysentery from Trincomalee Hospital: of the sick who were placed on shore on Tonnelier Island, four died who had been long ill; viz. John Twaites (cook's mate), Philip Harvey, James Coffy, and Francis Lilly (seamen): all of them had recovered from dysentery and cholera Indica a short time before, but remained extremely debilitated, and had a return of dysenteric symptoms, under which they sunk.

Three weeks after the arrival of the ship at Port Louis, the cholera Indica made its appearance amongst the inhabitants, and continued to carry off in Port Louis from fifty to sixty persons daily, chiefly slaves. It appeared immediately afterward in other quarters of the island with equal fury. It was suspected by the inhabitants that the disease had been introduced by *His Majesty's* ship Topaze; for, although the Medical Committee, composed both of English and French practitioners, had, in their public report, declared the disease not to be contagious, nor of foreign origin, yet some of the same French practitioners suffered the inhabitants still to entertain their suspicions, and even appeared to agree with them, notwithstanding their public declaration. The consternation was unbounded and nearly general. In consequence, remonstrances were made against the ship being taken into the Trou Fanfaron, the only place of safety for a ship in the then approaching hurricane months, and the wishes of the General commanding were acceded to, by taking the ship to the Seychelles Islands, where she remained during the hurricane season; but previous to the departure from Port Louis, six men were sent to the Military Hospital, being unfit to be carried to sea; three of them, severe cases of dysentery, died there; one of the latter was of the number received on board from Trincomalee Hospital ill of dysentery, and was afterwards seized with cholera Indica on board; he recovered from both of these, and became convalescent, but from irregularity in his diet relapsed. Dysentery still harassed the ship's company during her stay at the Mauritius; but no case of cholera occurred after her arrival there, although all the merchant ships in the harbour were losing men by this disease. The weather was warm and without rain; the thermometer ranged from 78° to 82°. The cases of dysentery were separated from the rest of the crew as soon as they appeared, by landing them on Tonnelier Island, under the care of Mr. Porteous, assistant surgeon of this ship, to whose ability and uncommon attention these unfortunate men were indebted for their recovery.

WARM AIR BATH.

To the Editor of the London Medical Gazette.

SIR,

I SHOULD feel obliged by your giving insertion to the following case, in which the "warm air bath" was used with decided benefit.

I am, sir,
Your obliged servant,
W. WOODMAN.

Exeter, Nov. 8, 1831.

On Monday morning, November 7th, I received an urgent message to see Elizabeth Sprague, aged 65, residing in a very confined part of the city. On my arrival I found her writhing with agony, and complaining of excruciating pain and icy coldness of the stomach. The surface of the body was quite cold, the countenance anxious, the pulse almost imperceptible. She informed me she had been in that state about half an hour, but had been complaining all night of some pain in the stomach, with cramps in the legs, and had not been well since Saturday; she had not vomited or been purged, and the cramp had now left the legs; tongue clean and moist.

I immediately gave her 30 drops of cajeput oil in a little warm water; in about five minutes she vomited what appeared to be her breakfast; she now felt relieved for a few minutes, but the pain soon returned with its former severity. A large mustard cataplasm was now applied to the stomach, and 20 drops more of the cajeput oil, given in warm brandy and water; she soon after felt rather less pain of the stomach, but the coldness of the surface, the pulse, and other symptoms of collapse remaining unabated, I was induced to use the warm air bath, and am happy to say with complete success, as, in about five minutes the surface of the body became warmer, and in about eight minutes more it was above the natural temperature, with the pulse full and throbbing, and attended by complete relief at the stomach. In the evening she felt quite comfortable, with a soft pulse and moist skin.

My intention in reporting the above case is to call the attention of the profession to its resemblance in some of the symptoms to cholera, and to the facility with which, in the short space of thirteen minutes, re-action was completely established, the temperature of the body being raised above the natural standard, and the pulse, from being almost imperceptible, becoming full and throbbing. I should therefore earnestly recommend the different Boards of Health to provide a proper supply of the baths, in order that a fair trial may be made of their utility, should the cholera unfortunately appear among us. Its portability renders it applicable in every instance; and without the least fatigue to the patient we are enabled to surround his body with an atmosphere of warm dry air, which, acting as a stimulus to the whole surface, will tend to relieve internal congestion, and restore the cutaneous circulation to its natural state.

INHALATION IN CHOLERA.

To the Editor of the London Medical Gazette.

October 25, 1831.

SIR,

IF you think the following hint likely to be of practical use, it is much at your service for insertion.

During the perusal of some papers inserted in your Gazette, particularly one by Dr. Barry in the 191th Number, in which he describes cholera to be most fatal in its first stage, and when accompanied by the following symptoms—"Sudden paralysis, and rapidly diminishing action of the heart, of the arteries, and of the organs of respiration, with stasis and thickening of the blood, the loss of the power to generate heat," &c., the following idea occurred to me, viz. that these symptoms might perhaps be *relieved* by the inhalation of the protoxide of nitrogen, or dephlogisticated nitrous air of Priestley. Most of the profession are aware of its exciting effects, and that those effects are immediate, and not followed by the depression usually produced by opium or spirits.

I remain, sir,
Yours sincerely,
M.

ABORTION.

To the Editor of the London Medical Gazette.

Lincoln, October 1831.

Sir,

Should you think the following case of sufficient interest, I shall feel obliged by your giving it insertion in the Medical Gazette.

I am, sir,
Respectfully yours,
Frederic Kent.

—— Lee, aged —, called upon me, in August, 1830, to request me to attend her in labour, which she expected would take place in November. She has had several children, and reckoned with great accuracy. Thought she quickened early in July. I was not sent for till the 13th of December, at which time she was not larger than a woman four months gone with child. She was complaining of violent pain in hips and loins, sickness, great debility, with loss of appetite and sleep. During the last two or three months her abdomen had been distended to the size of a woman at her full time, when the above symptoms were much increased, and accompanied with pain and difficulty in making water, great pain when having stools, and tenesmus. These symptoms were relieved by the discharge of two or three quarts of a brownish-yellow fluid, not in the least offensive, and generally having several pieces of thin membrane floating in it: occasionally the fluid was mixed with blood. On examination per vaginum, I found the neck of the uterus nearly its natural length—the uterus rather larger than a child's head. I now asked her very particularly about her pregnancy, and, from her account, I was satisfied she conceived about the latter end of February or beginning of March. I do not think it necessary to enter into a detail of the treatment. I tried, upon two occasions, to expel the contents of the uterus, by the secale cornutum, but with no good effect. She continued to have the occasional discharges of fluid till the 28th of January, 1831, when, after violent flooding, a placenta was expelled about the size of a child's head. In spite of all sorts of treatment she continued, from this time to June, to have repeated floodings, with discharges of fluid. At the latter end of June she could not get out of bed; kept alive almost entirely by wine and quinine. In September the floodings had nearly ceased, and she was able to walk about the house.

October 20th.—She still has occasional discharges of fluid, but in less quantity, and is gradually gaining strength.

On laying aside the membrane attached to the placenta, the latter presented a surface covered with hydatids of a purplish colour, varying in size, from a pea to a small walnut; several loose folds of membrane, which appear to be remnants of the burst hydatids; a blighted ovum, rather larger than a horse-bean, attached by a funis near the middle of the placenta; eyes and extremities distinct. It was discharged, as nearly as can be calculated, eleven months after conception.

I consider the above case rather singular, as I believe hydatids, connected with the uterus, are always described as pedunculated; and, on referring to Bailie, Burns, and Blundell, I find nothing similar to the above.

OVARIAN DROPSY.

To the Editor of the London Medical Gazette.

Sir,

The annexed case is at your service, if you think it worthy the notice of your valuable periodical.

I possess notes of a very interesting case of asphyxia, from hanging, successfully treated. I opened the larynx, and inflated the lungs with a common elastic gum catheter. Also three of lithotomy—respective ages, 2½, 8, and 70, in which I used, with very great satisfaction, Frère Come's instrument, the lithotome cachée, so much condemned by some eminent surgeons. It possesses all the advantages of the knife, without that great defect—not being able to regulate the extent of the incision through the prostate.

Should you wish these, I will with much pleasure send them to you.

I am, sir, a subscriber,
and your obedient servant,
Thomas Fereday.

Dudley, Oct. 31st, 1831.

Case of Ovarian Dropsy, complicated with Psoas Abscess and Pregnancy, spontaneously subsiding.

Mrs. Hall, æt. 40, of naturally healthy constitution, and fresh-coloured countenance, had a child in the year 1810, ten months after marriage, and in the twentieth year of her age. Her husband died, and she again married in 1814, and eighteen months afterwards had a second child.

About eight years ago she had uterine disturbance to such a degree as induced her medical attendant to tell her she would bear no more children. What this affection was I cannot satisfactorily learn, the practitioner to whom she applied being dead. She says that a purulent discharge continually issued from the neighbourhood of the uterus, and that a large bougie was occasionally introduced up the vagina. From this and the attendant constitutional symptoms she recovered in about four years.

Menstruating regularly, she again conceived about Midsummer, 1830, sixteen years from the birth of her last child, attended, during the early months, with an aggravation of the usual symptoms of pregnancy. These subsided, but recurred towards its close, accompanied with pain in the loins, a tenderness and tumefaction in the right iliac region, pains in the hips, numbness of the right thigh, a disagreeable taste in the mouth, and slight fever.

March 10th. — The predominant symptoms are the shooting pains in the loins, the numbness of the thigh, and the nauseous taste in the mouth, compared by the patient to rotten eggs. There is an enlargement, about the size of a child's head, with rather indistinct fluctuation to the right of the spinous processes of the lumbar vertebræ.

23d.—The pain in the back has been so severe for the last week that the patient has been confined to her bed. There is evidently a deep-seated fluctuation. A puncture discharged about a quart of good pus, with great ease to the patient, but had no apparent influence upon the ovarian (?) swelling. The wound was closed with sticking plaister, and a flannel roller bound round the loins. In the course of the evening, and after some hours severe pain, the dressings gave way, letting out a quantity of matter, nearly equal to that withdrawn in the morning, and with as great relief.

24th.—There has been severe pain in the back, shooting from thence along the course of the psoas muscle, with much bulging in the former part. For the first time, too, an oblong swelling, about the size of a large hen's egg, is observed midway between the crista of the ilium and the os pubis, above Poupart's ligament, and apparently in the direction of the inguinal canal. The ovarian (?) tumor remains pretty nearly in the same state. The contents of the abscess in the back were again discharged, by which the inguinal swelling was lessened.

In the night labour came on, but so reduced was the patient, from want of rest, suffering, and great secretion, that she became alarmingly exhausted after a few hours continuance of labour. The mouth of the womb being fully dilated, and the head of the child at the brim of the pelvis, (too high to admit of the application of the forceps with effect) I judged it more advisable to deliver by turning, than subject my worn-out patient to a repetition of inefficient pains, gradually becoming weaker. I effected this with very little difficulty, and no violence, the uterus scarcely recognizing the presence of my hand. The placenta and membranes were expelled in about a quarter of an hour by the natural contraction of the womb, the latter organ being distinctly felt as a round hard ball in the hypogastric region. Yet the abdomen did not appear lessened, and it was thought by the attendants that there was still a child *in utero*. It was evident, however, that the enlargement occupied principally the right side. After remaining quiet a full hour, I had her very quietly put to bed, and administered thirty drops of laudanum in a little warm wine and water.

25th.—The swelling in the iliac region continues, as do the pains in the hips, the numbness of the thigh, and the disagreeable taste in the mouth. There is about the usual quantity of lochial discharge.

In the evening of the 26th I was sent for in great haste in consequence of a great and very sudden discharge of watery fluid from the vagina, estimated by the attendants at two or three gallons, but admitting of no accurate measurement, as it escaped among the bed-linen. The ovarian (?) swelling disappeared; the pain in the hips, and the numbness of the thigh, subsided; and, for the first time for a month, the patient had a comfortable night's sleep. From this time she became convalescent; the discharge from the back continued unabated for about a fortnight, and then very slowly, but gradually decreased; a small quantity, however, still remains, with but little pain, and scarcely any inconvenience. The ovarian (?) disease, with its attendant symptoms, vanished; the pulse, which during the whole progress of the case had been accelerated, became more quiet; and the tongue, which had been coated with a white fur, became clean; whilst by nourishment and fresh air she acquired sufficient strength to suckle her child. Both are now, (October) and have been for some months, stout and healthy, the mother having long resumed her ordinary domestic avocations.

In relating this case, it is obviously not with an intention of describing the treatment adopted: that was necessarily very simple, and such only as tended to relieve general and urgent symptoms. This, therefore, I have not deemed it necessary to introduce.

The great peculiarity consists in the spontaneous discharge of a thin and limpid fluid from the vagina, attended with the immediate subsidence of an intumescence in the right iliac region, together with its attendant consequences, pain in the hips, numbness in the thigh, and disorder of the stomach. These symptoms are readily accounted for; the former from the pressure of the swelling upon the anterior branches of the lumbar and sacral nerves, the latter from the universal sympathy which exists between the generative organs and the stomach.

Now it would seem that the right ovary was predisposed to disease in consequence of the disturbance that existed in the uterus years back, when, possibly, adhesive inflammation had agglutinated the fimbriated extremity of the fallopian tube to the ovary, and that the process of utero-gestation had proved the exciting cause to the dropsical effusion, (in which, perhaps, the fallopian tube was implicated) which, by the bursting of the sac which contained it, had been transmitted through the tube to the uterus, and thus effected its escape.

It could not have been dropsical effusion of the uterus, for that would have required a closure of the cervix during its formation, which would have been incompatible with a constant and free discharge of the lochia. The same may be said of that irritable state of the vessels secreting the liquor amnii to excess, as mentioned by Dr Mason Good. It could not have been an hydatid, for it must have had an envelope; its expulsion, too, would have required much uterine pain; in addition to which, the uterus was distinctly recognized as a firm and hard tumor in the hypogastric region, totally distinct from that I conceived to be the ovarian enlargement.

It is universally admitted that a vesicle in the ovary bursts from the stimulus of the seminal fluid, and that the ovum is conveyed from thence through the fallopian tube to the uterus; and why, by a sudden movement of the body, or other agent, may not the sac of an ovarian dropsy be lacerated and its contents escape into the uterus?

If, then, this view be correct, it must be considered a very fortunate and rare termination of a disease very little under the control of medicine. Such an event must not frequently be expected.

I have purposely avoided much notice of the abscess, evidently a psoas. It was unusually rapid in its formation and reproduction, and unattended with disease in the vertebræ. It had been punctured in the back before it pointed in the groin. A depending opening would certainly have expedited the healing, but one was already made, and I hesitated making another through the aponeurosis of the abdominal muscles, thereby rendering weaker a part already too susceptible of important diseases.

AMPUTATION OF HIP-JOINT.

To the Editor of the London Medical Gazette.

Glasgow, Nov. 12, 1831.

SIR,

MAY I request the favour of your publishing, in the next number of the Gazette, the following case of successful amputation at the hip-joint, which was under my care in the infirmary here during last summer.

I am, sir,
Yours, &c. &c.
JOHN MACFARLANE, M.D.
Senior Surgeon to the Glasgow Royal Infirmary, &c. &c.

Elizabeth Kerr, two years of age, was admitted about 11 o'clock of the forenoon of 6th July last, on account of a compound fracture of the left femur, in its upper third, with extensive laceration of the muscles and integuments. This severe injury was produced, an hour and a quarter before, by the wheel of a loaded waggon passing obliquely across the upper part of thigh. The bone was fractured transversely, a little below the trochanter, and completely denuded of integuments and muscles anteriorly, to within an inch of its condyles. Posteriorly the soft parts were also dreadfully lacerated and contused, especially at the upper boundary of the popliteal space, where the limb was only retained by a narrow flap of integument and muscle. The femoral artery and vein were torn across, opposite the fracture, but the bleeding was trifling, and consisted only of a slight oozing from the latter vessel and from the lacerated surface. There was also a compound comminuted fracture of all the phalanges of the left little finger.

The child was carried to the hospital from a distance of two miles, the injured limb being extended on a pillow. The countenance was pale and anxious, the pulse rapid and feeble, and the body cold. The usual expedients for exciting re-action were promptly adopted; heat was applied to different parts of the body, and warm wine negus administered. A consultation was immediately called, and I saw her for the first time at a quarter to 12. She still lay in a state of collapse, but was quite sensible, and cried bitterly when the limb was examined.

From the nature and extent of the injury, it was sufficiently evident that the child's life could only be preserved by amputation at the hip-joint. This operation I was anxious at once to perform, but from the absence of my colleagues it had to be delayed, and a second consultation summoned at 1 o'clock. At this hour I was favoured with the presence and assistance of my friend, Dr. Perry, with whose concurrence I immediately proceeded to the operation.

An outer and an inner flap were formed by transfixing the limb with Lisfranc's knife, which was passed so closely around the head of the femur as completely to divide all its muscular coverings; the capsular ligament was opened with a scalpel, the bone disarticulated, and the limb removed; the femoral artery was effectually compressed by an assistant, and not more than six drachms of blood were lost; five vessels were secured by ligature, after which the flaps were brought together, and retained in apposition by adhesive plaister, compress, and bandage.

During the operation the child was exceedingly weak, and before the wound could be dressed it was in a state of syncope, and appeared to be dying; the pulse at the wrist was imperceptible, and it was even with difficulty the pulsations of the femoral artery in the wound could be discovered; the face was deadly pale, and covered by clammy perspiration; the body cold; the eyes fixed in their sockets; the pupils dilated; and the breathing hurried, interrupted, and laborious. Stimulants were freely administered, and the child was removed to bed, having been about twenty minutes on the table. She lay in a state of collapse and insensibility till 7 P.M. when, after vomiting once or twice, reaction was gradually established. At 8 the stimulants were discontinued; the pulse was 130; skin hot and dry; the fractured finger was now removed, and a poultice applied.

Ol. Ricini, ʒij. et post hor. duas enema domestic. Mistur. Diaphor. c. Vin. Antimon. 3tia q.q. hor.

7th.—The child is not weaned, and has been sucking freely; was much annoyed with startings during the night; pulse 140; skin hot and dry; respiration hurried; no oozing from wound.

Submur. Hydr. gr. ij. et rept. post hor. quatuor. Contin. Mistur.

Half-past 7, P.M.—Has been in a drowsy state since two o'clock, from which she can with difficulty be roused; eyes clear and sensible to light; pupils contracted; increased heat of skin, and flushing of face; troublesome startings; pulse 160, feeble; no stool.

Enema domestic. Abrad. Capill Bladder with iced water to head. Contin. alia.

8th.—Is more alert, and answers questions readily; respiration still hurried, and there is mucous râle in trachea; pulse 160; bowels free.

9th.—Had rather a quiet night, with less starting; tongue cleaner. On undressing wound, flaps were found adhering throughout, except a small portion at upper angle, which is sloughy.

Omit. Medic. Four ounces beef-tea.

11th.—Pulse still very rapid, but febrile excitement diminishing. Edges of wound clean, cut granulations pale.

Vin. Rubri, ℥j. in dies.

13th.—Was more restless last night, and is troubled with cough, dyspnœa, and occasional vomiting. Mucous râle is heard over the whole thoracic parietes. Countenance rather sunk; frequently refuses the breast; pulse very rapid and weak.

Mistur. Mucilag. c. Træ Opii. Calomel, gr. ij. 4ta q. q. hor. Contin. Vin.

Without continuing the diurnal reports, I may shortly state, that the symptoms of bronchitis continued rather urgent till the 19th, when they began gradually to diminish. From the 22d till the 6th of August, although the wound continued steadily to heal, there existed a good deal of irritability of the bowels, accompanied by diarrhœa, and an aphthous state of the mouth. During this period, three molar teeth cut the gum, and ultimately gave relief to the symptoms. The wound gradually healed; the child improved daily in strength and spirits, and was dismissed cured on the 13th of August.

There were several points in the history and progress of this case which rendered the prospect of its ultimate success extremely problematical. The extensive nature of the injury, and the alarming collapse which it produced, might have reasonably deterred almost any surgeon from undertaking, in circumstances so apparently hopeless, the performance of so severe and formidable an operation as that of amputation at the hip-joint. On first examining the injured limb, I was induced to view the case as almost, if not altogether, hopeless. It was only, however, by amputation that the child's life could be preserved; and although I was aware that this operation might, in a subject so young, and already so much sunk by the shock of the injury, be productive of immediately fatal consequences, I considered that I should have failed in my duty had I not recommended and adopted it. I regretted at the time the delay that took place in obtaining a consultation. The child was in a more favourable state for amputation at eleven than at one; and had it been performed two hours sooner, I am convinced that the shock to the nervous system would have been lessened by its nearer approximation to, and by its becoming in some measure blended with, the shock of the injury. It was evident that the collapse which existed before, and for about five hours and a half after amputation, was not occasioned by loss of blood, but depended on the depressed condition of the nervous system, produced by the injury and aggravated by the operation. With the view of lessening as much as possible the injurious effects of the operation upon the nervous system, I was anxious to perform it rapidly, and with little loss of blood. Both these objects were in part attained. The formation of the flaps, disarticulation of the femur, and removal of the limb, did not occupy a minute and a half, and not more than six ounces of blood were lost. I am no advocate for the hurried performance of operations: nevertheless, it must be admitted, that when the pain and irritation of a tedious and protracted operation come to tell, as they must do, injuriously on a constitution which has already sustained a violent shock, the most serious consequences may be anticipated; and we are certainly warranted, in such circumstances, in expediting the operation as much as is consistent with its safe and efficient performance.

The youth of the child was also inimical to the success of the operation. The physical irritability which exists during the two or three first years of childhood is so great as to render the performance of any capital operation extremely dangerous. I am not aware that there is any case on record in which amputation of the femur at the cotyloid cavity has been performed on so young a subject; and I have been chiefly induced to communicate this case to the profession, in order to shew, that in serious injuries inflicted on young children we ought not altogether to despair of success, even in the most desperate circumstances, but to proceed with those operative measures which the peculiarities of the injury may demand. I must state, however, that I have witnessed amputation of the hand twice on children under three years of age, and in both cases convulsions supervened, and proved fatal in less than twenty-four hours. We would not willingly select such cases for operation, neither should we decline to use the knife when it is absolutely called for.

We had not only the immediate dangers of the operation to deal with in the case of Kerr, but we had also to contend with a series of untoward occurrences during the progress of the cure. 1st. The collapse was unusually severe and protracted; and, as generally happens, was productive of excessive excitement. 2d. The continuance of this febrile excitement, for eighteen hours, produced an affection of the brain, accompanied by partial coma. 3d. The occurrence of bronchitis, which was rather protracted and severe, was another source of danger; and, 4th, the existence of troublesome diarrhœa, with dentition. The child not having been weaned was, I think, also detrimental to its recovery. The continued anxiety of the mother; her absence from her family, and her close confinement in the hospital, occasioned a continued diminution, and, occasionally, an almost complete suspension of the secretion of milk; and as the child refused spoon-meat of all kinds, it must be obvious that she was often but imperfectly nourished.

Since she was dismissed from the Infirmary, she has completely regained her health and strength, and she is now able to move about the house with the aid of crutches.

MEDICAL GAZETTE.

Saturday, November 19, 1831.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

MODE IN WHICH CHOLERA IS PROPAGATED.

WE last week laid before our readers a *resumé* of those considerations which had induced us, after mature investigation, to conclude, that *the* cholera was a contagious disease — by which term we understand, a complaint capable of being transmitted, mediately or immediately, from the sick to the healthy. To our minds the narrative, when examined as one continuous chain of events, is convincing; but to produce its impression, the question must be considered in this manner,—if we content ourselves with a few links only, or view with miscroscopic eye certain detached facts—we naturally magnify them into unreal dimensions, and lose sight of the general bearings of the whole. Now this is precisely what the non-contagionists appear to us invariably to do: they bring forward certain cases, in which individuals exposed to cholera have escaped, and draw from them the illogical inference, that those who have been similarly exposed, and taken the disease, have done so from some other cause than the exposure. Let us consider this matter a little farther. One of the persons cut off at Sunderland the week before last, was taken to the hospital; of the nurses employed about the patient, one took the disease, and the others escaped. The only rational deduction from these premises is, that so far as one example goes, they shew cholera to be contagious, but not necessarily to affect all who may be exposed to it. But the anti-contagionists are not content with this: they say, no; the escape of the nurses, who did not become affected, neutralizes the evidence. Now let us apply the same mode of reasoning to the agency of any other influence external to the individuals—let us, for the sake of argument, suppose cholera to arise from one of the many hypothetical explanations which have been offered; *e. g.* subterraneous changes. The nurses inhabiting the same dwelling, must all have been exposed to this same subterraneous incomprehensibility; but as some did not take it, therefore she who did, must have taken it from some other cause than that which left the others uninjured. It is thus quite evident, that no argument can be drawn from the exemption of those who escape, which does not apply to all other supposed sources of the malady quite as forcibly as it does to contagion. Again, it is said that the nurse who died was very much afraid of taking the disease

—a most unfortunate argument for the side it is intended to uphold, because fear is universally admitted to render individuals more susceptible of diseases which are communicable, and serves to explain the circumstance which made her system yield to an impression which others resisted. Nor is there any way of getting over this inference otherwise than by holding, that the nurse died of fright, and not of cholera; but few, we suppose, will be disposed to go as far as this.

We have adduced the preceding illustration, because it has actually been brought forward since our former article was written, as an argument against the accuracy of the views which we adopt; whereas, it proves only—what every one knew before—that all who are exposed to the contagious influence of cholera do not take the disease, any more than all fall sick of small-pox, measles, or scarlatina, who are exposed to their several contaminations. This, however, leads us to make some observations on the nature of the communicating principle, for which neither time nor space served us last week. It appears to us, that the proportion of mankind, who are susceptible of cholera, is smaller than holds good with regard to the other transmissible diseases; but that they who are susceptible, have the susceptibility in a more exquisite degree, so that a much more considerable dilution of the poison becomes necessary before it is rendered innocuous. This dilution generally took place to a much greater extent in India than in Russia, and hence the propagation of the disease by transmission from man to man was much less conspicuous in the former country than in the latter. In India, the spare garments of the inhabitants, their copious ablutions, and the universal admission of air into every portion of every dwelling, tended so much to counteract the influence of the poison, that many were constantly exposed to it with impunity, and hence the true mode of its propagation long escaped detection; but in Russia, the sheep-skin clothing of the lower orders, which is but rarely changed, the stove system, and the close apartments of all ranks, from which indeed every breath of air is carefully excluded, contributed to present the deleterious impregnation in a more concentrated form, and hence the fact of the disease passing from man to man there, became much more apparent. The result, in both cases, was such as might have been expected. Dr. Russell came from India impressed with the opinion, that the disease was not communicated from man to man; he comes from Russia convinced that it is. Dr. Lawrie, an intelligent surgeon from India, observes, "I feel myself called on to say, that, while in India, and even after I had commenced writing the present remarks, I was convinced that cholera was not contagious. I must candidly confess, however, that a closer investigation of the history of the Indian disease, added to a due consideration of the progress which cholera is still making on the continent, has shaken my previously established opinion*." Another Indian practitioner presents himself in Mr. Kennedy, who has gone so far as to assume the principle of transmission in the name he has bestowed upon the disease,—that of the "contagious cholera." And Mr. Orton, known as the author of a most valuable work on Cholera, after enumerating the facts in favour of non-contagion, adds, "These facts appear so clear and strong that if we looked no farther, they would seem to put the matter at rest; but with regard to the general question of the contagious, or (if another term must be used) infectious nature of the disease, *they appear to me no more than dust in the balance against those which may be opposed to them.*"

No country could be more calculated

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* Glasgow Medical Journal, November.

than India to prevent the communicable nature of the disease from being forced upon the attention, because the universality of thorough ventilation constantly enabled many to be exposed to the sick without harm, and these daily instances of immunity diverted men's minds from the truth. On the other hand, probably no quarter of the civilized globe could be better adapted than Russia to bring into obvious and unequivocal display the full development of that principle, from the pernicious influence of which the greatest peril is in concentration, and the only safety in dilution. This doctrine cannot be impressed too strongly on the public mind with a view to the common safety. All can understand, that if a dose of arsenic be put into a glass of water, it cannot be taken without almost certain death; that if it be put into a cistern, there would still be danger, though not to the same degree; but that if it were cast into the Thames, the whole town might drink of it with impunity.

That so many escape who are in close attendance on the sick, proves how few comparatively are susceptible of the disease; that so many become affected without obvious exposure, proves how great the dilution is which is requisite to rob the poison of its power over those whose systems render them open to its attacks. It seems clearly established, that approximation to the sick is by no means necessary to the production of the disease, and consequently the links which form the chain of communication are constantly subject to interruption. Thus we are informed by Drs. Russell and Barry, "that neither the near approach nor the immediate contact of an infected individual were indispensable to the infection of a healthy individual." From this it would appear that the influence producing cholera is of a more diffusible, though not less energetic, character than that which emanates from small-pox, or any of the other avowedly infectious maladies. Now there are two modes in which a poison of this description may be supposed to be conveyed to the previously healthy, without their approaching the sick: first, we may suppose it to impregnate the atmosphere, and be wafted by currents to a distance more or less considerable; and, secondly, that it may be carried by the persons or garments of individuals who themselves resist its impression, or by goods of various kinds. Of both these, numerous illustrations might be adduced, but a few must suffice. Of the former, a striking instance, which we subjoin, is given in the Madras Reports.

"Of two vessels, arriving and remaining at anchor in Madras Roads, one was attacked with the epidemic ten days after arrival there, and the other remained free from it for a fortnight longer; but on the first, in which it was still prevailing, changing her birth, and taking up a situation a quarter of a mile to the windward of the healthy vessel, it was immediately attacked; and those who slept on the side next the infected ship suffered particularly *."

And again: when the disease was raging at a town in Persia, a body of troops passing in that direction "were prevented, by guards situated at the gates, from entering it; but several of them passed the day under the walls. During the following day the disease manifested itself among them, and they suffered from it very severely †."

The above, though strong, are by no means solitary examples of this law; so far otherwise, that it is in accordance with general observation, that places to the leeward of an infected locality were, *cæteris paribus*, decidedly more apt to suffer than others.

The power possessed by those who have been exposed to and imbibed, but without being themselves brought under, the influence of the poison, is a circum-

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[*] Madras Reports, p. 25.

[†] Med. Chir. Transactions, vol. xii. p 359.

stance of still greater importance, because more generally doubted, and more calculated to lead to the unsuspected and unintended propagation of the disease. The opinions to this effect, however, and the examples adduced in attestation of its accuracy, are too numerous and too well authenticated to be got rid of by any summary expression of incredulity. Hear the account given by the Captain of the Bellona, a Russian man of war:—

" 'The Bellona frigate, 44 guns, 320 men, had 16 sick of cholera; first case on 30th June, (O.S.):—This man had not been on shore. *The first communication which the ship had with the shore was on the morning of that day, when the captain, some of the officers, and a boat's crew, went to Cronstadt.* In the evening the first man was seized, and in quick succession fifteen others, not one of whom had been on shore. No officer was attacked.' "

Again, we are informed by Captain Baron Loeventhall, that

" 'The Phœnix brig had been stationed for two months midway between Cronstadt and Peterhoff, about six versts, or four miles, from either, to the S.E. of the former, for the purpose of telegraphic communication with his Majesty. She continued in this situation to the 15th of July, in perfect health, though she sent her boat twice a week to the fortress for provisions. On the 15th she took up her station one verst to the S.W. of the town, and had two men attacked with cholera on the 17th. Neither of these men had been on shore. The surgeon only had gone to Dr. Lange's hospital on the 16th, for the purpose of observing the cholera, which he had not seen before. The doctor said to him, '*You had better not remain here long, else you may carry the disease to your ship, and I shall be responsible**.' "

Another circumstance, which tends strongly to establish the same position, is, that cholera first broke out among the Poles after an engagement with the Russians; and a second time, in a different quarter, the Polish army, which had been separated from its own sick, was again affected with the disease, immediately after an engagement. Now, from the acute and violent nature of the malady, it cannot be imagined that those actually labouring under it could go to battle; the legitimate inference, therefore, is, that they carried with them the contamination, whatever it may be, and communicated it to the opposing army with which they came into contact, not merely at the moment of contest, but many of them subsequently as prisoners. " It seems tolerably well ascertained," says Dr. Russell *, " that the cholera has not broken out spontaneously in any place without communication by persons or effects coming from infected places; but it is somewhat singular and unaccountable that the disease has appeared in situations where the persons arriving did not themselves labour under the disease at the time of their arrival." Certain it is, that persons not obviously labouring under the disease appears frequently to have been the medium of its communication; and this is a very important fact, both as serving to account for the difficulty which frequently exists in tracing out every link in the chain by which it is transmitted from man to man, and also as affording a necessary caution with regard to the degree of intercourse to be held between the healthy and the attendants of the sick. Generally speaking, the period intervening between the effectual exposure and the attack is very short, not exceeding a few days, but in other instances an individual may be exposed to the virus, without its influence being developed for a considerable interval, and he may thus transport it to a greater distance. We are informed that " a convict was sent from a village where cholera prevailed, to Chanda, eighty miles off. Two hours after his arrival he was attacked, and speedily died. Three of the four persons who carried him to the grave, and another who attended them there, were the next sufferers; and shortly the disease became general*."

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* Official Papers, p. 54.

* Private letter.

The inference from all this is, that an individual may commence a journey apparently well, and travel on transmitting the disease to others as he passes; or, on the other hand, if on board ship, he may have already escaped the first stage of cholera, and be on the sick list merely as labouring under fever. "Nothing (say Drs. Russell and Barry) is more certain than that persons may come to the coast of England, apparently labouring under common feverish indisposition, who really and truly are cases of cholera in the second stage†." These observations, we trust, will receive the attention they deserve from all concerned in the planning or execution of measures connected with the public health: even those who think differently from us on the subject of transmission, will still probably admit, that while any uncertainty is attached to the question, the public are entitled to the benefit of the doubt.

CHOLERA AT SUNDERLAND.

The disease at Sunderland seemed for a few days to have been checked, and the fears which its announcement excited were quickly subsiding—indeed many expressed their conviction that *the* cholera had not been there at all. When, however, we call to mind the capricious and insidious manner in which the pestilence has repeatedly behaved abroad, we fear that the confidence with which its immediate disappearance is inferred, is at least premature. In a place so dependent upon its trade, and with so many interested in concealment, from the Noble correspondent of the *Standard* downwards‡, the difficulty of getting at the truth becomes prodigious. Indeed the most contradictory statements have been put forth, on the authority of the medical men at Sunderland, with regard to the disease and its origin. They are very anxious to prove that it was not imported, and that it is not of a contagious nature. They add further, that the town is more healthy than it usually has been at this season of the year. However, it remains undisputed (only because it is indisputable) that sucpected vessels were suffered to come up the river, through the shipping to the Wear, to perform quarantine; and that at least one such vessel had left Hamburgh after the disease was acknowledged to be there. It farther appears, that in Sunderland the disease first broke out among persons connected with the shipping, and spread to several members of the same family; while a nurse, who assisted to remove the body of the first patient who died in the hospital, became affected with the malady in a few hours after. Now, without going farther, we crave leave to ask, how the sudden appearance of a disease, which carries off, within a few hours, more than one-half of those attacked with it, is in their opinion to be accounted for, in a town *unusually healthy*? and by what train of reasoning they have been able to come to the positive conclusion that the vessels from the infected place did not bring it? One question more, and we have done. What kind of *diarrhœa* may that be to which several sudden deaths are set down, at a time when the town is "unusually healthy?" Dr. Daun complained, in his reports, of want of co-operation, and it is quite evident that concealment to a considerable extent was, and probably still is, adopted. Meantime he has positively declared the disease to be *the* cholera, whatever its origin may have been, and whatever its future course; an opinion in which the Central Board in London fully concur. Although held at Sunderland, as at almost every other place when actually present, not to be communicable from man to man, the utmost precautions are taken to prevent the operation of the contagious principle. This is all we want: be practically contagionists, and we care not a rush for theoretical differences of opinion.

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* Mr. Montgomery. Medical Repository, May 1826.

† Official Papers.

‡ See the Marquess of Londonderry's letter in the newspapers.

MALIGNANT CHOLERA AT SUNDERLAND.

	New cases.	Deaths.
Up to Nov. 3	7	5
4	0	0
5	0	0
6	1	0
7	6	2
8	8	5
9	17	10
10	7	4
11	1	2
12	2	0
13	0	1*
14	7	3†
15	3	5†
	59	37

THE COURIER *v.* THE MEDICAL GAZETTE.

The Courier has replied to our exposure of their inconsistency last week, but in a subdued tone, and without being able to show that we were wrong on a single point; in fact, the case did not admit of it. They say it is "impertinent" to call Dr. Johnson's series of letters, in their paper, "protocols:" we know, however, that, in expressing our disapprobation of medical men embracing the opportunity afforded by public excitement, to bring themselves forward through the medium of the daily papers, we do but echo the sentiments of the profession at large. It is a system of advetising, let gentlemen disguise it as they will, and one against which we always have and always shall set our faces, come from what quarter it may. Dr. Johnson, we observe, is now a "contingent contagionist," and he talks of cholera being not "*very* infectious," *à la bonne heure*: if infectious at all, those on whom the attempt to exclude it devolves, ought not to be told, that they would be held blameless, though the disease should pass their boundaries. As to the rest, no man regards the disease as necessarily communicated to those exposed to its influence, except other "contingencies," such as predisposition, conspire to give it effect*.

BOARDS OF HEALTH.

The new arrangements connected with the public health, which we announced last week, have been carried into effect. We understand that the members of the original Board are to constitute a Committee of reference, for the guidance of the Privy Council, with regard to medical questions, on such occasions as their advice may be deemed requisite; while the new, or "Central Board," will devote their whole time to the details, and take upon themselves the entire executive department.

LOUIS-LE-GRAND'S PRESENT TO HIS APOTHECARY.

There is at M^{r}. Garden's, the chemist, in Oxford Street, a superb vase of rich-toned bell-metal—a specimen of the age and munificence of Lewis the 14th. It is a noble mortar, measuring nearly two feet in diameter at the mouth, and weighing upwards of two hundred and eighty pounds—presented, with characteristic propriety, by the *Grand Monarque* to his beloved state apothecary. In the founding, the following inscription was cast upon it in relief:—"Charles Angibaud, M^{e}. App^{e}. et Ordinaire du Roi. A Paris, 1678:" this goes round it beneath the upper margin. On the body is a shield of emblematical heraldry, having reference to the tree of life, and supported on either side by the arms of France. Two rams' heads project from the body by way of handles, though, if M. Angibaud could lift it by them, he must have been able to effect more than any two of his descendants "in these degenerate days." The whole is in the most beautiful state of preservation, though it bears marks of having seen some service too, as no doubt it should, like other *mortars* of the days of Lewis the Great.

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* Besides one death of common cholera.

† Besides one death of diarrhœa.

* In our last number, article headed "Courier *v.* Medical Gazette, p. 214, *for* "the Editorial writers," *read* "Editorial authorities;" and in the quotation from the Times, at the conclusion, *for* "(provided there be time)," *read* "(provided they be timely)."

WESTMINSTER MEDICAL SOCIETY.

Saturday, Nov. 12.

DR. STEWART IN THE CHAIR.

Cholera.

Dr. James Johnson announced that he had in his possession a letter of Dr. M'Whirter's, of Newcastle, which he would beg of the Chairman to read.

The Chairman then read the letter, the substance of which was, that the writer and some other physicians at Sunderland, had had a special interview with Dr. Daun, and *extracted* from that gentleman the opinion, that there was *no proof* of the cholera having been imported into the town by infringement of the quarantine precautions. Dr. M'Whirter also mentioned, that there were *two* nurses employed about the corpse of the patient who had died of cholera at the infirmary; that the one who sunk under the supposed infection had been greatly alarmed; the other one was not frightened, and escaped.

Dr. J. Johnson resumed, by observing that these were important facts; and that, coupling them with the bulletin of the night, that but one more case had occurred since the last announcement, he could not help thinking that a *little* too much alarm had been excited. There were circumstances under which cholera might be considered as occasionally contagious; but if ever those circumstances did exist anywhere, it was at Sunderland, where the lower orders were continually drinking spirits, and the quarters where they resided were the filthiest that could be conceived. Saffron-hill and St. Giles's were palaces to them.

Dr. Sigmond was glad to find Dr. Johnson beginning to entertain right notions on the subject of contagion.

Dr. J. Johnson.—"I was always an advocate for *contingent* contagion."

Dr. Sigmond could not desire a more satisfactory avowal. The most decided contagionists held no more. The *contingency* existed in the predisposition, and some few other circumstances which were generally admitted.

Mr. King rose to announce himself a contagionist; but he considered *all* diseases to be contagious as well as the cholera morbus. The honourable member then entered into a profound argument to prove that morbid matter existed in the body of every person who was sick, and therefore might be communicated to somebody else. Certain other remarks which fell from the honourable member induced

Mr. Hunt and **Mr. Douchez** to give their opinions on the communicability of typhus fever, and specific diseases, by contagion.

Mr. Searle being generally called for, rose, and expressed his willingness to communicate whatever he knew on the subject of cholera. He denied that any of the medical attendants on the sick, so far as he could know, and he had ample means of knowing (placed as he was at the head of one of the chief hospitals in Warsaw), ever suffered from the complaint. There was one apothecary's boy, indeed—a drunken, idle rascal – who fell a victim to it; but none of the physicians were attacked, and they were very numerous from all parts of Europe. It was not true that the members of the French medical commission were boys, as asserted in a certain periodical; they were mostly professors—and such indefatigable men he (Mr. S.) never saw. They performed a thousand experiments upon themselves; they inoculated their persons, tasted the excretions, &c.; they fed pigeons, dogs, and cats, upon the offal of the sick, inoculated them, &c., yet none of the consequences ever tended to prove that the disease was propagated by contact or infection. He dwelt considerably upon the fact of his own immunity, notwithstanding his perpetual and laborious attendance upon cholera patients, in all stages of the complaint; he had frequently been obliged to lie down on the hospital beds, debilitated with his labours—attending from an early hour in the morning until ten o'clock at night; nor could he always procure perfect ventilation and cleanliness: coming to the hospital sometimes unexpectedly, he found the windows close shut, and various things in the filthiest state. He spoke altogether from facts which had fallen under his own personal cognizance; yet he would decline to announce himself either a contagionist or non-contagionist—though the evidence, in his mind, certainly weighed in favour of the latter side of the question.

Dr. Granville, after expressing his satisfaction at Mr. Searle's communication, requested that gentleman to inform the Society further, what he knew relative to the battle which was said to have infected the Poles.

Mr. Searle explained, that the Polish troops had previously made a forced march of twelve German leagues, or about fifty English miles; they captured a large body of Russians, certainly—but they encamped in a marsh, and the mortality, even on the first night, was above fifty. He thought that too much stress was laid upon the circumstance of the hardships of the Poles. Why, he would be glad to know, was it always stated that the Poles were infected by the Russians on these occasions, and not the Russians by the Poles? He had heard it jocularly observed by a distinguished person in Poland, that this complaint of the cholera was one of the best things that had ever happened his countrymen.

Professor Burnett, though in common with the other members of the Society, he was much obliged to Mr. Searle for his kind communication, yet could not but confess himself unconvinced by that gentleman's remarks. He had been much more impressed, on the last night of meeting, by the able arguments of Dr. Macleod; who had shewn that, though many (perhaps the majority) of the individuals who had seen the disease in India, had in their reports subscribed themselves non-contagionists, yet the three great Boards had come to the opposite conclusion. Nor was the Bengal Board an exception; for though Mr. Jameson, the individual who drew up the report for that Board, professed himself a non-contagionist, yet, in the body of the report, he stated his opinion that, though the disease was not communicable from man to man, it was so from one body of men to another body of men! Then there was the disease travelling simultaneously northward by the Volga, and southward by the Don, and in the teeth of the winds; there was also the disease distinctly shut out of Teheran along with the caravan, and forced to go onward to a more distant locality. The fact, too, of the village which had been one half ravaged by the disease, and the other half preserved by a barricade, was very convincing. The learned Professor then proceeded to expose the quibbling of the non-contagionists, who were now driven to the admission of a modified or *contingent* contagion. He illustrated the nature of predisposition and infection by reference to the growth of seeds; and, in conclusion, dwelt upon the advantages of precaution: *Melius est sæpe cavere quam pati semel.*

Dr. Granville rose to reply to some of the arguments of Professor Burnett, and complained of the treatment which his (Dr. G.'s) party received from the contagionists. He particularly alluded to the *tranchante* manner in which the Medical Gazette of that very day disposed of the non-contagionists and their opinions. The learned Doctor now drew from his pocket a newspaper containing the propositions of Drs. Russell and Barry, relative to the contagiousness of cholera, and endeavoured to shew that these gentlemen thought exactly on the question as he did!—(Surprise and laughter).

Dr. Gregory moved an adjournment, in consequence of the lateness of the hour, and wished to know whether it would not be rather better to exchange the question of the communicability of the disease for that of its nature and treatment?

Dr. Copland was proceeding to make some remarks, when the meeting broke up, as we understood, with the intention of resuming the debate on Saturday next (this night).

The room, during the evening, was thronged with members and visitors: indeed we never recollect an attendance so full.

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

BY

Dr. Anthony Todd Thomson.

Lecture V.

November 10, 1831.

Epilepsy—Chorea—Lumbago.

Gentlemen,—There are two cases of epilepsy at present on the books of this establishment. The one, that of a girl, about fourteen years of age, of a melancholic temperament, with black, straight hair, and sallow complexion, in whom the disease has existed for several years; and although the frequency of the attacks has been much diminished by the means which have been employed, yet there is little hope of the disease being ultimately cured. Indeed, from the physiognomy of the patient, there is every reason to fear that the result of its continuance will be a state of fatuity. Tracing the history of this case, it appears " that the disease may have originated from a blow upon the occiput, which the girl received six years ago, as the epileptic paroxysm displayed itself soon afterwards, and has continued ever since, with short intermissions. In this case, it is probable that the injury done to the brain may have produced a predisposition to the disease, but as the seat of the mischief is beyond our reach, it is not likely to be removed. The second case is that of Arthur Leny, a green-grocer, who was admitted on the 27th of October: a case to which I have already twice alluded. I have stated that the most singular circumstance in the attack of this case, in which the epileptic paroxysm first displayed itself twenty-three years ago, is the long period which intervened between the first attack and the second, which did not occur till seven years afterwards. Since that time, until within two years ago, the patient suffered generally two or three attacks every year, which, however, were of a slight nature; but two years since they became more frequent, and for a few weeks prior to his application at the Dispensary, he seldom escaped having three or four attacks during the twenty-four hours. The remedies which have been employed, and to which I shall afterwards direct your attention, have been so far successful as to check the frequent recurrence of the fits, none having taken place since the day after he began the use of them.

In all cases of Epilepsy, there appears to be a peculiar predisposition of the person's system—an augmented susceptibility of impression in the brain, which may be denominated the *Epileptic* diathesis. Various circumstances, therefore, which in the ordinary states of the habit, produce little or no effect upon the nervous system, excite it so strongly in those labouring under this diathesis, that convulsions, and spasmodic action of various kinds, may result. In some individuals, particularly in females, hysteria is the consequence; in others, ordinary convulsions may take place; while in others, *Epilepsy* is produced. The usual causes to which Epilepsy is ascribed are either injuries done to the head, preternatural growths of bone from the skull, or other mechanical causes of irritation in the head. It is not, however, necessary that the irritation be confined to the brain: cases having occurred in which the disease has been traced to mechanical irritation on nerves out of the cranium. De Haen, for instance, has recorded a case in which it arose from a gangliform concretion adhering to the phrenic nerve. Dr. Short mentions one which was cured by the extraction of the irritant, a tumor, which pressed on the tibial nerve; and the Memoirs of the Medical Society of Geneva describe a fatal case, that evidently arose from an osseous body, about the size of a small nut, with a rough surface, which pressed on a branch of the sciatic nerve. Worms also, or other morbid irritants in the intestinal canal, acting by sympathy upon a highly-susceptible state of the brain, are exciting causes of Epilepsy. Sometimes, however, the exciting causes are of a mental kind, such as sudden grief, violent passions, or terror; or occasionally the attacks can be referred to the retrocession of disease; such, for example, as repelled gout, or cutaneous eruptions; and occasionally to uterine irritation, in which case they may be complicated with hysteria.

The variety of the disease to which I would refer the case under consideration, is that which is termed Cerebral Epilepsy; and I am disposed to regard it as the result, in this instance, of sudden gusts of passion—of a very fretful temper. I am induced to conclude that this is the fact, from strict inquiry into all the circumstances which might have induced this attack, and from having found nothing which would lead me to refer it to any mechanical irritation, or to repelled disease. I can only attribute it to some mental emotion; and the physiognomy of the individual indicates anything but a good-natured and calm disposition. The effect of mental causes in producing Epilepsy was known at a very early period, and obtained for the disease its Latin appellation—*Morbus Comitialis*—from the circumstance of its being by no means an unfrequent result of the high state of excitement produced by the harangues of the Roman orators, in their popular assemblies. By this man's account of himself, also, it appears that he has had much to fret him, and were it possible to trace it, we should probably find that the first paroxysm originated in a gust of boyish passion.

That the disease is *Cerebral* Epilepsy, is distinctly pointed out by some symptoms which seldom accompany any of the other varieties of Epilepsy: for instance, the constant winking, or nictitation, which still continues, although the fits are suspended; and the giddiness, which also still remains in the absence of the headache. During the fortnight that the paroxysms were of frequent daily occurrence, the patient complained of severe headache; an occasional loss of sight for some time after each paroxysm, and intolerance of the light of a candle or a fire.

These symptoms sufficiently point out the state of the cerebral organ, and with the predisposition which obviously exists, it is not difficult to comprehend that irregular nervous action and resulting convulsions would follow any mental emotion sufficiently violent, either to determine a sudden rush of blood to the head, or to produce sympathetic congestion in the brain.

We know that, in some states of apoplexy, depending on transitory fulness of vessels, epilepsy is not an unfrequent accompaniment; and in the direct ratio of the strength of the epileptic tendency, is much blood-letting unnecessary. Another reason for suspecting mental irritation, rather than permanent fulness of vessels, to have been the exciting cause in this instance, is inferred from the small frequent pulse and singing in the ears, which were present at the time the patient first applied for advice.

In none of the fits does he recollect having experienced any peculiar sensation which might be regarded as its harbinger; neither have the paroxysms kept any stated periods, or made their attack at any particular hour of the day.

In the treatment of the first case which I have mentioned, the only remedies that appeared to be productive of benefit were active purgatives and the introduction of an issue in the neck. The issue is still discharging, and when the bowels are freely open the recurrence of the paroxysms is comparatively distant. I am also inclined to attribute some advantage to the girl being kept in constant occupation, so as to divert, as much as possible, her mind from the contemplation of the disease. Before she applied to the Dispensary, the mother conceiving that her daughter was in a disease which required rest, allowed her to remain in a state of inactivity; but since she was informed that this was improper, and since the girl has been occupied in the ordinary duties of her station, the paroxysms have been less frequent and decidedly less severe. Previous to the introduction of the seton, all the usual means were employed without any obvious advantage. From the state of apathy in which she appeared to be, and from there being a manifest aura eleptica, dry cupping along the spine, stimulant embrocations, and powerful anti-spasmodics, both of a vegetable and mineral kind, were freely tried. It has generally been supposed, that we are to look for some local irritation in the part where the aura eliptica arises; but I have have had sufficient reason to think, that this symptom is as often an accompaniment of the disease, when it depends on general constitutional derangement, as on local irritation. My chief reason, however, for ordering the cupping along the spine, was an idea that there was disease existing there, as the limbs seemed often to give way, prior to the approach of the fit; and I knew, from the dissections of Mons. Esquirol, that affections of the spine, particularly softening of the cord, has been detected in post-mortem examinations of Epileptic cases. The hip-bath daily, and mercurial alteratives, were also employed for nearly two months, without any benefit; indeed, nothing seemed to check either the severity or frequency of the paroxysms except the purgatives, the seton, and the mental occupation.

In the treatment of the second case, although no permanent plethora manifestly existed, yet I considered it necessary to unload moderately the vessels of the head, by cupping behind the ears. Eight grains of Calomel were directed to be taken immediately afterwards, and to be followed by a brisk cathartic. On the 29th of October, two days after he had used these remedies, he had had no return of the epileptic fit; but from the feelings which he then described—the singing in the ears, the nictitation, and the sensation of wind being in the head—I was induced to continue the purgative system, not so much with a view to carry off any existing acrimony in the bowels themselves, as to set up in them a degree of counter-irritation, which might operate by lessening the irritability of the brain; and so far it appears to have been successful.

I would take this opportunity of remarking to you, gentlemen, that, not unfrequently, a course of cathartics alone has cured epilepsy; and although Dr. Hamilton, who revived the attention of the profession to the influence of Cathartics in Epilepsy, perhaps expected too much from their employment, yet the public owe him a debt of gratitude for expelling the prejudices against the employment of Cathartics, which prevailed for nearly a century before his time. You will find, in his work on purgatives, many cases illustrative of their powerful influence in the treatment both of this and several other nervous affections. The use of purgatives, however, in Epilepsy, is not of modern date; you will find it insisted upon by Celsus, who says—"Necessarium est, ducere alvum, vel nigro veratro purgare, vel utrumque facere, si vires patiuntur." The choice of the Cathartics in Epilepsy, is a matter of some importance: where we suspect the existence of mechanical irritation in the head, those which tend to unload the vessels of the system generally, and thereby to diminish the quantity of circulating fluid sent to the head, are the best adapted to fulfil the indication in view. Where worms are supposed to be present, as we generally find that they are either ascarides or tænia, perhaps the rectified oil of Turpentine, in large doses, should be preferred; and when, as in the present instance, our object is not so much to unload the bowels and improve the secretions as to produce a revellent effect, or to determine from the head, I prefer very moderate doses of Calomel in combination with Camphor and Henbane, given two or three times a-day, and, in the intervals, small doses of the common Senna mixture, with Wine of Colchicum and Camphor Julep. My object in prescribing the Calomel and Colchicum is to favour the free excretion of bile into the duodenum, so as to afford a general stimulus to the whole intestinal canal; whilst, at the same time, the Camphor and Henbane may moderate the general irritability of the nervous system. The Henbane has one advantage over all other narcotics—it does not confine the bowels; and there is no other which I would prefer, unless where the epilepsy has followed a repelled eruption; in which case, I should be disposed to employ the extract of Belladonna, from its known property of producing a scarlet efflorescence on the skin, owing to its peculiar action on the cutaneous capillaries. It is, however, but candour to say, that as I have never seen Epilepsy proceeding from such a cause, I have had no experience of the effects of Belladonna; and therefore my idea of its proving beneficial must be regarded as conjecture.

As no fits had returned on the 5th of November, and although the headache had completely disappeared, there was still giddiness and constant winking of the eyes, I was induced to order, in addition to his former medicines, moderate doses of the arsenical solution.

In no disease has a greater variety of tonics been tried than in Epilepsy; each has had its day, and the greater number have sunk into merited neglect. I may safely assure you, that you will derive no benefit whatsoever from the use of any of the vegetable tonics. On the contrary, many of them, in particular Bark and the salts of its alkoloids, appear rather to have done mischief. Indeed, in all cases in which the object of prescribing a tonic has been to allay irritability, and to diminish the spasmodic tendency, vegetable tonics have been found inefficient. Yet they are still used; and it is melancholy to observe that a credulity, worthy of the worst periods of superstition, continues to display itself in modern times, and that medical practitioners can be found weak enough to place confidence in the Misletoe and similar remedies. Almost all the metallic tonics have proved more or less beneficial; amongst these the preparations of Zinc, Copper, and Silver, have been most generally approved. As far, however, as my own experience authorizes me to judge, I do not place much confidence either in the Oxide or the Sulphate of Zinc, although in the use of both the doses have been carried to their highest extent. I have given six and eight grains of the Oxide, and fifteen of the Sulphate of Zinc, three times in the twenty-four hours, for many successive days, without any very perceptible advantage. As to the Salts of Copper, I have always regarded them with a suspicious eye, on account of their poisonous qualities, and therefore probably may not be able to give a fair opinion concerning them, as I have never carried either the Sulphate of Copper, or the Ammoniated Copper, beyond a very moderate dose.

Soon after Dr. Power made his experiments with the Nitrate of Silver in St. Bartholomew's Hospital, I was induced by his success to try that salt in two cases of Epilepsy; in one the dose was carried to the extent of six grains three times a day; in the other to eight grains; and to its use the disease yielded. But I confess that I have been prevented from employing it since by the dread of the known effect which it has in colouring permanently the skin; and I am of opinion the more effectual it is in small doses, the more likely is this discolouration to be produced. I can scarcely believe it possible, that any stomach can bear even six grains of the Nitrate of Silver for a dose, unless a considerable quantity of free Muriatic Acid, or of the Muriates, be present in it. Now, if this be the case, the greater part, or the whole, of the Nitrate, will necessarily be converted into an inert Muriate; and therefore only that which remains undecomposed can operate as an effective tonic. On the contrary, if a small dose produce a beneficial effect, there is reason for supposing that it has entered into the system; and, under such circumstances, we can have no certainty that it may not be decomposed in the rete mucosum, and permanently retained there as a Chloride, affording, from the action of light, and that of Sulphuretted Hydrogen, not unfrequently exhaled from the skin, the dark colour which it has unfortunately too often produced. I hold an opinion, that such an effect is likely always to take place, when the rete mucosum actually exists in the European; for it is there that colouring matters of an insoluble character, even when introduced mechanically under the skin, remain unabsorbed for life; and as the Chloride of Silver is insoluble in the animal juices, it will necessarily follow the same law when formed in that organ. How far the administration of diluted Nitric Acid, at the same time that the Nitrate of Silver is given, might prevent such an event, experience has not enabled me to determine. These being the reasons which have prevented me from prescribing some of the metallic salts, I prefer the Arsenical solution for two obvious reasons. In the first place, its influence in allaying irregular nervous action has been well established, in the treatment of intermittent fevers; secondly, because it has effected decidedly beneficial results in the Epileptic attacks of the delicate, and in worn out habits; in the drunken Epileptic when plethora is not present, and in many other spasmodic diseases not attended by plethora. It is not so likely to interfere with the purgatives as the Salts of Zinc or of Copper, and it does not accumulate in the habit, nor has it the disadvantage of colouring the skin, which may attend the administration of Nitrate of Silver. I do not wish you, however, to suppose that Arsenic more than any other metallic salt, is applicable to every case of Epilepsy; such an idea would mislead you. Much depends on the circumstances attending the case, and the constitution of the patient. In the present instance the spare habit of this man, and the absence of plethora, seem to indicate its use, even although the fits observed no regular periods. Our patient has already benefited by its use, and he will, I have no doubt, obtain a permanent cure.

You may, perhaps, gentlemen, be surprised that I have not noticed any of the direct Antispasmodics in mentioning the general treatment of Epilepsy; but I have never seen the smallest benefit accrue from the administration of any of them, except Musk, which is too expensive a remedy for a Dispensary. In one very severe and unmanageable case, of several years' continuance, which came under my care in private practice, I gave Musk, in gradually increased doses, to the extent of half a drachm, three times a day, with most decided benefit. The fits, which for several years had amounted to two or three daily, became gradually less frequent and less severe, until, the full dose of musk being given, and continued for some time, the fits were suspended for five months, and they might not, probably, have returned again had the patient lived, but he soon afterwards died of typhus fever.

As in the treatment of every other disease, Diet and Regimen require to be attended to in that of Epilepsy; the former should be moderate in quantity, not more than the stomach can fully master, of a nutritious quality, but light: all fermented liquors should be avoided. Exercise ought to be regular, but not carried to fatigue; and the mind and spirits should be maintained as far as possible in a cheerful and elastic state.

Chorea.

Since I last lectured, another case of nervous affection, *Chorea*, has been admitted. The patient, Elizabeth Brady, aged seven years, with light hair, and of a sanguine temperament, tall, and spare in her habit, was in perfect health until about a fortnight ago, when immediately after her return from school, the disease displayed itself. She complained that she could not sit still; there was constant jactitation of the arms and legs, and uninterrupted movement of the fingers, a rolling of the head, and twitchings of the muscles of the face, which continued even when she was in a recumbent position. She has scarcely slept at nights, not more than half an hour at a stretch; then generally awakes with a start, and the movements of the limbs and head immediately commence. She walks with great difficulty, the limbs twisting in various directions, so that she requires to be held up to prevent her from falling. Her speech is a little impaired, and her eyes have a very peculiar, rolling character. The bowels are open; the urine is high coloured; the pulse small, feeble, and quick; and the tongue slightly furred. The alvine evacuations are of a dirty, unnatural colour; and the perspiration has a peculiar offensive odour.

My first object in this case was to remove all acrimonious matter from the bowels, and to improve the character of the secretions. My second to restore the tone of the habit, and thereby overcome the spasmodic tendency. With these views, the patient was ordered a powder, containing five grains of Calomel, and twelve of Jalap, to be taken immediately, and repeated at the distance of eight hours, for three times successively; after which, she was directed to take ten grains of the Carbonate of Iron, and three of the powder of Conium, every third hour during the day; to observe a light diet; and to have friction applied along the spine and over the abdomen, as she was incapable of taking exercise. As the girl has not made her appearance again, I must reserve my remarks upon the case for our next lecture.

Rheumatism—Lumbago.

The cases of Smith and Sparrow have been discharged cured, and as West has not again paid us a visit, I have every reason to believe that he also is well. One case of severe Lumbago has been admitted since these cases, and the pain still continues with little abatement. The patient is an old man, with a broken-up constitution and of irregular habits: he has been cupped across the loins twice, blistered, and put under a course of purgatives, with Calomel, Colchicum, and Opium, and, latterly, he has taken also a Terebinthinate mixture, with large doses of the Tinctura Conii. The pains, however, continue. Although he has been strictly enjoined to observe regularity in diet, yet, I am apprehensive, that these orders are not obeyed, and to this circumstance, in part, may be ascribed the little benefit which treatment has produced in this case. It is one of many instances in which the influence of medicines, otherwise most effective, is completely counteracted by the abuse of dietetic stimulants. Such is also the case with the woman Eadwell, to whose case I formerly alluded as an illustration of that mixed form of rheumatism and gout, commonly called rheumatic gout As long as this lady remains sober, she improves; but as habit has confirmed her irregular tendencies, and her hours of sobriety are, "like angel visits, few and far between," I have little hope of being able to effect a cure in her case.

SANITARY HINTS RESPECTING CHOLERA.

Council Office, Whitehall, 14th Nov. 1831.

Sir,

The Central Board of Health having maturely weighed all the information which has been transmitted to them relative to the progress of the Asiatic Spasmodic Cholera in various parts of Europe, but more particularly guided by the conclusions on this head, to which Drs. Russell and Barry have arrived after a five months' careful and laborious observation of the character of that disease in those parts of Russia which they have visited, beg leave to suggest for your consideration the following sanitary hints :—

I.—*As to Precautionary Measures.*

In order to ensure the adoption and realize the benefit of any system of sanitary arrangements in a large community, the first essential point is to divide that community into subordinate sections, and to form District Boards of Health, each to consist, if possible, of a resident clergyman and a number of substantial householders, and of one medical man at least.

These Boards should be charged with the following duties in their respective districts, viz.:

1st, To appoint inspectors. Each inspector to visit daily, and to inquire carefully after the health, means of subsistence, cleanliness, and comfort, of the inmates of, say, (100 houses, more or less,) according to local circumstances.

2dly, To receive and examine the reports of these inspectors, which should be made up to a given hour on each day.

3dly, To endeavour to remedy, by every means which individual and public charitable exertion can supply, such deficiency as may be found to exist in their respective districts in the following primary elements of public health, viz. the food of the poor, clothing, bedding, ventilation, space, cleanliness, outlets for domestic filth, habits of temperance, prevention of panic.

4thly, To report to their principal Boards respectively on the above heads, as well as on the actual state of health of their districts.

The subordinate divisions of each district ought to be numbered or lettered, and each district named, the names of the members of each Board, or of the medical men attached to each, and of the visiting inspectors employed, should be placarded in conspicuous places.

Principal Boards of cities, towns, or parishes, to report directly to the Central Board in London:—

1st, On the actual state of health of their whole population.

2dly, On the precautionary measures already carried into effect.

3dly, On the measures contemplated.

4thly, On suspected sources, if any there be, from whence this particular disease might possibly spring.

With regard to precautions as to intercourse with suspected or really infected persons or places, the Board are confident that good sense and good feeling will not only point out, but morally establish, as far as may be practicable, the necessity of avoiding such communication as may endanger the lives of thousands.

But they strongly deprecate all measures of coercion for this purpose, which, when tried upon the Continent, invariably have been productive of evil. The best inducements to a prompt acknowledgment of the disease having entered a family, as well as to an early and voluntary separation of the sick from the healthy, will always be found in the readiness and efficiency with which public charitable institutions attend to the objects noticed in S. 3.

It is with much satisfaction that the Board feel themselves authorized to declare, and it will no doubt be highly consolatory to the public to learn, that under proper observances of cleanliness and ventilation, this disease seldom spreads in families, and rarely passes to those about the sick, under such favourable circumstances, unless they happen to be particularly predisposed.

It will not therefore be necessary, where there is space, and where due attention is paid to cleanliness and purity of air, to separate members of families actually affected by the disease, nor to insulate individual houses, unless in cases of crowded, filthy, badly-ventilated habitations, and other contingencies, which involve the health and safety of all.

It having been proved, by ample experience, in more than one city in Europe, that the fitting up and furnishing of hospitals for the reception of the poorer classes supposed likely to be attacked by the disease, at a period too long before its actual breaking out, has been productive of great waste of means, by the spoiling of various articles, and the consequent want of wholesome accommodation when most required, the Central Board would recommend that proper and sufficient house room only be secured and prepared in the first instance, and that the charitable be called upon only to *pledge* themselves to furnish at a given notice such articles of bedding, furniture, &c. or the value of them, as they would have at once contributed.

By this means the deterioration of perishable articles will be avoided, and should the district entirely escape, the contribution will be saved.

The situation which the Board would recommend for temporary Cholera hospitals would be, those most detached, insulated, and thoroughly exposed to free and open air; the description of house, such as would admit of the most perfect ventilation and cleanliness, and the largest space around the sick.

The Board would recommend, when a family is reported to be in an unhealthy state by the sub-inspector, and the disease confirmed to be Cholera by a medical member of the District Board, that the head of such family, if unable to afford proper accommodation at home, be advised to send the sick person forthwith to the temporary hospital, and that the other members of the family be supplied with such additional means and comforts as their state may require to enable them to resist the influence of the infected atmosphere in which they live.

II.—*Medical and Dietetic Precautions.*

These will be found of considerable importance, from their contributing to prevent or diminish the susceptibility to infection which individuals may possess at the moment the disease breaks out.

No sudden nor extensive alterations should be made in the usual modes of living. All changes of food, to be useful, indeed not to be absolutely prejudicial, should tend to render it drier, more nutritive and concentrated; moderately costive bowels, the almost invariable consequence of a dry, invigorating diet, will be found more conducive to exemption from cholera than an opposite habit.

Whenever aperients may become indispensable, those of a warm aromatic kind in moderate doses, or domestic means, should alone be resorted to.

What is generally understood by salts, viz. Glauber's salts and Epsom salts, as well as other cold purgatives, should not be taken *in any quantities*, nor on any account without the express prescription of a medical man.

The medical members of the Board beg to state, in the most decided manner, that no specific preventative against cholera is known to exist, and that the drugs hitherto offered with this pretension, in countries where the greatest ravages have been caused by this disease, not only did not possess the negative virtue of doing no harm, but were found to be absolutely injurious.

The true preventatives are a healthy body, and a cheerful, unruffled mind. Looseness of bowels should be immediately checked, and any thing like periodical chills or cold perspirations should be met by quinine in suitable doses; but habitual drugging, at all times *improper*, is to be deprecated in the strongest terms, when epidemic disease is apprehended.

The Board has been anxious to lay before the public, as early as possible, the above precautionary outlines, which they trust will tend, together with the suggestions emanating from the wisdom and observation of your and other local Boards, if not to exempt the whole population of these realms from the scourge of spasmodic cholera, at least to enable them to meet it, in the event of its appearing amongst them, with physical and moral constitutions the least likely to suffer from its virulence.

The Central Board will avail themselves of the earliest opportunity to transmit to you any further sanitary suggestions which may occur to them on the subject of precautionary measures, as well as an outline of instructions now in preparation for communities supposed to be actually attacked.

I have the honour to be, Sir,
Your most obedient servant,
E. STEWART, Chairman.

DRS. RUSSELL AND BARRY ON CHOLERA.

THE above named gentlemen, when at Calais, made a memorandum of the conclusions at which they had arrived, on a mature consideration of all they had witnessed abroad relating to Cholera. A copy of this has appeared in various newspapers, but containing several very important mistakes; the source from which we have received that which follows leaves no room to doubt its correctness.

"After having meditated on the above facts and documents from the moment they came to our knowledge—after having weighed them with all the attention of which our minds are capable, and after having compared the opinions which each of us separately, and without discussion, had grounded upon them, we find our impressions as to the mode of origin and spread of the late epidemic at St. Petersburgh and its neighbourhood, so perfectly identical in all important particulars, that we now agree to and sign the following propositions, containing the heads of our unanimous opinions on this part of the business of our mission:—

"1. That the germs of the disease were brought to St. Petersburgh by the boats and barks which arrived from the interior this year, previously to the 14th (26) of June.

"2. That those germs were diffused and the disease propagated in two ways; one which may be called personal, by the dispersion over the whole city immediately after their arrival, of several thousand passengers and boatmen, who had come from infected places, or had been exposed to infection on the passage or on board these vessels. The other, which may be termed atmospheric, by emanations from the barks, and their contents suspended in and carried by currents of air to susceptible persons, independently of direct communication.

"3. That the germs of the same disease were carried to Cronstadt, and propagated there by boats and lighters which had been loaded directly from the barks already mentioned, by persons who had recent communication with these barks, or had been in their immediate neighbourhood.

"4. That the disease was introduced into all the villages round St. Petersburgh, in which we have been able to obtain authentic intelligence of its progress, by persons directly from the city, or from other infected places.

"5. That neither the near approach nor the immediate contact of an infected individual were indispensable to the infection of a healthy individual susceptible of the disease at the moment.

"6. That the epidemic of St. Petersburgh did not possess those absolute and indiscriminating communicable qualities attached to the plague and small-pox, and that the risk of infection incurred by the healthy who approached the sick, was in direct proportion to the want of cleanliness, ventilation, and space, around the latter.

"7. That in a generally infected atmosphere the additional danger of infection incurred by approaching one or more individuals labouring under this disease was not greater than would accrue from approaching one or more typhus patients under similar circumstances.

"8. That under favourable circumstances of body and mind, personal seclusion did afford protection against the disease, more particularly if that seclusion had been accompanied by shelter from currents of air passing through sources of infection.

"9. That those continued exempt from the disease who retired from and avoided communication with infected places; and those who resided to windward of, and those who were protected from the currents of air passing through such places; that the next in point of immunity were those who, though living in the midst of general infection, avoided large accumulations of sick placed in confined atmospheres, the young, the vigorous, those who could afford to live well, yet lived temperately. In short, those who were placed under circumstances the most favourable to health, cheerfulness, and comfort of every kind.

"W. Russell, M.D.
"D. Barry. M.D."

DEBATE ON CHOLERA.

In consequence of the very crowded state of the room in Sackville Street, during the two last nights of meeting of the Westminster Medical Society, the Committee have summoned the members to meet, on Saturday next, in the Great Windmill-Street Museum; when the discussion on cholera will be resumed.

BOOKS RECEIVED FOR REVIEW.

An Essay on the Epidemic Cholera of India. By Reginald Orton, Surgeon, H. P. late of his Majesty's 34th Regiment of Foot. Second Edition, with a Supplement.

Observations on the Nature and Treatment of the Cholera Morbus, now prevailing epidemically in St. Petersburgh. By George William Lefevre, M.D.

A Transltion of the Eight Books of Aul. Corn. Celsus on Medicine. Second Edition, carefully revised and improved. By G. F. Collier, M.D. of the Academy of Leyden; of Magdalen Hall, Oxford; and of the Royal College of Physicians of London.

A. Cornelii Celsi de Re Medica Libri Octo. Ex recensione Leon. Targæ. Accedunt J. Rhodii, Dissertatio de Celsi Vita, Schillingii Questio de Celsi Ætate, L. Targæ Præfatio et Index Libb. MSS. Editorumque, excerpta de Balneis, de Ponderibus et Mensuris Romanis Monitum breve, cum Conspectu Capitum. Præfixis Characterum, Balnearum, Instrumentorumque Tabulis. Editio Secunda accuratissime emendata opera et studio Georgii Frederici Collier, ex aula Magd. Oxon, e Colleg. Reg. Medic. Lond., et ex Acad Lugd Bat. M. D.

ERRATA

In Dr. Elliotson's Lecture, published last week.

P. 169, line 2, after *body* insert *often*.

P. 170, line 23, for *false*, read *fallacious*; same page, line 31, for *minutiæ*, read *minuteness*.

P. 171, col 2, line 1, for *celer*, read *celeris*; line 3, same column, for *cœler*, read *celer*.

NOTICES.

We have received a copy of the *real* opening lecture delivered at the College of Surgeons in Ireland. We are astonished. We thank our correspondent, but need scarcely tell him that the thing is utterly unpublishable.

Dr. Elliotson's Lectures.—The arrangement by which the accuracy of these Lectures is secured, renders it impossible for us to comply with the request of several "Students" to insert more than one Lecture in each number.

W. Wilson, Printer, 57, Skinner-Street, London.

11, WATERLOO PLACE, 7TH NOV. 1831.

This Day is published, in One Volume, 8vo. Price 10*s.* 6*d.* bound in cloth,
With Maps illustrative of its Geographical Progress,

THE
HISTORY
OF THE
CONTAGIOUS CHOLERA;
WITH
FACTS
EXPLANATORY OF ITS ORIGIN AND LAWS,
AND OF
A RATIONAL METHOD OF CURE.

BY JAMES KENNEDY,
MEMBER OF THE ROYAL COLLEGE OF SURGEONS, LONDON.

CONTENTS.

SECTION I.—Early Impressions on Entering Bengal.—Climate, &c.—Origin of the Malignant Variety of Cholera, in the year 1817, ascribed to the Climatic Vicissitudes and Peculiarities of Bengal.—Propagation of the Disease through the Upper Provinces of Hindostan ascribed to Contagion.—Progress of the Contagious Cholera during the first Twelve Months of its Course.

SECTION II.—Abstracts taken from the Medical Reports compiled in India, by order of the Government.

SECTION III.—Medical Treatment of the Contagious Cholera.—Progress of the Disease exterior to Hindostan.—Laws of the Disease.—Evidence of the Existence of these Laws.—Quarantine.

APPENDIX.—General Description of the Symptoms of Cholera.—Varieties in particular Symptoms.—Diagnosis.—Sequelæ of Acute Cholera.—Appearances after Death.

CRITICAL NOTICES OF MR. KENNEDY'S WORK.

"We regret that Kennedy's recent work, 'History of Cholera,' did not reach us in time to be included in our review of that subject. It is calculated to be more extensively popular and useful than any on our list."—*Quarterly Review, Nov.* 1831.

"A most admirable performance."—*Literary Gazette, Nov.* 5, 1831.

"Mr. Kennedy, who observed and treated the disease in Bengal, has given a vast mass of information as to its progress, symptoms, and treatment."—*Examiner.*

"There is no publication which we can recommend, either to the professional or general reader, with greater confidence than Mr. Kennedy's."—*Scots Times.*

"Mr. Kennedy's book is the best work published on the subject, and with the experience of the author, worth all the published reports of the College of Physicians, and the College to boot."—*Metropolitan Magazine.*

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"Did we permit another number of this journal to issue from the press without seizing the occasion to place Mr. Kennedy's work in the most prominent position before our readers, we should be guilty of flagrant injustice both to them and to that talented author; for in whatever light we regard it, we consider his volume of greater public value than any other which this fertile subject has yet produced. The part of the work on the treatment of Cholera deserves the highest praise. Indeed, in the event of the arrival of Cholera amongst us, the practitioner who neglects to study this part of Mr. Kennedy's work, neither deserves the confidence of his patients, nor the approbation of his own conscience."—*Lancet, Nov.* 5, 1831.

London:

THE
LONDON MEDICAL GAZETTE,
BEING A
WEEKLY JOURNAL
OF
Medicine and the Collateral Sciences.

SATURDAY, NOVEMBER 26, 1831.

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LECTURES
ON
THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

By Dr. Elliotson.

Part I.—Lecture VIII.

Inflammation (continued.)—Pain—Morbid Appearances—Divisions—Common Inflammation—Symptoms.

In the last lecture, gentlemen, I commenced the consideration of special pathology—the consideration of particular diseases; and I stated that I should first speak of those diseases which attack various parts of the body,—that I should then consider such diseases, as seated in particular parts, and consider also those diseases which do not attack various parts, but are confined each to peculiar situations.

I therefore began the consideration of general diseases, if I may so speak; and, as the most important, the most common, and the most connected with all other affections, I commenced with inflammation. I stated that inflammation was characterized, generally speaking, by four principal signs—redness, swelling, heat, and pain. I considered, as far as appeared necessary, the three first of these; and, at the conclusion of the lecture, I was speaking of the fourth—namely,

Pain.

I stated that pain, like two of the other symptoms of inflammation, might arise from other causes than inflammation — that is, swelling and heat may exist independently of inflammation, and so may pain. Pain frequently arises from spasm, from scirrhus and cancer, or from neuralgia; in the latter of which it depends upon a peculiar state of the nerves, not necessarily inflammatory. But I stated that pain was a very common attendant upon inflammation, though not invariably present.

The pain of inflammation is generally increased by pressure, at least by sudden pressure, whereas pain arising from spasm is generally relieved by pressure. You have this very strikingly shewn in colic and in enteritis. In the latter, the pain, of course, arises from inflammation; if you press the abdomen, even with the point of one finger, the patient experiences exquisite pain; whereas, in the former, which is a mere spasm of the intestines, the more you press the more is the patient relieved; so that I have found a patient able to bear the pressure of both hands, with my feet almost raised from the ground, standing on tiptoe, and the more I pressed the greater was the relief: but the two may be connected, and then you have a certain portion of relief and a certain portion of aggravation.

However, it is necessary to say, that although the pain of inflammation is increased by pressure, it is rather by this being *sudden* than gradually increased; for frequently, if you press an inflamed part very slightly, and gradually augment the amount, very considerable pressure can be borne, provided you press the *whole* part. If you press only one spot, even slowly, you are sure to give pain; but if you can embrace the whole part, and press it all gradually, so as to empty it of its blood, you will frequently produce ease. This is shewn every day in the case of inflammation at the bottom of the foot. If you have a blister upon the sole of the foot, or at the ball of the great toe, and you rest gradually upon the part, the pain becomes mitigated, till at last it seems to be almost entirely removed; but the moment you take off the pressure and raise the foot from the ground, you feel the part begin to throb—to throb with violent pain. It appears, therefore, that if an inflamed part be pressed suddenly it is sure to have its pain increased, if it be pressed even gradually, but only partially, you generally produce the same effect, because, if you press only one part, the other becomes more gorged with blood; but if you can press the whole together, so as gradually to empty all the vessels, or diminish their contents, then pressure can be borne, and, more than that, it frequently gives relief.

Hence pressure has been recommended as a means of cure, by certain writers, in some inflammatory diseases. Pressure has been recommended for the cure of gout and rheumatism, by Dr. Balfour, of Edinburgh; and a French surgeon (Velpeau) has recommended it in erysipelas. But it frequently happens that, although relief may be given in some cases, considerable aggravation is excited in others. It is necessary that the part should be all compressed—that the pressure should be equable; for any deviation in this respect, so that a part of the vessels were allowed to become the more distended, would increase the mischief. Very frequently, too, inflammation does not depend altogether upon local causes; there is a tendency to it in the constitution, and although you impede the circulation of the part, you do not stop the impetus of blood running to it; the large arteries around are found to throb violently against the part compressed, and pressure frequently cannot be borne at all. On all these accounts, it is exceedingly difficult to cure inflammation by pressure. As a general rule, however, we may say that the pain of spasm is diminished by pressure, and the pain of inflammation increased by it; and this criterion is continually resorted to as a means of diagnosis—as a means of distinguishing whether pain is inflammatory or spasmodic.

Pain is, generally speaking, greater in proportion to the tension of the part attacked. The less a part will yield, the greater generally is the pain. Hence much inflammation of a theca of a tendon is generally attended with great agony, and from a similar circumstance, when matter is formed, if it cannot escape, from the part not yielding at all, the pain is most agonizing; whereas, by relieving the tension in making an incision, so as to allow the escape of even the smallest quantity of matter, and the yielding of the part, the pain instantly disappears; and more than that, the great irritation of the system, which, perhaps, amounted to a violent delirium, also vanishes. All these things frequently cease, as it were, by magic, when an incision is made. Hence, too, the great use of incisions in certain inflammations of the skin and the cellular membrane. In what is called *phlegmonous erysipelas*, the cellular membrane becomes excessively gorged with blood—excessively gorged with fluids—the skin is distended, and the greatest agony is felt; but a few incisions will put a stop to all this. I presume it is for the same reason, that, in inflammation of a serous membrane, you have far more violent pain than in inflammation of a mucous membrane. These serous membranes in general are tense, and give the most violent pain when they are inflamed; whereas the mucous membranes are all of a yielding character, and inflammation of them is consequently never attended by a violent stabbing pain. You know that serous membranes are generally spread out pretty tightly, at least a considerable portion of them, and therefore you generally have a violent stabbing pain when they are inflamed; but sometimes this is not the case, and then the less degree of pain may arise from the affection being seated in a portion that will yield.

But pain, according to its situation in various parts, may be sharp, dull, smarting, or burning. Sometimes it is a mere soreness—no pain is felt unless the part is irritated mechanically, or some particular stimuli are applied. Parts which have little or no sensibility in health, acquire sensibility under inflammation, and frequently very intense sensibility. Many parts which may be cut in health without the individual, be he man or brute, experiencing any sensation, when inflamed cannot bear the slightest motion or the slightest pressure. If, therefore, parts which show no sensibility in health become very sensible in inflammation, you may suppose that parts which are naturally sensible in health may become very painful when in a state of inflammation. However, the degree of pain will not be commensurate with the degree of original sensibility, because it depends so much likewise upon the tension or the yielding character of the part. If it so happen that a part which has no sensibility, or but little, but which is nevertheless an unyielding part, become inflamed, the pain will be dreadful; more dreadful than it would be in a part which naturally has sensibility and yet is of a yielding character.

Diagnosis.—Inflammation is generally known by pain increased on pressure, by feverishness, and a disturbance of the function of the part. When we see a person labouring under disordered function, and labouring under feverishness—pyrexia—and under pain, which pain is increased by pressure, although we cannot see the part, we have no doubt that it is inflamed—that the proximate cause of these symptoms is inflammation. Sometimes there is no pain experienced, but simply disturbance of function and feverishness; and when that is the case, we are not sure that there is an internal inflammation. When a part can be seen, the symptoms which I have already mentioned, few or more of them, will be sufficient to characterize the complaint; but we are certain also of the existence of inflammation when the part is invisible, when we cannot ascertain whether it is red, hot, or swollen or not; but where there is pain increased on pressure, the function of the part disturbed, and the patient labouring under what is called "feverishness, or pyrexia."

Morbid Appearances.

These are the four symptoms of inflammation during life, but they may have existed at that period and none of them remain after death. The pain and the heat, of course, must go, but the redness and swelling you might expect to remain. When, however, we have seen a part inflamed during life, or such symptoms have been present as to leave no doubt of the existence of inflammation, in both cases you may find, on examination after death, no marks of inflammation at all; and yet the absence of these does not in any way invalidate the opinion which you formed during life; for it will happen that after death every mark of inflammation will disappear. You may sometimes, in inflammation of the surface of the body, or of the eyes, visible enough during life, and of which the patient died, find both parts pale after death; the skin, perhaps, will be slightly swelled, but the swelling will be greatly diminished, and the redness entirely gone. This is a certain fact respecting inflammation in general, because it is observed with regard to external inflammation. After apoplexy you will frequently find nothing. Although during life the head appears to be bursting from an accumulation of blood, and the patient has died of the complaint, yet you will frequently find after death no morbid appearances. All the vessels, when the temperature of the body cools, recover themselves, or the blood in some way or other goes to other parts; but this occurrence does not always take place. You will sometimes find that where there was no doubt from the symptoms that internal inflammation existed, the marks of the complaint disappear after death; sometimes they do, but it is by no means general.

However, on the other hand, you must recollect that it is not every preternatural redness that you discover after death which is inflammatory. You may find an intense redness of the internal parts in the bodies of patients, whom during life you did not suspect to be labouring under inflammation, and you are not on that account *alone* to suppose that they did; for just as the redness of inflammation will frequently disappear after death, either in a great degree or altogether, so after death, without any previous inflammation, you may have morbid redness of the parts. If the patient have died with great difficulty of breathing, so that a large accumulation of blood has taken place in the lungs, you may find the liver gorged with blood, and you may find the mucous membrane of the stomach and intestines exceedingly red, not from inflammation, but simply because the blood was obstructed in the lungs, or in the heart, and consequently congestion took place. In a case of great debility, a continuance of the patient in one posture during the latter period of his existence will cause such an accumulation of blood as, after death, to give rise to great redness; and these appearances are always greater in proportion to the number of capillary vessels in the part to which the blood inclines by its gravitation. The blood will not merely accumulate in the vessels in this way, but it will transude from them so as to dye the surrounding parts perfectly red, and prevent your distinguishing the appearance from inflammation. This is particularly the case in hot climates and in hot weather. Dr. Davy, the brother of Sir Humphry, says, in a paper published in the tenth volume of the Medico-Chirurgical Transactions, that he has found it in hot countries often impossible to distinguish the redness of inflammation from the redness of transudation, or the redness from the circumstance of a piece of membrane being merely steeped in blood; and from all his observations and experiments he draws the conclusion, that if in a hot climate a body be not opened within twenty-four hours after death, the surfaces become red; that the serum of the cavities likewise becomes bloody, and the viscera livid, so that no opinion can be formed as to what was the state of the parts during life. The reason is this—the hotter the temperature within certain limits, the sooner does decomposition take place; the more a part is decomposed, of course the more easily does transudation occur. The solids become more soft, more spongy, and blood which comes in contact with them easily pervades them, oozes through them, and dyes them and any of the liquids which the parts may happen to contain.

You notice in dead bodies which have lain any time upon their back, especially in hot weather, that the posterior parts all become livid, and filled with blood, while the anterior parts are pale. The body being placed usually on the back, if you open it and examine the lungs, you find their posterior part heavy, filled with blood, livid—in fact exhibiting exactly the same appearance as the anterior part does in violent bronchitis; and every one accustomed to make post-mortem examinations allows for this occurrence; he never thinks of considering it a morbid appearance; it is simply the effect of the blood gravitating to the lowest parts. You will observe also when bodies are at all decomposed, that red streaks appear along the surface, in the direction of the various veins. The blood accumulates in all the veins after death, and as the substance of the coats becomes more and more decomposed, it allows the blood contained within the veins to transude, and in that way the coats themselves become thoroughly dyed with blood, and the cellular membrane around the vessels becomes dyed likewise; you may trace the superficial veins in the dead body from this appearance. This is nothing more than a transudation of the blood, through a degree of decomposition having occurred.

The observations made by Dr. Davy many years ago, respecting the importance of examining bodies early in hot weather, and shewing the importance of knowing that such appearances as are induced by inflammation may take place merely from decomposition, have been all lately confirmed by Andral, who, to make his observations the more striking, has examined the same part of the body at different periods. Early after death he has found an organ pale; but if it were afterwards so placed as to favour the gravitation of the blood to a certain portion of it, that part of the organ, when subsequently examined in a few days, decomposition having proceeded, had became intensely red.

The blood will not only accumulate in the solids in this manner, and not only exude in sufficient quantity to dye any liquids that may be within the cavities, but it will itself exude to such an extent, that a canal may be found containing a large amount of pure blood. By causing a considerable portion of intestine to be dependent, the blood has gravitated to such an extent as to pass through the vessel into the cavity, and there lie in the form of a hemorrhage. Two French writers have made numerous experiments upon this subject, Rigot and Trousseau; and they say that they have frequently made the blood ooze from the inner surface of the intestines, by merely giving a portion of them this dependent position.

You must therefore be aware, that the mere appearance of redness is not sufficient to justify you in saying that a part has been inflamed. A part may be red because it was inflamed during life, but you must always take into consideration, whether the body has lain long, so as to be partly decomposed, and allow the diffusion of blood into the substance of the parts. Indeed, a degree of putrefaction not merely gives this infiltration of blood into the parts, but the parts themselves become dissolved into a liquid substance — they liquify. As the body putrifies it becomes soft, and a large quantity of liquid is produced, in addition to the pure blood, which assists materially in dyeing the different parts. You have therefore to take into consideration what is the period at which you examine the body, and also what is the position of the part, whether it is such as is likely to have favoured the gravitation of its own blood, and that of other parts, in any quantity. You have also to take into consideration another circumstance, independent of time and of position—that is, whether any mechanical obstruction existed during life. You may examine a body immediately after death, so that putrefaction has not occurred, and placed upon its back with the stomach high, so that the blood could not have gravitated there, and you might see such redness of that organ that you would consider it inflammatory, if you did not also know or ascertain that there had been a mechanical obstruction. If there have been a mechanical obstruction to the return of the blood through the lungs and heart, then that will explain redness of the stomach without your being under the necessity of referring it to inflammation. There are therefore three circumstances to be taken into account—the period at which you examine the body, the situation of the part as to gravitation, and the previous existence or non-existence of mechanical obstruction. A simple inspection of the parts which are red, provided there is nothing but redness, will give you no information whatever. You must ascertain whether there were any symptoms of inflammation during life, and observe the three circumstances to which I have just alluded.

There is, however, another circumstance which will enable you frequently to say that inflammation actually existed, and that is the presence of the products of inflammation. If you find a part intensely red, and you see a layer of lymph effused, or you see a large quantity of serum—perhaps turbid serum—perhaps, too, with flakes, then you may be sure almost that the redness is inflammatory. The redness, it is true, may be increased by decomposition—may be increased by mechanical circumstances; but if you see the products of inflammation, then you may fairly conclude that the redness, either altogether, or the greater part of it, is inflammatory. These observations you will find of use with respect to inflammation in the inner surface of the heart. The lining membrane of the cavity of the heart, especially at the valves, is sometimes very red without any inflammation. Some persons will say there was inflammation during life; others will say that there was not. Of course the point generally may be cleared up by minute observation—by feeling whether the heart is soft or not, so as to allow the membrane to be stained by transuded blood; but if you see an effusion of lymph upon the membrane, then you may be sure that the redness is of an inflammatory character—that it is the result of inflammation, either in part or altogether.

These are the observations that I have thought it necessary to make, gentlemen, respecting the marks of inflammation during life and after death.

Divisions.

Inflammation is usually divided into two kinds—the one *common*, the other *spe-*

cific. "Common" inflammation is that which we see every day where a local injury takes place, or when a part has been exposed to the vicissitudes of temperature; but the epithet, "specific," is given to inflammation when it runs a peculiar course, or when it arises from a peculiar cause, neither mechanical, nor simply irritative. When inflammation runs a peculiar course, it is then by many called "specific," but more frequently it receives that appellation only when it arises from a peculiar cause. For example, inflammation of the urethra, from the forcible introduction of a bougie, or a catheter, would be common inflammation; but the inflammation of gonorrhœa, arising from a peculiar cause, is termed specific inflammation. In specific inflammation you have all the symptoms which occur in common inflammation, but some peculiarity is superadded. Common inflammation is the groundwork, but in specific inflammation some other circumstance is superadded.

Common Inflammation.

I will now speak of the course of common inflammation. Common inflammation may begin with pain, with a blush, and with a feeling of unusual warmth. When an inflammation is first set up, it may begin in this way, but the redness is sometimes not in the form of a blush, but occurs in one particular point, and from that it spreads. If the part be a secreting organ, its secretion will become changed in its appearance—perhaps changed, likewise, in its smell, and at the same time it is generally increased in its quantity. The pain and the redness increase in degree, and also in extent, and then a swelling occurs, or tension, or swelling and tension. The parts swell to a certain point; and if they cannot swell farther without difficulty, they become tense. As the inflammation proceeds, the secretion of the part generally diminishes again, though perhaps it does not come down to, or does not descend below, its natural amount; but the secretions still remain unhealthy in appearance, and, perhaps, also in smell. As all this goes on, or when it arrives at a certain point, the constitution begins to feel its effects. The pulse becomes quickened; and it likewise becomes variously altered in its volume, firmness, strength, and regularity. Thus the constitution sympathizes at last. The affection of the pulse is not always according to the danger, for in mere inflammation of the tonsils, or in mere rheumatism of the joints, both of which are unattended by danger, you will sometimes find the pulse as rapid and as violent, or perhaps more so, than in many dangerous inflammations. The same remark may be made with regard to the temperature. The temperature of the whole body will sometimes be greatly elevated, although the inflammation may be one of no danger. In a case of mere acute lumbago, from which a man very speedily recovered without any bleeding—merely by the exhibition of colchicum, I found the pulse at 160, and the heat of his whole body 107°. I find, in a note which I made, that he required no blood letting at all, and that he merely took vinum colchici for three or four days, yet that his pulse was 160; and by the thermometer employed by myself, and placed in various parts of the body—under the tongue, and in the axilla, the temperature was not less than 107°. The heat, too, frequently varies in different parts of the body. Besides the alteration of the pulse and the temperature, the patient becomes restless, uneasy in any position, and complains of general soreness; the whole surface of the body frequently becomes sore—the patient feels tender, he says, all over. The head frequently becomes painful, and likewise the loins; and sometimes the pain in these two parts are among the severest constitutional symptoms. The constitutional derangement extends to the secretions, so that most of them become diminished and altered, and from this circumstance you will have, in the first place, thirst. The tongue will be dry or white; and sometimes, if there be much gastric disturbance, it is yellow, or if little strength, dark. The skin becomes dry, though in one particular inflammation—active rheumatism—it generally falls into profuse sweating. The urine becomes scanty; and though it remains clear it is high coloured, and has a strong animal smell—it smells something like gravy soup. The bowels become torpid, and the fæces unhealthy, both in colour and smell. These are all instances of diminished or altered secretion.

Now these constitutional symptoms altogether are called by Cullen and his followers *pyrexia*—not fever, but pyrexia—fever being the name given by them to certain distinct specific diseases. Some persons denominate these symptoms fever, under all circumstances. We have no English word for this term, pyrexia, except we use *feverishness*, but *ish* implies only a slight degree of any thing; whereas the pyrexia may be very great, and therefore the term "pyrexia" is always better. The word is used by Cullen to distinguish this constitutional excitement under all circumstances, from those specific diseases, which he calls, whether remittent, intermittent, or continued, *fever*; to distinguish it from fever properly so called. However, some persons, aware of the confusion which might arise from calling both these symptoms "fever," and other specific fevers, such as typhus, also *fever*, denominate these symptoms when they are dependent upon inflammation, or other local cause—*symptomatic;* and they call the others—fevers, properly so termed—*idiopathic* fevers—fevers dependent solely upon themselves—grounded by themselves. You will, therefore, find these symptoms, which are sometimes called "constitutional excitement," "constitutional derangement," denominated by some writers, "symptomatic fever," to distinguish them from *real* and specific fever, which they call "idiopathic;" but you find in Cullen the word fever not given to this state in general, but that he uses the word "pyrexia." For the purposes of clearness, it can make no difference whether, in the case of inflammation, we say pyrexia, or symptomatic fever; for all that is necessary is, that we should make ourselves understood.

Symptoms.

Now it very frequently happens, that when the local disease first begins, before it has increased to such an amount as to quicken the pulse, and induce the other symptoms which I have mentioned, the person complains of chilliness, a general soreness, and even rigors, so that he shivers with cold; the pulse at this time being small, the face pale, and the skin rough—from its deprivation of blood, and its low temperature. it is rough, and is called *goose's skin—cutis anserina*, or this state is called *horripilatio*. After the existence of these symptoms—this cold stage, for a certain time—the inflammation appears, and the excitement of the whole constitution gradually begins. Sometimes, therefore, inflammation begins, and after a certain period is attended with constitutional excitement; but sometimes, the moment inflammation begins, the system falls into a cold stage, like fever—exactly such as happens in the cold stage of an intermittent, so that the local inflammation appears simultaneously with the constitutional symptoms.

The general symptoms are usually proportionate to the intensity and the extent of the local inflammation, and likewise to the importance and sympathies of the organ affected; and sometimes, as I have already mentioned, the general symptoms are out of all proportion in their violence, so that if you depended upon them, and did not ascertain the state of the part affected, and recollect its nature, you might suppose the patient was in the greatest danger, when that was not the case. If you found an individual with a temperature of 107°, and a pulse of 160, you would suppose him in imminent peril, if you did not ascertain that the inflammation was inconsiderable, or was seated in a part not at all necessary to life—not at all of importance to the system; whereas, the same derangement attending inflammation of an important organ, or attending a very extensive inflammation of another organ even not so important to life, would point out the greatest danger. You see, therefore, from all I have stated relative to the colour of parts after death, and the excitement of the system in inflammation, a proof that what I before remarked is well founded, that we are not to depend upon one symptom—one circumstance—in a case, generally speaking, but we must always take into view as many particulars as we can ascertain.

Besides these local and these general symptoms, the functions of the part inflamed are disturbed. This I have already mentioned so far as the secreting organs are affected, but other organs are likewise disturbed. If it be the brain that is inflamed, you have delirium. If it be the lungs that are affected, you have dyspnœa. If it be the stomach, you have vomiting. If it be the intestines, you have either constipation or diarrhœa. If it be the bladder, you of course have a frequent and painful desire to make water.

Buffy Coat.—It is not, however, merely these disturbances — these changes — that take place. The blood itself becomes changed in inflammation. The red particles separate so fully from the fibrin, that this fibrin generally remains colourless and transparent at the top, and, when this occurs, it is said to have a *buffy coat*. There must some change take place in the blood for this phenomenon to occur; and, as soon as you remove a portion of blood from the body, you find that some alteration has taken place, for such are the effects. Occasionally the fibrin, white and clear above from the red particles below, is drawn into such an excavated form, its centre so depressed, that it resembles a cup, and such blood is called *cupped*. Blood may be buffy without being cupped; the red particles may separate entirely from the fibrin, and leave a buffy coat; but sometimes, in addition to this, the fibrin is drawn into an excavated form, so that it is buffed as well as cupped.

Occasionally, this buffy appearance of the blood takes place, it is said, without inflammation. It is usual in pregnancy, but then really a pregnant woman is in an inflammatory state; it is not a state of actual inflammation, but the condition of the womb is really inflammatory. You see that the whole body frequently in pregnancy wastes; the eyes become hollow, and the whole health, for the most part, suffers a little. The pulse becomes very quick—quicker than in health, and all the blood-vessels of the womb become greatly enlarged. The womb itself, if it be not in a state which justifies us in calling it inflammatory, is, nevertheless, in a state of such great activity, that it comes as near to inflammation as possible. And, in truth, with regard to other periods of generation, when animals are in heat, and copulation takes place, the genital organs become violently red, violently hot, perhaps blacken with blood, as if they would burst, and all the blood-vessels around throb. If that state be not to be called inflammation, still it is very clear that it is all but so, and whatever word you choose to employ, you must look at it as physiologists and pathologists. We cannot therefore wonder, that, in pregnancy, the blood should frequently be buffy.

But this state of the blood is sometimes absent in inflammation. You will find a patient undoubtedly labouring under a most violent inflammation, and on using the remedies for that affection, you produce the most decided effects, and cure him, and yet no buff may be present in the blood. This is by no means uncommon in bronchitis, where a patient has a rapid pulse, great heat of body, cannot turn in bed without coughing, suffers under a difficulty of breathing, so that he is obliged to breathe in a shallow manner; cannot expand the lungs without extreme soreness, tightness, and extreme cough; the tongue is white, and violent pyrexia is present; and if you bleed, you produce instant relief to the patient, so that both the symptoms and the treatment prove the existence of inflammation. The blood will not in bronchitis in many, *many* cases, show the slightest morbid appearance; whereas, if it were another membrane that was inflamed, instead of the lining membrane of the air tubes, as, for instance, the pleura, the smallest quantity of blood that you could take away would most likely show an intensity of buff, and be cupped in addition. The absence of this buffiness of the blood is by no means a proof that there is not inflammation; but, if it be present, it is generally in proportion to the violence of the inflammation. If you be in doubt as to whether the patient is labouring under inflammation or not, the appearance of this buffiness might justify you in concluding, that the symptoms which you suspected to be inflammatory really were so. The absence of it is no proof at all that inflammation does not exist; but the presence of it will, in doubtful cases, very much confirm your conclusions, that the symptoms you were treating were inflammatory. The buffiness, however, is not necessarily in proportion to the *danger* of the inflammation, because in acute rheumatism, where there is no danger whatever—sometimes there is danger in acute rheumatism from internal inflammation, but in a case where there is no danger whatever—the blood will generally be intensely buffed, and not unfrequently intensely cupped at the same time. You are, therefore, usually to consider this as a mark of inflammation, or an inflammatory state; but its absence will not warrant you in saying that there is no inflammation; and the intensity of the buffy or cupped state is not to be considered by you as a proof that the inflammation is of an exceedingly dangerous character[*].

ON THE

CEREALEAN ORIGIN

OF

CHOLERA.

BY DR. HAYCRAFT.

IT seems rather unaccountable, that amidst the disputes concerning the causes of cholera, that the influence of a vitiated diet, in producing the disease, has not of late years been discussed. Contagion or some peculiar state of the air, or both of them combined, have been assigned as the only probable causes.

That improper diet has been a predisposing cause of cholera in this country as well as in Germany, and also of the epidemic cholera of India, has been allowed by Sydenham, Richter, Sprengel, Rankeen, and many others; and that it usually begins at, or soon after, the harvest period in each country respectively, is also stated by most of these writers. This, added to the consideration that cholera, in its various forms, is essentially a disease affecting the stomach and bowels, and always begins with gastric and splachnic symptoms, would lead us to suppose, that whatever the exciting causes may be, the predisposing cause might consist in the use of food made from corn in its too recent state, or otherwise unfit for the human constitution.

Dr. Rankeen, who was with the army on the Bengal station, in 1818, (see Edin. Med. and Sur. Journ. Jan. 1823,) ascribed the epidemic of the time "chiefly to a diet of rice or other grain vitiated by the wet of the season:" to this Dr. Mason Good, I think without foundation, objects, "that the grain and other food of such wet seasons is usually the product of the year before, which may have been peculiarly dry and healthy."—(See Study of Med. vol. i. p. 281.) From inquiries I have made of gentlemen well qualified to give an opinion, I find that this objection will not hold good, at least in this country, and probably not in any other. I am informed, that as early as the month of August it is the custom in London to use one part of recent to two parts of old wheat[*], and that in some parts of the country the bread is occasionally made entirely of the most recent wheat, namely, that which a week or ten days previously had been standing in the field. Now this occurs in the month of August, which Sydenham* affirms, and which every body knows, is the month in which cholera in this country is peculiarly rife.

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[*] In Dr. Elliotson's lecture published in our No. for Nov. 12, p. 173, col. 2, l. 62, *for* "medicine," *read* "médecine."

[*] Unless old is mixed with the new wheat, the bread will not "rise," as it is called, occasioned, I suppose, from the panary fermentation being imperfect.

Sprengel† and Richter‡ both accuse a bad quality of diet as a cause of cholera; the latter states, that it prevails in Germany from harvest till August and September, and also that the epidemic cholera of children, in North America, is most frequently about the harvest months.

The Russian cholera of 1829 also made its first appearance in the month of August, or about the harvest period, and according to the statements of Dr. Onufriev, physician for the circle of Orenburg, it appears that during the prevalence of the epidemic, there was scarcely a single inhabitant who had not some symptom of disordered digestion. This was ascribed by some of the physicians to a peculiar epidemic state of the human constitution; others ascribed it to an unusual abundance of fruits, especially of gourds and water melons; but we have too little information on the subject to determine if these universally prevailing symptoms of indigestion were occasioned by some general cause, independent of the state of the air, namely, a vitiated diet. That improper diet, or some general depravity of the usual articles of food, was in fault, is probable; for, we are informed by Dr. Hawkins, in his history of the Epidemic Spasmodic Cholera of Russia, and also by others, that the disease has made its principal ravages among the poorer inhabitants, who "obtain a scanty, and often improper nourishment;" and the Warsaw Committee of Health reports, " that few persons of easy condition have been ill, and that the disease has expended itself chiefly on the poorer inhabitants of that low and thickly peopled city." Indeed, almost all the writers on cholera who have observed the disease, have stated that it attacks chiefly the poorer classes, and those that are badly fed.

The epidemic of 1830 first appeared in June at Rescht, lat. 37° 18′ N. a town in Ghilan, in Persia, situated on the southern coast of the Caspian Sea: it then appeared at Baku, lat. 40° 2′ N. early in July, and on the 27th July was observed at Teflis, lat. 43° 20′; and about the same time, namely, the 19th July, appeared at Astracan, lat. 46° 22′ N. From Astracan it arrived at Tgaritza, lat. 48° N. on the 4th August. On the 6th of the same month it appeared at Saratof, lat. 52° 4′ N.; and at Penza, still more to the north, it appeared on the 17th August. "About the end of August" it reached as far north as Nisen or Nischenei-Novogorod, lat. 56° 34′ N.; and from this place it branched off in two directions, namely, to the east and by south to Casan, lat. 55° 43′ 58″, where it appeared on the 9th September, and also to the west and by south to Moscow, lat. 55° 45′, where it broke out about the same time as at Casan, namely, " about the middle of September."

The reader, if he examine the foregoing paragraph, which I have condensed chiefly from the review of Dr. Lichtenstadt's *Die Asiatische Cholera in Russland,* contained in the Edin. Med. and Sur. Journ. July 1831, adding the latitude to each place, will perceive that the date of its first appearance at each place corresponds with the harvest period, which, for the most part, agrees with each respective latitude, with one or two slight exceptions, which may be explained from local circumstances: so that, for example, Casan and Moscow, lying almost exactly in the same latitude, began to be affected almost at the same time, although nearly five hundred miles apart.

The epidemic of 1830 did not cease as did that of the previous year, " but continued to spread with little intermission" westwards, and arrived at Warsaw in the beginning of May. But as it now appears to have assumed a contagious character—on which, however, there are doubts—we should not expect its progress to be distinctly marked by the harvest periods of the different places which it visited. What I would now remark is, that the common diet of the lower classes in the countries which have suffered from the disease, is chiefly bread from rye corn—a species of grain which has always been accused of being more subject than any other to vitiation, both from diseases to which it is liable, such as blight, ergot, &c. as well as to a foreign admixture of injurious seed, such as from Raphanus raphanistrum, the Agrostuna, the Centaurea cyanus, the Nigella satira, and the Lolium temulentum, all of which the cultivation of rye peculiarly favours. In Georgia, as well as in most parts of European Russia, in the northern parts of Prussia, Poland, and even in Hamburgh, I am informed that the common food of the poor is from rye: often the bread, or rather cakes, are made from grain uncleaned, ground with the husk, imperfectly fermented, and half baked, and in this state kept for use. In Persia, from whence the disease is supposed to be traced, the usual food is rice, the supply of which, we are informed, owing to the improvidence of the people, is seldom sufficient for the supply till the following harvest, so that the inhabitants are obliged to use the new crops in their most recent state. Is it not, therefore, worth inquiring if the propagation of the disease in question is not occasioned, or at least favoured, by an improper quality of diet, especially from *corn bread*, arising from the corn being used in too recent a state, or from a vitiation to which rye is more subject than other grain?

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* See Opera Universa, p. 177.

† Handbuch der Path. § 123, 127.

‡ Spec. Therap. 4. Band, § 124.

It may, perhaps, assist our inquiries if we turn our attention for a few moments to the epidemic which, at different periods, from the year 1577 to 1771 was so fatal in Silesia, Voightland, Lower Saxony, and other parts in Germany, as well as in Sweden, Switzerland, and in France, between the Cher and the Loire. This disease has, by all writers on the subject, been ascribed to the causes we have mentioned, namely, to an improper quality of bread corn, especially of rye, which was the common food of these countries: it also presents so many resemblances to the epidemic now prevailing in Europe, that a comparison between them will not be uninteresting to the reader. I have, therefore, copied *verbatim* Drs. Barry and Russel's graphic description of the cholera as it occurred at St. Petersburg, and have condensed Richter's description of the epidemic spoken of, which he designates by the name of "Die acute oder fieberhafte Kriebelkrankheit (*raphania* of Mason Good), as contained in his *Specielle Therapie*, vol. vii. p. 607. I have placed each description in alternate paragraphs, that the comparison may be made with greater ease.

Dr. Barry states that "Diarrhœa, at first feculent, with slight cramps in the legs, nausea, pain or heat about the stomach, malaise, give the strongest warning."

According to Richter, the warnings (vorbothen) consist in a loss of appetite, furred tongue, pain in the region of the stomach, numbness or slight trembling of the limbs, formication.

"When violent vertigo, sick stomach, nervous agitation, intermittent, slow, and small pulse, cramps beginning at the tip of the fingers and toes, and rapidly approaching the trunk, give the first warnings, there is scarcely an interval."

There is vertigo (schwindel), obscure pain in the occiput; pulse small, quick, and intermitting; clonic, alternating with tonic, spasms in some parts.

"Vomiting, purging, or both of these evacuations, of a liquid like rice water or whey, come on."

Vomiting of sometimes nearly black bile. (This dark-coloured vomiting is sometimes observed in the Indian cholera.)

"The features become sharp and contracted, the eye sinks, the look is expressive of terror, and wildness, and as it were, a consciousness on the part of the sufferer that the hand of death is upon him."

The appearance is pale, sunken, and hippocratic; the eyes roll in their sockets, which gives the patient an horrible appearance; he squints, and the pupils are contracted.

"The lips, the face, the hands, the feet, and soon the thighs, arms, and whole surface assume a leaden, blue, purple, black, or deep brown tint, according to the complexion of the individual, varying in shade, according to the intensity of the attack. The fingers and toes are reduced at least a third in thickness; the skin, and soft parts covering them, are wrinkled, shrivelled, and folded; the nails put on a bluish pearl white; the larger superficial veins are marked by dark lines, of a deeper black; the pulse is either small as a thread, and scarcely vibrating, or else totally extinct; the skin is deadly cold, and often damp; the tongue is always moist, often white and loaded, but flabby and chilled, like a piece of dead flesh."

The pulse is small, very quick, spasmodic (krampfhaft), frequently remitting. The patient has a cold fit (frost,) the skin is either rough or dry, or breaks out in a cold clammy sweat, the tongue is always discoloured, and finally almost black. (N.B. In the Indian cholera, the tongue has been observed to have a dark fur towards the end.)

"The voice is nearly gone; the respiration is quick, irregular, and imperfectly performed. Inspiration appears to be effected by an immense effort of the chest, whilst the alæ nasi (in the most hopeless cases, and towards the close), instead of expanding, collape and stop the ingress of air. Expiration is quick and convulsive. The patient only asks for water, speaks in a plaintive whisper (the vox cholerica), and only by a word at a time, from not being able to retain air in his chest for a sentence."

Respiration is difficult, and performed with great effort, (heftigen anstrengunen;) there is, as Mason Good expresses it, " a sense of suffocation, and a difficulty of articulating distinctly," with an unquenchable thirst.

" He tosses incessantly from side to side, and complains of intolerable weight and anguish around the heart. He struggles for breath, and often lays his hand on his stomach and chest, to point out the seat of his agony. The integuments of his belly are sometimes raised into high and irregular folds, whilst the belly itself is violently drawn in, and the diaphragm upwards and inwards towards the chest. Sometimes there are tetanic spasms of the legs, thighs, and loins, but we have not seen tetanus, or even trismus."

The extremities are alternately drawn up and extended, to which is conjoined an irresistible inclination of the patient to restrain these spasmodic movements, in which he succeeds very imperfectly. There is a burning sensation in the internal parts; then follows grinding of the teeth, and even trismus, by which the tongue is not seldom injured.

" There is occasionally a low suffering whine. The secretion of urine is always totally suspended; nor have we observed tears shed under these circumstances. Vomiting and purging, which are far from being the most important or dangerous symptoms, and which in a very great number of cases of the present epidemic have not been profuse, generally cease, or are arrested. . . . . Towards the close of the scene the respiration becomes slow; there is a quivering among the tendons of the wrists; the mind remains entire. . . . . After the cold blue period has lasted from twelve to twenty-four hours, or upwards, the pulse and external heat begin gradually to return. . . . . A profuse critical perspiration comes on from the second to the third day, and leaves the sufferer convalescent; but much more frequently the quickness of pulse and heat of the skin continue; the tongue becomes brown and parched, the eyes are suffused and drowsy; there is a dull flush, with stupor and heaviness, about the countenance, much resembling typhus; dark sordes collect round the lips and teeth; sometimes the patient is pale, squalid, and low, with the pulse and heat below the natural standard,—but with the typhus stupor, delirium supervenes, and death takes place from the fourth to the eighth day, or even later," &c.

Towards the close of the hot stage of the disease the patient falls into a mild delirium, mutters between his teeth, is sad and low, is indifferent to surrounding objects, and neither hears nor sees correctly; he shews all the common symptoms of typhus, *cum torpore, nervosa, stupida.* After some days, colliquative and putrid symptoms set in; meteorismus and emphysematous enlargements, petechiæ, ecchymosis, abscesses and real carbuncles on the extremities, quickly going into mortification. The fingers and toes are first discoloured, and then gangrene; the local mortification soon spreads universally; death happens commonly about the seventh day of the disease.

The reader will perceive two remarkable differences in these diseases, namely, the sense of formication in the extremities at the beginning, and the tendency to mortification of the extremities towards the termination of the disease, which are not observed in cholera. It appears, however, from the authority of Sprengel, Wichmann, and Taube, that these two symptoms belong rather to the chronic raphania; Dr. Mason Good has therefore omitted them in his short sketch of the disease in question, and has with propriety referred the tendency to mortification of the extremities to the chronic disease, under the name of gangrena urtilaginea, or mildew mortification, a disease universally ascribed to the same cause as the one before us.

There are many points of resemblance between the two diseases, which we have not yet noticed, namely, the season of the year in which raphania made its appearance—about the harvest period; it also chiefly attacked the poor and ill-fed; it was also remarked that in raphania the violence of the spasms did not indicate the greatest danger; also the most successful treatment was similar, such as large doses of calomel (Taube), antispasmodics, and externally the warm bath, spirituous and stimulating liniments, &c. The post obituary symptoms were also similar, as dark spots on the stomach and intestines, distended with air; the abdominal vessels were found distended with much black carbonized blood (mit vielem schwartzen verkohlten Blute angefuellt). This disease has, notwithstanding its acknowledged cerealean origin, also been supposed to become contagious.

The absence of the congee evacuations, and the presence of bilious vomiting, would induce us to consider the disease as akin to bilious cholera; and the violence of the spasmodic symptoms, among which even trismus has been observed, would give it a nearer resemblance to the spasmodic cholera; the typhoid symptoms, however, would seem to bring it into a nearer relation to the Russian cholera. The chief difference between this disease and the various species of cholera seems to be a less tendency to collapse in its beginning, by which the cold or blue period is less strongly marked; there is likewise a less strikingly observable reaction in the system after the cold stage; also a typhoid character is more observable throughout its whole course.

The chief use I would make of this comparison between the acute raphania, or, as it might be called, the Silesian cholera (from Silesia, where it has been most frequently epidemic), and the other species is, that as that disease has been universally allowed to have had a cerealean origin, it would strengthen our suspicion that the other species may have the same cause.

That diet has some influence in the propagation of epidemics has never been denied; that unwholesome diet prepared from corn in an improper state, especially from rye, which is, as before remarked, more subject than other grain to become unfit for food, has been a frequent cause of fatal epidemics, has been generally believed. I suspect we may trace somewhat of the acknowledged injurious effect of this kind of diet in modifying the usual bilious cholera of Germany, which, as described by Sprengel, Richter, and other writers, is a much severer disease than we are commonly acquainted with in England, being frequently accompanied by convulsions, and inflammations tending to gangrene, of the stomach and bowels—symptoms that have long since been ascribed to the use of rye food. It may be remarked, likewise, that cholera both in India and Persia, countries whose inhabitants do not cultivate rye, has existed for many years, without exhibiting typhoid symptoms, or being usually supposed to be contagious; yet, as soon as it visited Russia and other rye countries, it puts on a typhoid form, and people begin to believe it to be contagious. I believe also in such countries diseases in general, and especially fevers, do not bear bleeding so well as in this country, and are more apt to run into a typhoid form.

On the whole I am inclined to think, although direct matter-of-fact proofs are not sufficiently numerous to make it certain, that the continental cholera has for its chief predisposing and modifying cause the use of rye as the common diet, and perhaps of that and other grain in a vitiated state; or in other words, that the peculiar epidemic constitution of the human system, by which it becomes predisposed to the Russian cholera, is the said use of rye, immature, or otherwise vitiated.

Should this view be correct, it would be a consolatory reflection that in this country, at least, we shall be exempt from this scourge of the human race, or should it visit us, it will be disarmed of half its terrors.

Even in those instances which have lately been reported, where individuals of the higher ranks, and in whom a vitiated diet may not apparently be accused with justice, yet have received the disease, as is supposed through contagion, such cases, should they be fully proved, would only shew that other causes beside diet would also modify or determine the character of the disease in those individuals: they would not in the least invalidate the idea that the general character of the epidemic has been influenced or determined by the nature of the diet.

I think Dr. Prout's doctrines of *merorganization* will, when more fully developed, throw much light upon the subject before us: for example, that grain, in its too recent state, may be supposed not to be so perfectly *merorganized* as to be perfectly fit for food. If we consider the *merorganizing* power as something distinct from the organizing, plastic, or formative principle of Sir Gilbert Blane—which latter builds up, as it were, the structure both in vegetable and animal life—it will then be understood to be that which forms or modifies the materials which the organizing power uses in its operations. *Merorganization may, then, be considered* as a chemico-physiological exposition of digestion, chylification, sanguification, assimilation, secretion, &c.; in all of which, merorganization, we suppose, forms an essential part. Yet, it not only contributes to organization as a preparatory process, but it must also depend upon the latter, because, without organization it could not have place. In grain, when first ripe, the organization, we suppose, is complete; yet we have reason to consider that the greater part of its substance, which is designed for the food of the future plant, as still undergoing the merorganizing process. If we regard the perfection of the panary fermentation as a test of the grain being sufficiently aged to be fit for food, or, in other words, that its *merorganization* is perfect, it would appear that this process requires several months for its performance; for I am informed that the new grain is not fit to be used by itself till about Christmas, until which time it is usual, in London, to mix a considerable proportion of old grain before sending it to the mill[*]. If grain, then, is used in its too recent state, we suppose that, its merorganization being incomplete, too great a tax is laid on the merorganizing power of the stomach, and probably of the whole system; and this will, if other causes conspire, produce diseases of the stomach and its collatitious viscera; of which diseases cholera is one.

I have also been informed, by good authority, that the late harvest in this country has not been productive of grain of so good a quality as is commonly believed. Mildew had been unusually prevalent—a disease of corn in which it is observed that a number of grains in each ear are small, and its organization imperfectly developed: how far this may have contributed to the unusual prevalence of cholera after our late harvest, I leave the reader to judge.

The following deductions, then, may be made; but, as we have not all the facts before us necessary for their demonstration, I will merely set them down in the form of queries.

1st. *Is* it not probable that grain, in its too recent state, or in an otherwise vitiated condition, is a common predisposing cause of cholera?

2dly. That as rye corn is more subject to such vitiation than other grain, and has always been accused of producing disease characterized by its resemblance to Russian cholera; and as the sporadic and epidemic cholera of rye countries have these characters; is it not probable that the use of vitiated rye bread is the predisposing cause of that disease?

It might also form a proper subject for inquiry, how far the various salts used in the manufacture of bread (which salts have been considered as merorganizing substances) may be useful to the human constitution, in preventing a predisposition to cholera; and this inquiry may, perhaps, be made in connexion with the interesting facts mentioned by Dr. Stevens, as well as those communicated through Drs. Barry and Russell, relative to the use of such substances in the treatment of the Russian cholera.

TRANSFUSION OF BLOOD IN CHOLERA, &c.

To the Editor of the London Medical Gazette.

SIR,

As the subject of cholera is becoming daily of more fearful interest, I am sure you will concur with me in the opinion that every suggestion, however remote the benefit of its practical application, demands publicity at the present moment.

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[*] Richter informs us that the government of the day recommended the inhabitants of the districts most subject to the epidemic, as a prophylactic measure, not to use the rye or barley too new; and also, when thrashed, carefully to separate all other seed, and even to wash and dry the corn in an oven, before sending to the mill, &c.

I offer no further apology for the following hasty observations; and, sir,

I remain, yours, &c.

T. W. W. Smart,
M.R.C.S., &c.

Cranborne, Nov. 14, 1831.

In the first place, I wish to be included within the number of those who object to the appellation of this pestilential disease, which, having been assigned to it on the assumption of a false analogy, is calculated to misdirect us into the paths of mischief and error. Before we may establish the identity of any two diseases, it is essential that the comparison made should be completely accordant in all its parts, neither exclusive of one particular symptom on the one side, nor giving the preference to another on the other side. The history of both must accurately agree. If a partial view alone be taken, we are continually liable to be misled; for how many differing diseases are there not, which manifest symptoms common to all of the same class? For instance, the acute eruptive diseases—in all of them the prefatory febrile affections are alike, and even when the cutaneous inflammation first appears, a mistake is very likely to be made by an unpractised eye, though the more skilful never would confound the early stage of scarlatina with measles, or that of measles with small-pox; for he would apply his power of observation to discover the peculiar diagnostic mark which each wears, in addition to its common resemblance to the other. So is it with English and Asiatic *Cholera*—as it is called. They may have some symptoms in common, but there exist others sufficiently strong to determine a wide distinction between them. Vomiting and purging, the very characteristics of our native disease, are by no means the invariable or most prominent symptoms of the other; spasms, so constant in the Asiatic, are of unfrequent occurrence in *ours*. Then the fatality of the one compared with the almost innocuousness of the other; its decidedly contagious nature; besides, those symptoms of universal paralyzation, that sudden abolition of all physical energy, present to my mind phenomena with which English cholera bears no parallel, not even in degree. It sometimes happens in this country, that patients under this complaint, (females particularly,) seem very depressed, low, and faint, but not more so, nor so much, as they do often under an attack of hysteria. It frequently happens too, that in some of its severest forms the symptoms of nervous depression are scarcely developed at all. I had an opportunity of remarking this the other day, in one of the most acute cases of cholera I had ever witnessed. It began as usual by rigors, febrile action, pain, vomiting, purging, and the matter voided was of a yeasty pultaceous quality; this was the first stage. The next was ushered in by spasms of the legs, thighs—in fact of the whole frame—accompanied by the most torturing pain; this stage lasted eight hours. Still there was no exhaustion, no debility beyond what one would reasonably expect to ensue after copious evacuation; for I promoted the object of nature to expel the contents of a loaded, stuffed alimentary canal, by the use of purgatives, and they brought away quantities of black, horribly offensive fæces. The man was a free liver, a great eater, and he had experienced for two or three days previous to the attack an increasing distention of the abdomen; but when the bowels had been cleared, there was no more distention, no more pain, no more spasm, no more *cholera*,—and this was effected in twenty-four hours; in twenty-four hours more he was well, to his astonishment, considering the previous acute suffering, but still complained of muscular tenderness. This was a specimen—a strong specimen—of English cholera. I might have prescribed the fashionable nostrum, cajeput, in its dose of 40 drops, or preferably to that the oil of turpentine, as it unites the antispasmodic virtue of the former with its own purgative quality; and these remedies might have succeeded *à merveille*; for my idea is, that the cause of the disease is not the presence of acrid bile (χολη ρεω), but a spasmodic action of the muscular fibres of the alimentary canal, depending probably on nervous irritation, brought on by the distention, or the excitement produced by an excess or an impurity of the food. But are we to expect success from these, or similar remedies, in the Asiatic pestilence, now threatening to devastate our country—that deathblow of our nature, as it were—that corrupter of the fountain and springs of life? I am incredulous of the advantage to be derived from their use, as well as from other popular modes of treament. What, may I ask, are venesection and calomel expected to effect? means, the best adapted, in my opinion, to finally extinguish the still flickering flame of life. When I spoke of cholera as a "corrupter," I meant to allude to the changes which take place in the blood in that disease. This is a fact known to foreign pathologists; it is also known by Clanny and Stevens, that changes, perhaps similar changes, take place in the blood of typhus patients. Hence, there is some reason for assimilating the two diseases, as well as on the additional ground that, after the violence of the attack of cholera is past, fever of a typhoid character is apt to ensue. If the restoration of the healthy properties of the blood be so important a point in the treatment of typhus, as the researches of those gentlemen would affirm, and if this can be effected by the internal administration of carbonic acid gas or the neutral salts, I have long thought, why should not these substances be directly conveyed into the current of circulation by injection? Again, another point of resemblance between *the* cholera and typhus is seen in that stage of prostration of vital power, which, again, bears some analogy to the prostration resulting from excessive hæmorrhage. I would therefore propose, not only to try the injection of the above remedies into the veins, but also the transfusion of pure blood. The object of treatment seems to me to be that of exciting the vital powers, in fact, of simply keeping the body alive; and would not the substitution of a few ounces of pure blood, in the room of an equal quantity of vitiated fluid, be likely to prove a powerful, as it is the natural, stimulus of the heart's action? I leave these suggestions with your readers, determined myself to act upon them, should circumstances call for my interference. With regard to other plans of treament, I would mainly rely on brandy and opium, stimulating frictions to the body, stimulating injections, and the hot sand-bath, as it will afford the same advantages as the hot water bath, without incurring the necessity of removing the patient from bed. Steam may be similarly applied.

Note on the late Influenza.

The influenza visited the part of the country where I reside, in the middle of August, and continued till the latter end of September.

I attended more than 100 cases, but these were not all that occurred.

It attacked persons of all ages, both sexes, of all constitutions, and in all situations. The old, and those weakened by previous diseases, particularly by chronic coughs, felt it most.

It began with the usual symptoms of slow fever, which, in some old people, assumed the typhoid type, and reduced them very low. Then cough, of a spasmodic character, sometimes like hooping-cough, attended with mucous secretion, not affecting the respiratory sound, at least in those cases I subjected to the stethoscope; coryza, epiphora, sore throat; but, whenever it affected the power of deglutition, very little inflammation visible; aphonia, in some cases.

Duration of cases in general about a fortnight.

Only two fatal cases, and those indirectly so. The one, a young girl, who had suffered with spasmodic asthma seven years; the other, an old man, a cripple, with a chronic cough.

In every case, in which my attention to the point after a while was directed, I found tenderness of the spine, chiefly of the upper dorsal and cervical vertebræ, so much, in some instances, that very slight pressure could not be endured; and the intensity of this local affection was in direct ratio with the violence of the general disease. I found antimony, with Dover's powder, the best medicines.

Blisters to the spine relieved the pain in the head and the cough; tartarized antimony ointment irritated.

I consider that the disease was more spasmodic than inflammatory, resulting probably from spinal and other nervous irritation.

T. W. W. Smart.

GALVANISM IN CHOLERA, &c.

To the Editor of the London Medical Gazette.

Sir,

I do not consider a newspaper as the legitimate channel for the conveyance of medical opinions in general, but, under existing circumstances, some allowance should be made for an anxiety to communicate any information that we may deem important, on the all-engrossing subject of cholera.

Strongly impressed with the conviction that galvanism might be employed, with good effect, in the alarming cold stage of cholera, I addressed a short letter to the editor of the Globe and Traveller, on Sunday last, (published in that paper on the 15th instant,) with the hope, that the hint might be immediately conveyed to some of my professional brethren at Sunderland.

With your permission, I am desirous of making a few further observations, through the channel of your excellent periodical.

If we may depend upon the experiments, recently made, touching the identity of the "nervous power" and galvanism, as connected with the fact of the evolution of caloric from arterial blood, they may lead to the application of galvanism, for very important remediate purposes, in other diseases, although my present views are principally directed to "spasmodic cholera."

It has fallen to my lot to witness and to treat *very many* of the worst kind of fevers, modified by the different climates of the four quarters of the globe; and it is the conviction of my mind, from all that I have seen and read, that the modified spasmodic cholera, as it has appeared on the continent of Europe, is a fever, only differing in degree, and in a very few symptoms, from some other fevers of a malignant nature.

The poison producing these fevers, (whether arising from malaria, or from the effluvia of the human body,) appears to me to make its primary attack upon the nervous system; and, by its powerful sedative effect, deprives the body of that nervous influence by which only, caloric can be evolved from the arterial blood. Hence the coldness of the body immediately manifested outwardly; and which (coldness) diminishing the capacity of the bloodvessels near the surface, and forcing the blood to retire inwardly, produces that state of congestion of the vital organs so strikingly illustrated by the laboured breathing and languid and oppressed state of the circulation, as well as by the gastric symptoms which generally supervene. The spasms I also attribute to the interruption of the supply of the nervous influence to the different muscles.

With all due deference to those gentlemen who recommend the use of internal stimulants, I must take leave to say, that I think they frequently do much harm. I sincerely believe that the congestive state is rather increased than relieved by giving such quantities of liquid fire.

If I might be permitted to recommend a plan of treatment, I should say, *immediately* on the attack of cholera, place the patient in a warm bed, solicit a return of the circulation to the surface, by means of the hot air-bath, and, if necessary, by passing a gentle and continued stream of galvanism from head to foot of the patient; and then relieve internal congestion by the abstraction of blood from the arm. If the gastric symptoms are severe, I believe that moderate, not large, doses of calomel and opium, frequently repeated, will be found most serviceable in allaying irritation; and even the exhibition of a few drops of prussic acid might be resorted to, in the event of vomiting being a very urgent symptom. The calomel would also have, in the course of a short time, a very beneficial effect, by emulging the liver; for even in the severe cases of English cholera, I have observed that the flow of bile is generally suspended.

It is well known that venesection has been resorted to, with much benefit, in the cold stage of obstinate intermittents. There can be little doubt but that bloodletting relieves, by abating the internal congestion, which had been brought on by the coldness of the surface of the body; but, in these cases, if the hot air-bath were applied at the same time, to solicit a return of the circulation, to the blood-vessels near the surface, the good effect of venesection would be more evident.

On the subject of contagion, I will say but little. I believe the following to be the real state of the matter. When a person, at all predisposed to receive infection, is exposed to the poison of cholera in a concentrated state, he will certainly get the disease; but if not in some measure predisposed to receive infection, he will in all probability escape, even when exposed to the concentrated poison: on the other hand, a person must be strongly predisposed to receive infection, before he can contract the disease, when exposed to the poison in a *diluted* state. Hence arises the discrepancy of opinion on this subject.

I will close this letter by mentioning an apt illustration of the susceptibility of the same person, at different times and under different states of the body. In the summer of 1801, I had under my care, on board an hospital-ship, in the river Medway, more than a hundred cases of the worst type of typhus fever. For several months I visited these patients three or four times daily, and slept in apartments divided from the sick only by a thin partition of boards, and consequently breathed the hospital atmosphere night and day. Yet, I escaped the fever, although many died around me. One night, however, I unfortunately slept at a friend's house, a few miles distant in the country, and being anxious about a French officer, a prisoner of war, then under my care, I walked from my friend's house, when, heated, and rather tired, I entered the hospital, without resting, or taking my breakfast. I caught the fever, and *narrowly, indeed*, did I escape with my life.—I remain, sir,

Your very obedient servant,
JOHN TWEEDALE, M.D.

Lynn, Norfolk,
Nov. 18, 1831.

APPLICATION OF GALVANISM IN CHOLERA.

To the Editor of the London Medical Gazette.

Lynn-Regis, Norfolk,

SIR,

You can readily imagine that the inhabitants of this place, from their connexion by shipping with the port of Sunderland, are at the present moment in no small degree under the influence of fear of cholera coming amongst them, and that there exists some foundation for this fear no one will deny. The consequences of the panic are, that every individual is putting his house into order; and the magistracy, the head of which possesses energy, and the best of all senses, *common sense*, have to the utmost of their power instituted the wisest precautionary measures, and we are now, as men on the eve of battle, anxious, yet vigilant and prepared.

Of course the medical men at this juncture are not sleeping at their posts, but are cheerfully co-operating, giving their time and thoughts to every measure that is likely to conduce to the health and safety of the inhabitants.

I now address myself to you, for the purpose of eliciting your opinion, and the opinions of the profession generally, through you, (if you deem it worth while) on the probable utility of the application of galvanism in cholera. I have forwarded a letter to the president of the Medical Board in London on the subject, and have no doubt, if the suggestion is of the smallest value, it will meet with attention. I give the following extract from the communication alluded to:—

"The paralyzing effects of the cholera poison upon the viscera of the chest and abdomen, through the medium of the pneumo-gastric and sympathetic nerves, are so abundantly apparent that it has long since struck me (as most probably it has others) that the instantaneously stimulating powers of galvanism may be brought into operation in the stage of deathly collapse, so as to induce reaction, and by giving vigour to the circulation, &c. enable us to pursue a course of practice which the developed disease demands."

I have not now leisure to proceed further with the subject, but may just mention, that having seen the extraordinary resuscitating powers of galvanism in the case of my own child, who I have every reason to imagine was restored by the influence of galvanism applied for more than twenty minutes along the course of the nerves proceeding to the heart and chest—after being laid lifeless, to all appearance, on its couch from struggling with continued and appalling convulsions for five days, I cannot but entertain a hope that the same means may prove useful in rousing the system out of the overwhelming depression of all its energies, which appears to constitute the most appalling feature and dangerous symptom of cholera.

I have the honour to be, Sir,
Your most obedient servant,
THOMAS INGLE.

CHOLERA—A HINT TO THE LADIES.

To the Editor of the London Medical Gazette.

Sir,

At a time like this, when every mind is agitated by direful apprehension of the awful plague which, having ravaged so large a portion of the continent, has at length reached our shores, no suggestions, however apparently trifling, can, I trust, be deemed really unimportant if they tend, in any degree, to promote precautionary measures, especially, too, when those suggestions are addressed to the "better half of man," and may possibly guard them from much danger. I would, therefore, through the medium of your highly-valuable and intelligent journal, earnestly call their attention to the few following observations, and beseech them to put them into practice, since, as they cannot at all events be injurious, they may be eminently beneficial. First, then, I would strongly recommend them to pay particular and constant attention to the preservation of warmth, which is universally acknowledged to be one of the most efficient means of prevention. With this view, they must, of course, give strict attention to their clothing, selecting such articles of dress as are of the warmest texture, without so much regard to appearances as too generally prevails, and especially keeping warmly covered the neck, bosom, feet, and arms—parts of the body the chilling of which occasions often the most lamentable results. I would advise them to discard the thin and flimsy silk stockings which are so much worn, and adopt woollen ones, with boots; for cold feet in an eminent degree contribute to produce severe affections of those very organs most concerned in cholera. What are called snow-boots, also, would be very useful and comfortable for exercising in, both in carriages and when walking: they may not be very elegant; but surely that should be no consideration when the preservation of life is put in competition. Next I would advise them to avoid the practice of tight-lacing, than which nothing can at any time be more injurious, but now of the most vital necessity; for only let them consider the importance of the organs which are compressed and squeezed out of all natural shape and proportion by such a habit; and let them be informed that two of these—the lungs—are great outlets to carry off any deleterious matter in the system, which function they cannot perform if their size is diminished, and their action hindered by the forcible compression of the chest, occasioned by tight stays.

Next, I would most seriously urge upon them not to frequent large, crowded, and heated parties and assemblies, where they are compelled to breathe over and over again the contaminated atmosphere created by the congregation of many persons, and where also they are more subject to the effects of cold air: when quitting the heated and crowded room they have often, in winter, to encounter a cutting, frosty wind, and perhaps sleet or snow, in entering their carriages. In addition to the foregoing advice, I would also recommend those of a nervous temperament, and those with languid circulation, to have the limbs and surface of the body well rubbed every morning and evening, for at least half an hour each time, so as to excite a healthy action of the nerves and blood-vessels, and regularly to take exercise by walking in the open air for two hours at least daily, and to observe regular and earlier hours for their meals than are now the fashion. And last, though not least, let them, putting a firm trust in that Almighty power which can alone direct and stay this plague, discard as much as possible unnecessary fear, and employ themselves in cheerful occupations, with which domestic affairs will supply them; and in a blessed way may their minds be further preserved from personal apprehension by their preparing, as far as is in their power, articles of clothing, and other comforts, to distribute among their poorer fellow-creatures, who, if attacked by this horrible malady, will in this manner be better provided to withstand its effects, and whose blessings will carry joy and peace to their hearts. If, sir, these few suggestions should be thought by you worthy of insertion, you will much oblige one who is anxious for the welfare of the fair sex, and who begs leave to remain your obedient servant,

Philogynaikos.

Nov. 15th, 1831.

A BRIEF SKETCH OF THE BOTANY OF THE VICINITY OF SYDNEY, NEW SOUTH WALES.

By George Bennett.

Having arrived at this interesting colony on the 22d of January, 1829, my attention was soon directed towards its numerous vegetable productions; but my brief sojourn did not permit any extension of my *herborizations* far inland. The Botanic Garden, however, established at Sydney (under the superintendence of Mr. Frazer), afforded me ample opportunities of viewing the more distant indigenous productions of the colony.

The country in the vicinity of Sydney was strewed with a profusion of flowering shrubs, thriving in an arid soil; but there seemed to be a peculiar character in the vegetation of this country, the foliage of the Australian trees and shrubs having a dry appearance, and being destitute of that lustre so observable usually in those of other countries. This want of lustre is attributed, by Mr. Brown, to the equal existence of cutaneous glands on both surfaces of the leaf [*]. Another peculiarity is the lofty trees being destitute of branches, excepting at the summits, and shedding their bark; some being observed perfectly decorticated, and in others the cortex is seen hanging in long strips about the trunk. All these peculiarities conveying to us different ideas from those which had been formed from the vegetation of other countries.

Among the extensive Proteaceæ family was the Grevillea sericea, Isopogon anethifolius, &c. and the beautiful Banksia genus (the honeysuckle of the colonists) [†], the wood of which is used by the colonists for boat-building: among numerous species of this genus was the spinosa, serrata, integrifolia, ericæfolia, &c. The Hakea genus was also abundant. That curious tree of the Asphodeleæ family, the Xanthorrhœa, or yellow gum-tree, attracted observation by its peculiar growth, long grassy foliage (from which it derives its name of grass-tree from the colonists), and towering scape, terminating in a cylindrical spike, either crowded with its small white melliferous flowers or dark-coloured angulated capsules, containing small black seeds.

At a beautiful glen at Little Cudjee, near the south head of Port Jackson, among other plants I collected the Smilax glyciphylla, or sweet tea-plant of the colonists, which was, on the first establishment of the colony, extensively used instead of the "China shrub." It is a scandent plant, and the leaves have a sweetish taste, resembling that of liquorice-root, which afterwards leaves a very slight bitter. Among the ferns collected were the Davallia elegans, Gleichenia microphylla, Polypodium attenuatum (Brown Prod. Nov. Holl.), and others. In the vales were found that elegant Proteaceous shrub the Warratah, or tulip-tree, Telopea speciosissima, and the Xylomelum pyriforme (Proteaceæ), or native pear-tree, so named from its pyriform fruit.

In the Botanic Garden, the species of Acacia indigenous to Australia were very numerous; among them was the A. stenophylla, from the south-west interior, and was the only tree seen on the horizon at the *ne plus ultra* of Oxley's expedition of 1817, at the termination of the River Lachlan; the A. fillifannilius, from the deserts of the south-west interior (vide Oxley's Expedition, 1817); A. floribunda, pubescens, falcata [*], &c. &c. The magnificent Norfolk Island pine (Araucaria excelsa) also grows in the garden as well as at other parts of the colony, where it forms an adornment to the avenues, &c.

The olive trees thrive well in the colony, and attain, it is said, a greater size in a shorter period of time, than even in their native country. Young plants are propagated by layers, and are given to such of the settlers as are desirous of raising this valuable shrub. The Casuarina palludosa, Hibiscus spectabilis, and several new plants and shrubs from Moreton Bay, with a collection too extensive to enumerate, sufficiently engages the attention of the visitors to this garden. The castor oil tree (Ricinus communis) abounds almost in a wild state, and yet the oil is still imported into the colony, when any quantity that may be required could be expressed, not only for medicinal purposes, but for burning in the lamps, for which latter purpose it is used in some parts of South America.

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[*] "It is at least certain that on this microscopic character of the equal existence of cutaneous glands on both surfaces of the leaf, depends that want of lustre which is so remarkable in the forests of New Holland."—*Sketch of the Botany of the Vicinity of Swan River*, by R. Brown, Esq. F.R.S. published in the Journal of the Royal Geographical Society of London, 1830-31.

[†] "When the different species of Banksia first come into bloom, the natives collect from the flowers a considerable quantity of honey, of which they are particularly fond, and gather large quantities of the flowers (*moncat*) to suck. It is not, however, always to be procured: the best time is in the morning, when much dew is deposited on the ground; also in cloudy, but not wet weather."—*Description of the Natives of King George's Sound (Swan River Colony) and adjoining Country*, by Mr. Scott Hind, published in the Journal of the Royal Geographical Society of London, 1830-31, p. 35.

[*] This species is found very abundant at most of the Polynesian Islands.

On the 13th of February (1829), I visited Botany Bay, and collected numerous botanical specimens. Among several of the Eucalyptus genus was observed the E. resinifera, or red gum tree, E. corymbosa, &c. The leaves of most of this genus have a peculiar smell when broken, somewhat resembling the cajeput oil; there is one species in the botanic garden which had been brought from the interior, the leaves of which yielded a quantity of oil resembling the cajeput; on breaking a leaf in the hand, the oil which exuded was very abundant. During this *herborization* I collected among others the following plants:—

Burronia polygalafolia.
Leptospermum Juniperina.
Melaleuca Fimbriata.
Metrosideros lancifolia.
Viminaria juncea.
Epacris palludosa.
Calytus Australis.
Hibbertia volubilis.
Blandfordia nobilis.
Oxalis Australis.
Epacris microphylla.
Exocarpus cupressiforme.
Symphconia montana.
Liptomera acida.
Enokelia major et minor.
Lobelia gracilis.
Lambertia formosa.
&c. &c. &c.

Botany, as well as other branches of natural history, has an extensive range in Australasia; the discoveries made have been very numerous; and when it is considered that an immense extent of land still remains unexplored, many treasures in natural history may yet be looked for from that new and extraordinary portion of the globe.

London, Nov. 12, 1831.

REMARKS ON SOME REMEDIES IN THE TREATMENT OF INSANITY.

To the Editor of the London Medical Gazette.

Sir,

It appears to me, upon perusing the report of Dr. Seymour's Croonian lecture on the medical treatment of Insanity, in the Medical Gazette of October 29th, that some errors must have occurred in the abridging of it, which render the lecturer's meaning obscure; nay, perhaps, pervert it.*

Dr. Seymour is therein reported to advise "blood-letting, principally from the jugular vein, if on accurate inquiry the patient has been exposed to causes sufficient to produce inflammation of the brain, redness of the countenance, &c." because such symptoms "point out increased vascular action, with increased power." A case is cited to prove the good effects of this practice; but no symptoms are described indicative of cerebral inflammation, though many of mental derangement. As Dr. S. subsequently cautions against general blood-letting in cases of insanity, I think it is clear he never would prescribe it because a patient had merely been exposed to sufficient cause to produce inflammation of the brain, for he has too much experience and judgment not to know that exposure to the same causes may produce either acute inflammation, or simple derangement of the faculties of the mind.

I am the more anxious that the doctor should be clearly understood on this subject; for having very fully discussed the effects of abstractions of blood in the cure of insanity†, and knowing that medical practitioners are beginning to be aware of the danger of copious venesection in such cases, misconception might occasion a relapse into former errors.

Again, there is another passage which strikes me as a misinterpretation.

Dr. S. is reported to say, that physicians, "in seeking for means to allay the inordinately increased sensibility, principally, of course, occurring in that division of insanity termed *dementia*," should commence with the application of cold, in the form of ice, the shower-bath, or in a descending column of water, termed the "douche," not only to diminish vascular excitement, but as having a really sedative effect.

I fully appreciate the beneficial effects of refrigeration in a high state of vascular excitement, and the sedative powers, when the brain is the seat of excitation from that cause. *Si color, aquâ frigidâ multâ perfundere caput*

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* The circumstances under which the paper was published, render it impossible that this should have been the case.—Ed. Gaz.

† Commentaries on Insanity, pp. 583-693.

expedit, is a Celsian axiom never to be forgotten in the cure of insanity. But it should be noted, that this form of the malady (dementia) is rarely accompanied by vascular excitement, and in ordinately increased sensibility.

Dementia is considered by Esquirol, and others, to be owing either to the sensitive organs becoming weakened, or because the brain itself has not sufficient powers to receive and retain the impression which is transmitted to it. In fact, it implies that the energy of the brain is impaired, and therefore demands remedies, not to allay, but to stimulate and strengthen its energies,

There is, however, an acute form of dementia, attended occasionally by symptoms of vascular and cerebral excitation; and, during the existence of those symptoms, and only then, local sedative applications to the head are indicated. It is to this form of dementia solely that, I have no doubt, Dr. Seymour refers.

But the "douche," once so infallible, and so great a favourite with French physicians in the treatment of insanity, like most extravagantly vaunted remedies, has had its day, and is now comparatively little used as a means of cure. When resorted to, it is more as a means of repression, and a moral agent in furious cases. Death, disorganization of the brain, and incurability, has been not unfrequently the result of its application. As a remedy, it is now actually proscribed, in many of the best-conducted continental lunatic asylums, as absolutely dangerous. In British asylums, objections have always been made to the use of the douche.

The case quoted by Dr. S. (p. 113) is an excellent illustration of the good effects of the shower-bath in *furious mania*; but your reporter, Mr. Editor, has, no doubt erroneously, made the learned lecturer conclude, that, from the statement in that case, "it is to cases of *dementia* that it is applicable."

In a malady so obnoxious to remedies as insanity, every one suggested by respectable authority should have a fair trial. On Dr. Jenner's recommendation principally, I gave an ample trial to the inunction of tartarized antimony, as a counter-irritant. My success was by no means commensurate with the expectations raised by his account of its good effects in maniacal cases. The only cases wherein I found it useful were when accustomed cutaneous eruptions had omitted to appear, or when they had suddenly receded, or when there had been, previously to the maniacal attack, great gastric irritation.

Dr. Jenner's formula* for this remedy is the best, because the most expeditious in its effects on the skin. If counter-irritation be the sole object of blistering in mania, the tartarized antimony is very much to be preferred to cantharides. Vesication, by the application of the latter, especially if there exists much vascular excitement, commonly increases violence, and often superadds the irritation of stranguary. From the pustular eruptions of the former, such consequences are not to be apprehended.

In respect to the effects of narcotics, especially of opium, in cases of insanity, about which so great discrepancy prevails, I shall take the liberty of offering a few observations, by way of explanation. Having very fully treated on this subject elsewhere, I will be here as brief as possible†.

I suspect that the discrepancies concerning the effects of narcotics, in the treatment of insanity, have arisen from ignorance, or disregard of the distinct stages which this malady assumes.

Mania, certainly, comes on sometimes very suddenly; but commonly there are three distinct stages observable—1, incipient; 2, active; 3, decline. In each of these stages the symptoms differ in form or intensity, and in each the exhibition of a narcotic, particularly of opium, will produce different effects.

The good, or ill, or no effect of an opiate, is chiefly regulated by the observance or omission of having previously removed cerebral congestion and excitement, by local abstraction of blood from the scalp, and by emptying the primæ viæ. Unless evacuation be premised, sleep may sometimes be enforced by an opiate, but the degree of excitation will assuredly be aggravated.

Dr. S. is misinformed in stating that that powerful sedative, hydrocyanic acid, had not been prescribed in this country in the treatment of insanity.

During a tour I made of the public lunatic asylums in Great Britain, in the years 1821 and 1822, I learnt that very full trials had been made of its effects in several of them. As of all other remedies, its effects were differently represented. Some practitioners had abandoned it as inefficient; others thought it had been beneficial in a few cases; others again had a high opinion of its antimaniacal powers.

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* Supra cit. p. 623.

† Ibid, pp. 610-618.

The physician who, I believe, has given it the longest and most extensive trial, is my friend Dr. Balmanno, of the Glasgow Lunatic Asylum. I have adverted to his experience of the effects of prussic acid in my work on Insanity, and the result of my trial of its virtues on the recommendation of this experienced physician[*]. It is candid to confess, that mine was not a fair trial of its virtues; for, knowing its active and dangerous properties, I was unwilling to entrust its exhibition to any one but myself, and as I could not always be present to see it administered with due regularity, I did not persist with it.

In answer to an inquiry I made of Dr. Balmanno in 1828, whether he continued to exhibit this preparation, he says, "I still use the prussic acid, and generally in combination with hyosciamus. The formula is the following:—

℞ Extracti Hyosciami, ℥j.
Syrupi Sacchari, ℥xij. Misce.

"This is ʒij. of the extract to ℥j. of the syrup. Of this syrup I never give less than ʒij. for a dose, commonly with gtt. xxx. of the prussic acid. When this does not procure rest or quietness, I increase the dose; and I increased it in one case to an ounce and a half of the syrup, and eighty drops of the prussic acid, for one dose, with complete effect. In short, I never fail to quiet the most highly-excited maniac by this medium. Generally, throughout the day following a sufficient dose, the patient is calm, or somewhat stupid. I never saw the smallest injury produced by the largest dose I have given."

Aware of the fatal result attending in Dr. Ferrus's cases at the Bicêtre, in Paris, from the hydrocyanic acid being administered prepared from a different formula to that which he had been accustomed to prescribe, I requested Dr. Balmanno to inform me what preparation of it he made use of. In a letter I received from him last year, he observes, "I have not yet slain any of my patients by the prussic acid; but I still give the medicine with the hyoscyamus; and both in mania, and in the delirium of typhus fever, I find the combination effectual. The prussic acid which I use is that which is commonly procured from the shops."

In respect to arsenic, the utility of which Dr. Seymour suggests in maniacal cases, and which he believes has not been tried, I may remark that it has been recommended by several British authors in chronic insanity assuming a decidedly periodical type; and in Germany especially, and in other countries of Europe, arsenic has been most extensively used with the same intention.

Having before fully commented on the other remedies discussed by Dr. S. in the work referred to, I shall not lengthen this letter by a repetition of my remarks. Several, however, are omitted, such as digitalis, gyration, and the swing, highly deserving notice in a review of the remedies applicable to the medical treatment of insanity.

I am, Sir,
Your obedient servant,
G. Mann Burrows.

64, Great Russell-Street, Bloomsbury-Square,
November 1831.

CASE OF PROLAPSUS UTERI,

Cured by a New Operation.

By Marshall Hall, M.D. F.R.S.E. &c.

The subject of the case which I am about to detail, was a poor woman, whose bread depended upon the labour of her hands. Her sufferings, from the prolapsed state of the uterus, were often extreme, and she was frequently disabled from engaging in her various occupations.

For several years there had been complete prolapsus of the uterus; to this were also conjoined a partial descent of the bladder at the anterior, and of the rectum, formed into a pouch, at the posterior part of this prolapsus. The os uteri protruded at least two inches beyond the os externum.

It occurred to me that, if the canal of the vagina could be considerably, permanently, and firmly, reduced in its diameter, the uterus would be supported in its place, and prevented from resuming its prolapsed situation; and that this might be done by removing a portion of its mucous membrane along the anterior part, and by bringing and returning the denuded surfaces in contact by successive deep sutures, until they should unite by cicatrix.

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[*] Supra cit. p. 656.

This operation was performed by Mr. Heming, of Kentish Town. The uterus being protruded as much as possible, by the efforts of the patient, two parallel incisions were made through the mucous membrane, from the sides of the os uteri, along the course of the protruded vagina, to the os externum; the portion of this membrane situated between these incisions was then removed, leaving a space of one inch and a half in breadth, and of the entire length of the vagina, completely denuded. A suture was then inserted, near the os uteri. This suture being tightened, the os uteri was obviously pushed upwards. A second, a third, and other ligatures, were then inserted, in the same manner, at short intervals, to the os externum; each ligature, on being tightened, moving and supporting the os uteri upwards.

This operation was attended with little pain; the only sensitive parts of the membrane being those near the os uteri and os externum.

The patient was directed to keep quiet in bed. The bowels had been opened. An opiate was given. No pain or fever followed. In four or five weeks the denuded parts had firmly united, and shortly afterwards the ligatures were come away.

On examination, six, eight, and ten weeks after the operation, the os uteri could be just felt *in situ*, by the finger passed through the vagina: the vagina was firmly contracted along its whole course.

The prolapsus of the uterus was thus completely remedied. The descent of the pouch of the rectum was lessened.

14, Manchester-Square,
Nov. 19, 1831.

P.S. The principle upon which this case was treated, is illustrated by a fact, detailed to me by Dr. Holland, of Queen-Street, May-Fair. A pessary, introduced in a young person to support the uterus, subject to be completely prolapsed, induced great inflammation. This was followed by such firm contraction of the vagina, that the uterus ever afterwards remained in its proper situation.

MEDICAL GAZETTE.

Saturday, November 26, 1831.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

THE "BURKING" SYSTEM.

THE fearful disclosures made within the last week or two, through the activity of our excellent police, have thoroughly and justly alarmed the public mind. The cry of "burking" is once more revived; and, candidly speaking, (apart from the horror which we entertain in common with the rest of society for this worse than cannibal crime,) we are not sorry that it is revived. The existence of such a state of things in any community, as that wretches should be induced, under any circumstances, to murder the living for the sake of the profits arising from the sale of the dead, is dreadful; but if it does exist, it is undoubtedly better it should be known—else will the said community for ever remain obstinately bent upon avoiding the only reasonable measures for its prevention.

The preventive remedy is no secret—yet its use, we see, is constantly declined, because, forsooth, some say, it would interfere with certain supposed sympathies of persons unknown, or rather perhaps (if the truth were more honestly told) because it would prove to be productive of inconvenience to an influential class of individuals interested; and all this, while the evil to be remedied—especially in its recent hideous shape, is held to be of uncommonly rare occurrence.

We really believe that many were beginning to think of the Edinburgh atrocities as a species of horror unique and *sui generis*, and that its foul perpetrator was a monster such as the world never saw before, nor were likely ever to see again: certain it is, that vivid impressions of this "deed without a name" (until one was given it after the arch criminal) were fast wearing out, and that many recurred to the facts almost as they would to some harrowing spectacle which they had witnessed on the mimic stage, rather than on that of real life. From time to time a faint alarm would be raised of the existence of similar criminality here and there through the country; but the rumours were ill-defined, and such frequently, as upon being discovered to be destitute of foundation, served but for merriment; until a period at length elapsed (however short) sufficient to tranquillize every apprehension about the reappearance of similar guilt, and to render people unprepared for a second revealment of "burking." This false security is now disturbed—the film is removed from the eyes of the short-sighted—and they are painfully awakened to the dread possibility of this horrible crime going on within the very range of their daily walks. We are sorry we cannot condole with them: we do not on this question belong to the party of the tender-hearted incredulous, but to our shame, (if they will have it so) be it spoken, we confess ourselves to be rather content with the disclosures which have been made, and strongly inclined to congratulate the public upon them. As to surprise—we are sorry to say that it is an occasion on which we can feel none.

Truth to speak, whatever may have been our surmises before, we never had the slightest doubt since the revolting confessions in Edinburgh, of the more than possible existence of the same system in London. This great capital, we thought, was too notorious as the Babylonian nursing-mother of every vice under heaven—too full of a progeny ingenious in every branch of iniquity, not to possess at least a reasonable number of adepts deep in the mysteries of the new crime, if mystery there can be said to be in any thing about "burking," except in the concealment. We scarcely wanted a Gibbon Wakefield to warn us of our danger; though the recent pamphlet of that writer appears most opportunely to confirm our view of the circumstances, and to convince all good citizens of the desperate gangs of wretches who hover around them every hour—ruffians, rabble, and desperadoes, capable and ready for the commission of any enormity. We were satisfied with what we knew of the habits and capabilities of one set of miscreants tolerated in the community—we had the resurrection-men before our eyes. Far be it from us, however, to raise a single prejudice against the individuals of that class who await their trial at the Old Bailey: we only speak of the class abstractedly, as the most obviously chargeable with a crime like that for which those individuals will be arraigned. Let us take a single human being of that order —one who has fallen, from whatever cause, into that lowest of all vocations, the business of a resurrection-man. His employment is a secret, or known but to a few ill-trusted wretches of his own condition; it suffices not, with all its dangers, for his maintenance; he cannot live by it alone; he becomes a thief—that is, if he have courage enough to raise himself a degree above the abominations of body-snatching; the instruments for grave-breaking are also those for house-breaking, and the same vehicle which is used for removing the contents of the grave serves also for conveying away stolen goods *. This, however, is the picture of a resurrection-man not quite destitute of the humanities—one not yet deadened to the impulses of a sort of stealthy industry, and willing to save himself from the shifts of the lowest and most brutal de-

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* See the evidence of the resurrection-men, A. B. and C. D., in the Parliamentary Report of the Committee of Anatomy, 1828.

gradation. But we can conceive another wretch, capable of adding one crime more to the catalogue of his iniquities. He sees before him the prospect of a prize, far more promising than the amount of nine out of ten of the robberies which he might have it in his power to commit; he has within his grasp the fruits without the risks of body-snatching; the temptation from the high premium in view is irresistible, even though the facility of concealment were infinitely less. Now comes the victim in his way—some solitary wanderer, helpless, or rendered so by the stupor of intoxication:—the deed is done!—the murderer is familiar with the appearances of death—he hampers up his *subject*, and hastens for the possession of the lucre which he has earned. The entire order of these miscreants must be abolished: the wretches who live by the spoliation of the grave—much less those who add murder to that offence—must not pollute society with their presence.

But the preventive remedy, as we have said, is no secret. Amend the laws as they affect the practice of dissection—if not for the safety of the public, at least for the preservation of our national character as regards our legislative arrangements. Foreigners should no longer be permitted to observe, that British law is in one part so dangerously absurd, as to ordain that certain of its enactments can only be complied with by a violation of other of its ordinances. Yet so it is; and these are the very enactments and ordinances which profess to regulate our profession, and have at length given rise to a species of crime unknown to former ages, and startling to the contemporaries of this. The time is come when it is imperative on the legislature to interfere: the laws *must* be amended—some provision *must* be made for dissection, *or the practice of anatomy must be prohibited altogether:* there is no alternative. This point, however, is so clearly expressed in a very sensible letter which appeared in a late number of the *Times*, that we cannot deny ourselves the pleasure of a short extract from it by way of conclusion.

"While such a traffic exists, there can be no security for the public. The legislature of the country can no longer turn a deaf ear to the voice which has so often warned them on the subject. They must interfere in one or other of two ways:—either they must prohibit the teaching of anatomy altogether, and allow medical and surgical science to go back in the next generation to what it was some centuries ago; or they must remove those obstructions which the common law of the land (as it is construed by the judges) has imposed on the study of anatomy by dissection. My firm conviction is, that the latter alternative is completely within their power: that by some very simple enactments the schools of anatomy may be supplied with subjects, without the smallest offence to public decency, without the smallest outrage on the feelings of individuals, and without any danger to human life. No one who will be at the pains to peruse the report of the Anatomical Committee of the House of Commons, and at the same time to make himself acquainted with what is done in France, Italy, Germany, and even in some of the United States of America, can, as I conceive, entertain a doubt as to this opinion being correct.

"If, however, those in whose department it is to make the laws by which we are governed, come to a different conclusion, let them at once and altogether prohibit anatomical instruction. The medical profession, that is, we who are now established in practice, cannot complain that our interests are in any way compromised if they do so. On the contrary, we shall thereby obtain all the advantages of a monopoly. We may then, if we are so disposed, contrive to exercise our art even in our dotage, for there will be no succession of younger and well-educated competitors to push us from our stools. The children and grandchildren of those who now exist, may suffer for the want of medical and surgical assistance, when they sink under the depressing influence or writhe under the tortures of disease; but to us, the existing race of medical practitioners, if we were selfish, we could wish for no better consummation than this[*].”

CHOLERA AT SUNDERLAND.

The good people of Sunderland really appear in no very favourable light. It seems pretty clear that the public safety is, in their estimation, a very secondary object when brought into competition with the sale of coals. We last week stated our opinion, founded on the evidence of the returns, that the whole truth was not made known; and Dr. Daun's letter, which has since been published, confirms the accuracy of our suspicions. Upon his representation, and that of Dr. Barry, it is to be presumed that the Privy Council issued the order which appeared a few days ago, by which medical men were every where enjoined to report their cases instantly, under certain pains and penalties. Many of our brethren have an idea that the law does not bear out the imperative tone of the mandate; but in this they deceive themselves. A reference to the quarantine act will shew that practitioners must obey, or expose themselves to summary punishment. Medical men cannot, however, be compelled to give their services gratuitously; and the proclamation alluded to will probably make them more chary in doing so; indeed, we do not see why the *doctor* should be the only professional man whose services are expected to be had for the asking, and quite as a matter of course. Regarding cholera as a contagious disease, we would suggest that an adequate number of medical men should be appointed in each district, to take charge of the sick, and that their practice for the time should be limited to that disease, otherwise how are the public ever to be safe against the constantly renewed dissemination of so diffusible a poison?—Medical advice, too, though rendered accessible, ought not to be forced upon the poor, else we shall have them accusing the doctors of poisoning them, and then murdering their attendants, in return for their services, as they did by wholesale in several parts of Russia.

As to Sunderland, no doubt is entertained of the disease which prevails there being the same as that which has been the scourge of India and the north of Europe. This has been decided by those who have seen both. We are still left without any official details, either of the symptoms or of the treatment; but from our own private sources we learn, that the course pursued by the disease is extremely similar to that which it followed in Russia, and that the same post-mortem appearances have presented themselves in the very few dissections which have been made. Brandy and opium have been extensively used as remedies; indeed, a hogshead of the former, ready mixed in due proportion with laudanum, was supplied by government for the use of the poorer patients. A cholera hospital has been established; but owing to the malicious report that the bodies of the dead were mangled, very few can be persuaded to avail themselves of it. One striking peculiarity, which is mentioned by several of the Indian writers, has been observed in the disease at Sunderland, namely, the tendency, in some cases, to sudden sinking after apparent recovery from the stage of collapse. Thus a man—we believe a sailor—was found with cold surface, blue skin, and imperceptible pulse; brandy and laudanum were freely administered: in some hours after, he was found sitting up in bed, drinking gruel, and expressing great relief; yet, from imprudent exertion, or other less obvious cause, the symptoms returned, were uncontrolled by the most powerful remedies, and he died next day. If there has been no cholera but what is given as such in the returns, then the disease has not made much head, though the rate of mortality has been great; but if we are to regard all the cases of diarrhœa and common cholera as *the* disease, then has the rate of mortality been small. We see that they have at length ceased to return as diarrhœa those cases, some of which proved fatal in 24 hours; but their discontinuance shews that this part of the return was, to say the least of it, uncandid. As to their "common" cholera, it is evidently a very *uncommon* form of that complaint. Dr. Barry has been to Newcastle, where he found the report of the disease having broken out to be groundless: he has now returned to Sunderland.

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[*] Times, Nov. 18, 1831.

WHAT IS MEANT BY LIVING WELL?

By far the most popular recommendation, as a safeguard against cholera, is "to live well," by which most people understand eating somewhat more than their accustomed quantity, and taking an extra glass or two of wine after dinner. No one certainly thinks of diminishing his allowance: even the habitual tippler protests that any withdrawal of his accustomed stimulus, however slight, leads to debility. It is high time that this pernicious misapprehension should be removed. "To live well," means to live "regularly," taking due nourishment, and no more—eating and drinking to support the body, not to pamper the appetite. Viewed in this light, we would ask how many there are who do not already eat and drink enough, and more than enough? certainly not one man of a hundred, in easy circumstances. Almost the only thing which prevents people from eating and drinking too much, is being unable to afford it. Our injunction to all those who talk to us about "living well," is never to take more than they can easily digest—to regard all fortifying against the cholera, which produces feelings of oppression, as so much assistance lent to the enemy—and more especially with regard to fermented liquors, that all exhilaration must be followed by its corresponding period of depression—and that he stands the best chance of exception who so regulates his diet and regimen as to be attended with the least variation of the pulse during the twenty-four hours. These objections to an increase of the "creature comforts," apply with equal force to persons drugging themselves with bark or quina. Where there is much screwing up, the nerves will more readily become unstrung, and then is just the moment for any prevalent disease to make its attack. We cannot, however, but commend the exertions which are every where being made to add to the comforts of the poor: let the rich be assured that this is the most effectual way of keeping the evil from their own doors: the best way for them to live *well* is to put it in the power of their less fortunate brethren to live *better*.

VOLUNTARY DISPOSAL OF THE BODY.

Dr. Macartney's document is now signed by upwards of 400 names, chiefly of professional men, who are anxious, by practical example, to remove the prejudices which stand in the way of dissection. We believe it is somewhat more than three years now since the document was prepared. Is it not time we heard of some *actual* example?—or who shall have the honour of the proto-martyrdom?

There is one obvious disadvantage attending this sort of post-obit bond in favour of anatomy: it leads the public to suppose that medical men are more interested in the matter than other people; whereas, there is no class of persons who would benefit so much by the entire prohibition of dissection as the present race of practitioners. Their profession would become a complete monopoly, and no fresh competitors could arise to "push them from their stools."

CONTAGIOUSNESS OF CHOLERA.

It is worth observing, that M. Brierre de Boismont, the companion of Le Gallois, and the first of the French physicians who published an account of cholera after returning from Warsaw, is a decided contagionist. Nothing can exceed the scrupulous accuracy with which he seems to have made up his mind on this subject. On one point he gives the most satisfactory assurances: the Polish army was free from epidemic of any sort previous to the battle (ot Iganic) on the 10th of April: the cholera made its appearance immediately afterwards: and on three distinct occasions that a healthy division of the army came in contact with the Russians, so often did the disease regularly develop itself.

Some papers on cholera, published under the sanction of the French embassy in Russia, and attributed to M. de Mortemart, furnish abundant testimony in favour of the doctrine of contagion: it is singular, however, that the non-contagionists have laid hold of these papers as favourable to their views, though M. de M. has distinctly stated in his 13th page, (what he frequently afterwards reiterates in substance) that "le cholera est une maladie très sensiblement expansive dans les lieux ou elle est arrivée, très subitement transmissible d'un lieu à une autre par l'intermediare d'objets materiels, soit vivans, soit inanimés."

M. LE GALLOIS.

This gentleman sunk, under the sequelæ of typhus fever, at Landsburg, in Prussia, on his return from Warsaw. He was only in his 26th year, but already distinguished as an able experimentalist in physiology. On the mission to Warsaw, he attracted general admiration by his energy, humanity, and talent. His opinions on cholera are incorporated with those recently published by his friend, M. de Boismont.

THE FINE ARTS.

M. **Dalmas**, from Warsaw, has presented the French Academy of Medicine with a series of drawings descriptive of the appearance of patients in the several stages of cholera. M. D. acted both the artist and the physician to his patients; and his work is said to have been executed with great skill.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Nov. 19.

Mr. Chinnock in the Chair.

Discussion on the Question, is Cholera contagious?

The meeting took place in the theatre, Great Windmill-street, and was very numerously attended; in fact, by far the most so that we have ever witnessed.

When the minutes had been read, Dr. **Granville** rose and proposed, that, after three nights' discussion, the question of contagion should be passed over; and he moved, that the society do now proceed to consider the nature and treatment of the disease. This motion was not seconded, and, in fact, could not be entertained, inasmuch as it had previously been decided and announced that the subject alluded to should come on this evening.

Dr. **Copland** thought that the number of direct and incontrovertible proofs of the infectious nature of the present disease, furnished by the speakers on his side of the question during the previous evenings, was so great as to render it unnecessary for him to go into this part of the evidence further than to advert to certain facts and considerations which had not been touched on by previous speakers. In respect to the direct proofs of the infectious nature of the pestilence observed in India, notwithstanding the representation to the contrary, they were so numerous and so strong, that two of the Medical Boards of the Presidencies had insisted on the existence of this property of the disease; and the third, after great hesitation, concluded by stating a decided opinion to the same effect. Even the non-infectionists furnished unequivocal proofs of the existence of the property against which they were contending. Of the various circumstances and phenomena of the disease there were none which in his (Dr. C.'s) mind more strongly evinced its infectious nature, and its origin from an infectious principle, than the specific and uniform character it has always presented, both in Asia and in Europe, and in every season, temperature, and locality. This specific and uniform appearance evinced a specific cause, reasoning from our knowledge of other specific diseases. Another circumstance, which he esteemed as distinctly proving its infectious nature, is the peculiar and particularly offensive odour which the disease is described, as observed, both in Asia and in Europe, to emit during life and after death,—a fact which is likewise observed in all other infectious maladies, and therefore shewing its close alliance in its infectious nature to them, particularly as the infection has been traced by the patients themselves to their having inhaled this morbid effluvium, in a very sensible manner, from the bodies of the affected.

Dr. Copland next adverted to the principal arguments which have been adduced by those who supported the opposite side of the question. They had assumed that, as the cholera of warm or temperate countries had, previously to the irruption of the present pestilence, been acknowledged not to have been infectious, this pestilence being cholera could not possess a different nature. But he insisted that the cholera of warm climates, which, in its severest forms, has been called *spasmodic cholera*, or *mort de chien*, was a totally different disease from the pestilence which has lately ravaged Asia and parts of Europe. The next argument on which the non-infectionists mainly depended, was the number of those exposed to infection who escaped it. But this was a circumstance common to this and all other infectious maladies, for all are not liable to be infected by even the most virulent—all are not predisposed. As to the boasted non-infection from inoculation, &c. it proved nothing; for no concentrated virus being generated, it could not be thus propagated,—the materials for inoculation were wanting. Who would expect to communicate scarlet fever, or measles, by inoculation? The non-infectionists also laid great stress upon the number of those attacked, who could not trace the disease to an infectious source; but this, he conceived, is not otherwise than is to be expected, when we consider that a considerable period frequently elapses, in all infectious diseases, between the exposure to infection and the development of the disease. Even admitting only a few days to elapse, who could refer to a particular instance, or even to the several instances, of exposure to infection, either direct or indirect, which may have occurred during that time. The occurrence of epizootics was laid hold of by the non-infectionists, as a proof of the origin of the pestilence in a certain unknown state of the air, &c.; but it should be recollected that this disease had prevailed for fourteen years, and during so long a period was it not to be expected that the temporary coincidence of a more than usual mortality amongst some of the lower animals, with the prevalence of this malady, should be sometimes observed, particularly as epizootics are a frequent occurrence after very wet seasons? but the fact is, that the coincidence has been by no means frequent. As to the argument that the disease would have been kept out by quarantine if it had been infectious, the inference is devoid of weight, and entirely unfounded in fact; for, during the extension of the disease in the East, no quarantine was anywhere observed; and in the few places where it was more recently resorted to, under circumstances which could render it strict, it proved completely successful. When the infectionists argue, that this pestilence manifests a similar mode of communicating itself to that possessed by true typhus fever, one gentleman gets up and denies that this fever is infectious, and consequently that this pestilence must be non-infectious also. But as the false prophets, from whose mantle he has caught his inspiration, have not put their opinions upon record, they cannot be investigated. Every man of experience, and possessed of a sufficient range of observation, independently of the vast body of evidence upon record respecting it, proceeding from the highest and most experienced sources, knows how to deal with this objection. Without referring further to the many considerations which press a conviction of the infectious nature of the present pestilence, Dr. Copland concluded, that, although quarantine regulations could not be expected to succeed in thickly-inhabited and inland countries, they might have been successful as respects this country, on account of its insulation, and that they had been therefore resorted to with the greatest propriety, and with reasonable expectations of excluding the pestilence.

Dr. George Gregory began by observing, that if the schoolmaster was abroad, so also was the teacher of physic, for in every society he found persons giving their opinions upon the contagiousness of cholera, and laying down the law upon one of the most difficult and abstruse doctrines in medicine—the origin and propagation of disease. Merchants, and lawyers, and men of fashion, undertake to decide, when they find that "the doctors disagree." But do the doctors really differ? In this society, three gentlemen only have stood pre-eminently forward as anti-contagionists. One of them, Mr. Searle, avows himself, however, as still open to conviction. Dr. James Johnson acknowledges himself a *contingent* contagionist. With those gentlemen, he (Dr. Gregory) would not contend, but he was astonished beyond measure with the line of argument supported by Dr. Granville. He was at a loss to understand, why the able and conclusive arguments of Dr. Macleod and Mr. Burnett had failed to convince him, who, on former occasions, had proved himself so staunch a supporter of the orthodox doctrine. In this dilemma, he remembered that "his adversary had written a book," and he sought, in its pages, a clue to explain the mystery. He believed he had found it. In that work (a letter on Plague and Contagion), Dr. Granville, after carefully dividing diseases into sporadic, simple epidemic, infectious or contaminating epidemic, and contagious, lays down two fundamental laws, or maxims:—1st, that epidemic diseases are never contagious; and 2dly, that contagious diseases are never epidemic. This, Dr. G. G. contended, was *prejudicing* the question. It precluded all argument, and all reference to fact. The cholera morbus was avowedly an epidemic, (that is to say, it attacked many individuals in the same place, at the same time, and in the same manner.) Now, epidemic diseases, according to the dictum of Dr. Granville, are never contagious. *Ergo*, cholera is not contagious. This was the logic of Dr. Granville. He (Dr. G. G.) protested against these *axioms*, and argued, that small-pox and plague are both epidemic *and* contagious. The error lay in drawing a broad line of distinction between contagion (whether mediate or immediate) and communication by a contaminated atmosphere, commonly called *infection*. Such a distinction does not exist in nature. Contagion and infection run into each other by insensible degrees. Some diseases, such as cow-pox and hydrophobia, require, for their propagation, that the miasm, or morbid matter, be applied to the *wounded* surface. Others, again, such as syphilis and tinea capitis, operate on the unbroken skin. The effluvia arising from the lungs and skin in plague, extend a few inches from the patient's body. In the case of small-pox, the contagion extends through a chamber or ward, and even through the whole house; nor does there appear any reason for confining, even within such limits, the *sphere of contagious influence.* In some cases, it may possibly extend even across a street. What he and those with whom he was this night associated contended for was, *first,* that the cholera morbus was one of those diseases *recognized* by Dr. Granville as infectious or contaminating epidemics, (such, for example, as typhus fever and scarlatina); and, *secondly,* that infection or contamination of the atmosphere, by the *breath* and diseased secretions, is only a *modification of contact.* This expression, which, in point of fact, strikes at the very root of both Dr. Granville's memorable axioms, is actually to be found in that gentleman's work, (page 26.) Dr. Gregory proceeded to remark, that an absurd prejudice exists in the mind of many against acknowledging contagion (or communication from man to man) as a means of diffusing disease. It has been held up as unscientific, and as *cutting* the knot which the pathologist cannot untie. It has been called the last resource of the ætiologist, and only to be admitted when all other means of explaining the propagation of disease failed. He (Dr. G.) could never see the meaning of this, nor understand why a physician should entertain any scruples in acknowledging the influence of contagions, or *contaminating* exhalations from the lungs, skin, and vitiated secretions of persons labouring under disease. But, that such scruples were felt, and that very strongly too, was obvious, from the line of argument pursued by several gentlemen in the course of the debate. Mr. Searle, for instance, attributed the origin of the disease among the Polish troops to a long harassing march, and to the swampy nature of the ground on which they afterwards passed the night. When pressed, however, to say, whether the Russian prisoners, who had *not* made that same march, were yet similarly affected, his reply was, *non mi ricordo.* This silence, upon a point which would go far to settle the question at issue, was very significant. Dr. J. Johnson seemed inclined to refer the cholera to some unusual heat of the earth's surface, adducing, in support of this, the new Volcanic Island in the Mediterranean. Another gentleman strove to connect the appearance of cholera with some alleged irregularities of *atmospheric* phenomena; but what evidence we had of such, except the hurricane in the West Indies, he (Dr. G. G.) was unable to say. Upon the whole, he viewed these vague attempts at explaining the origin and propagation of cholera as the shifts of ingenious men, whose intellectual pride refused to acknowledge the simpler theory of *communication by contagious effluvia.*

[Either Dr. Copland or Dr. Gregory alluded to the fact that Mr. Orton had become a contagionist; to which Dr. Johnson objected, that little importance could be attached to the circumstance, as he was confined by illness in the north of England.]

Dr. Gilchrist having claimed the privilege of a visitor to address the society, proceeded to observe, that the evidence upon which the history of cholera is founded is unworthy of confidence. It was clear, he said, how difficult it was to come at facts, when we observed what was passing before us at Sunderland; how, then, were we to believe statements relative to Astracan? In reply to Dr. Gregory's remarks on Dr. Granville's opinions, as quoted from that gentleman's book, he (Dr. G.) begged leave to observe that Dr. Gregory also had written a book, in which the account of yellow fever was founded upon evidence the most fallacious that had ever been adduced. (The bitterness with which this gentleman spoke in some parts of his address, bore, we regret to say, but too strongly the appearance of his being actuated by personal feeling.) The Chairman begged Dr. Gilchrist to confine himself, as much as possible, to the question before the society.

Mr. Searle offered some remarks in explanation of what had been said relative to him by Dr. Gregory; and in reply to the observations of Dr. Copland, that the pestilential cholera is a peculiar disease, different from all other diseases called by a similar name, he read a passage from (we believe) Jackson on Fever in the West Indies, in which a series of symptoms are described, the difference between which and those in some of the worst cases that he had witnessed in Poland, was to him inappreciable. This West India case, he had no doubt, was genuine cholera, though not appearing in an epidemic form.

Dr. A. T. Thomson rose to make some remarks on the character of the discussion, and spoke at considerable length on the importance of seeking truth rather than victory. The learned Professor then took an able view of the arguments on both sides of the question, and regretted that, in doing so, he was perhaps (really) obliged to repeat much that had been stated before by other members*. In conclusion, he declared himself perfectly open to conviction, and not wedded to the creed of either party, though the weight of the evidence greatly preponderated on the side of the contagionists.

Dr. Macleod said, that when on a former evening he had addressed the meeting, unluckily for his argument, though not perhaps for the patience of his hearers the hour had expired while he was yet speaking, and he had thus been obliged to conclude abruptly, and without drawing the inferences which the facts he had mentioned appeared to him to warrant.

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* It is on this account alone that we omit the details into which Dr. Thomson entered.

He would not, however, recapitulate what he had formerly said, but merely remind the society that he had dwelt upon three facts, as especially calculated to shew the manner in which the disease was propagated, viz. First, its mode of travelling from country to country, and from town to town, invariably appearing first on the frontier next to an infected district, or if on an island, always first shewing itself in a sea-port—in short, never being met with except where communication had been held with places in which it already prevailed. Secondly, the manner in which it frequently spread through the members of a family, or communicated itself to those who visited the sick, though residing in a different and distant locality. Thirdly, the immunity which had, in numerous well-authenticated instances, attended a rigid isolation, as at Peterhoff and Zarcozelo, where there were from 8 to 10,000 persons who remained unaffected, while it raged around them. But probably most men selected in their own minds particular facts from the general mass, and he would briefly mention one or two which, in his estimation, carried convincing evidence of the disease being propagated by human intercourse. In looking over the Indian reports he found that, as a rule almost universal, there was one spot in each great town which the pestilence long respected; while it decimated the adjacent district, one favoured habitation was either left untouched, or at all events was the last to suffer. This place was the jail. He doubted not (after the specimens of argument which he had heard on the other side,) that he should be told that the regular lives, and perhaps even the philosophic turn of mind, originating in their retired habits, gave to the dwellers in these secluded precincts their apparent immunity from the disease; but, unfortunately for this anticipated explanation, it happened that when once introduced it spread among the prisoners with great and relentless violence, showing that there was no want of susceptibility, but merely a want of the necessary contamination. Another circumstance which had made a great impression on his mind, was the behaviour of the pestilence on entering Russia;—the manner in which it travelled *up* the Wolga to the *north*, and then, crossing over to the Don near its source, pursuing a course diametrically opposite namely, *down* that river, to the *south*; thus travelling in two nearly parallel lines in contrary directions—a phenomenon perfectly intelligible if the plain straight-forward doctrine of it being carried by man were admitted, but wholly incomprehensible on any other. He had been accustomed constantly to hear, that they only thought the disease contagious who had not seen it, and this had long influenced his mind against the doctrine of contagion; but he had clearly proved, on the evening before last, that such was not the fact—indeed, that in India and Europe, wherever Boards or Committees of medical men had been appointed to sift the evidence, they had always come to the conclusion that the disease was communicated from man to man; and here he particularly wished to correct an erroneous impression which had been made by what fell from Dr. Granville. That gentleman had stated, that when a committee of physicians was appointed by the Emperor of Russia, to decide on the contagious or non-contagious nature of the disease, they reported that goods did not convey the infection, *having previously stated that it was not communicated by man.* [Dr. Granville here exclaimed, that he had not said so, and that he would reply to this part of Dr. Macleod's observations; which intention, however, he did not carry into effect.] Such certainly was, if not the identical expression, at least precisely the meaning of Dr. Granville's statement, because he (Dr. Macleod) had at the time argued, that Dr. Granville was probably mistaken, else the fact of the report alluded to having led to the rigid exclusion of travellers by triple cordons on the roads, while merchandize was allowed to pass, became perfectly inexplicable. He alluded to the circumstance at present only to state, that he had since consulted the documents themselves, and found, as he had supposed, that although the Russian Board thought the evidence of cholera being conveyed by goods was not sufficiently strong to warrant the stopping of merchandize, they had no doubt whatever of its being conveyed by men. It appeared further, by the official papers lately published, that the Board consisted of forty physicians, of whom thirty eight held the disease to be contagious, and only two advocated the opposite doctrine. Against the numerous instances in which it evidently spread from one individual to another, the non-contagionists adduced examples of persons who had been exposed to the infection, if such it were, and escaped; but the multiplication to any extent, of instances where persons had *not* taken the disease, could never neutralize those where they had taken it. Calculations of the number who escaped might lead us to form some estimate of the proportion of mankind who are susceptible of the disease; but negative evidence can never, by any increase, disprove the positive results opposed to it. Yet this was the constant argument of the non-contagionists. Like the man tried at the Old Bailey for sheep-stealing—"Ah! my Lord," said he, "if I had known that you would have condemned me because two men swore they saw me steal the sheep, I would have brought twenty to swear they did not see me." Another advocate for non-contagion had just arrived on the field—Dr. Hammet, and the event was announced by a flourish of trumpets, the newspapers declaring that he had "several cases which set the question at rest." Several cases to set such a question at rest! the very announcement shewed the utter ignorance of those by whom it was made of the simplest elements of medical reasoning. But let us inquire what the non-contagionists have to offer in lieu of that doctrine which renders the whole history of this malady, as to its mode of propagation, perfectly plain and simple. Why, nothing—absolutely nothing that will serve either to explain the phenomena, or even that is intelligible to others, whatever it may be to themselves. Thus we are told that cholera is produced by sol-lunar influence. In the name of common sense, what are the sun and moon about now more than they have been since the world began, that they should be accused of engendering this pestilential offspring? Others speak of subterraneous influence. Why do they not tell us what that influence is, which prevails alike in marshy jungles and on arid plains, on the summits of mountain ridges and in the depths of valleys—in every diversity of soil, climate, locality, and all that render things dissimilar. *Subterraneous* influence, not confined to the earth, but in some cases disregarding many hundred miles of intervening sea. This same subterraneous influence appeared to him to be but a subterfuge calculated to mystify rather than explain. It was in vain to grapple with things which had neither form nor body; they were too refined to comprehend, therefore too subtile and visionary for argument to impress. He could not conclude without remarking on the gradual conversion of the non-contagionists. [A loud expressive *hear!* from Mr. King.] As the gentleman had given to his exclamation such an expression of incredulity, he would adduce some instances. At these discussions those on the opposite side had begun by holding cholera not to be contagious at all; now they talked of it not being *more contagious* than typhus, or some other disease assumed to be contagious—this was notorious; and it was quite obvious that they who argued on the degree of contagion, of necessity assumed the existence of that property to some extent or other. Again, it was known to all that Dr. Russell went to Russia a non-contagionist, but that the film had fallen from his eyes, and he returned holding that doctrine no longer. Dr. Lawrie, an intelligent physician, who had seen the disease in India, and had been a non-contagionist, had just acknowledged, in the Glasgow Medical Journal, that farther evidence had changed his opinion; and though last, not least, was Mr. Orton, known as the author of a very valuable work on cholera, formerly a staunch non-contagionist, and who now said that the arguments against contagion were "as dust in the balance"—such were his very words—when weighed against those which are now opposed to them. Dr. James Johnson has made light of this, because he says Mr. Orton has not of late seen the disease, having been confined to a sick bed. Granted; but these circumstances give additional weight to his opinions. He is now (said the speaker) removed from the scene of action, and can weigh the evidence without the prejudices of a partisan; that his recantation has taken place on a sick bed is the most convincing proof that it is—sincere. It was remarkable, that though the daily press was almost unanimously against contagion, the Reviews and Medical Journals were nearly all in its favour; and as they were better judges, this shewed where the evidence preponderated—indeed, one would think it scarcely possible that two opinions *could* exist with regard to a question where the Medical Gazette and the Lancet embraced the same side, and were seen fighting under the same banner.

Dr. Stewart begged that Dr. Copland would repeat, or more fully state, his opinion relative to the peculiar odour of cholera constituting a discriminative character of the disease.—[Dr. Copland explained: but we need not repeat what has been already incorporated in Dr. C.'s opening remarks.] Dr. Steward, in resuming, observed that it appeared to him, after the very convincing arguments of the contagionists, that there could not be and there really was no difference of opinion in the society with respect to cholera; that all the members seemed to agree that the disease was contagious under certain circumstances.

Mr. Burnett begged to be allowed a few words in explanation.

In maintaining the contagious character of cholera, he never intended to assume, indeed never did believe, that it or any other contagious disorder was always, and in every case, contracted by all who became exposed to its influence. On the contrary, he had discussed at some length the distinction between contagion and infection, and had insisted particularly on the very different degrees of communicability to be noticed in different disorders. Therefore the anti-contagionists, who, because they can cite the cases of some persons who have been exposed to the influence of cholera and have nevertheless escaped, think thus to confute the contagionists, only knock down a chimera themselves have raised; for the contagionists have again and again referred to the truism, that many diseases which are sporadic in their origin may become epidemic and contagious—as, for example, typhus fever. Indeed (continued he) there is as much difference between a disease being *communicable* and *communicated*, as between an object being *visible* and being *seen*. An object may be, indeed many objects are, visible, which nevertheless are not necessarily seen; and a disease may be, indeed many diseases are, *communicable*, which nevertheless are not necessarily *communicated*. Moreover, the contagionists have the temerity to attempt to prove a negative, and presume to declare that, because one, two, three, or more persons have been with patients suffering from cholera, and have escaped, therefore that cholera is not contagious. But can any conclusion be more illogical? If the cases of escape were ten times more numerous than they are—if, for every three, there were three thousand—they would avail nothing against even one well-marked and decided case of direct communication; all that they could legitimately be brought to prove would be the comparative facility of communication—the relative degree of contagiousness. Indeed, to this point do most even of the former non-contagionists seem to be veering; for one declares that it is *no more* contagious *than* typhus, and another that it is *contingently* contagious, Dr. Gilchrist being the only spokesman who now dares to uphold the unblenched banner of non-contagion. But what do these gentlemen mean by *contingent contagion*? Are not all contagious diseases *contingently* contagious?—and are not these *contingencies* the predisposing states of the patient's health? This, which they now so readily grant, is all that the contagionists ever have desired: yet why, when they are with us as to *facts*, are they not with us in *names* also?—for what will the public understand by their strenuously repeated affirmations that cholera is *not contagious?* Will the public understand that it is *contingently contagious?*—will they not rather suppose them to mean that it is NOT *communicable?*—and then, should such a dire disease be suffered unimpeded to devastate this great town, will not the anti-contagionists be rightly accused of breaking the word of promise to the sense, even if they seem to keep it to the sound.

As to the statement of the learned member, Dr. Thomson, that the speakers have appeared to him to be contending for victory rather than for truth, it may be a sufficient answer to acquaint him, that had he been present at the former meeting he would have known that the very course he himself just now pursued, to arrive at truth, was the course that the contagionists have several times journeyed over for that very purpose; that Dr. Macleod, Dr. Sigmond, Mr. North, and himself, have over and over again been obliged to prove and re-prove the facts of communication; that they had traced the progress of the disease from place to place—had shewn that as man travelled it travelled, that as man stopped it stopped; that when communication was interrupted its progress was interrupted; that as travellers journeyed round, it journeyed circuitously with them; and in continuation of these arguments, Dr. Macleod had this evening, among other points, well noticed the important fact that the last places infected with cholera had been the gaols. He might likewise have shewn, not only that it attacks last those places which are most secluded, but also that it attacks first those which are the most public; for where does cholera first break out?—why, in sea-ports and frontier towns. Where has it first set its foot upon our own land?—why, it has been true to its general rule, and chosen a sea-port town for its invasion. Mr. B concluded by denying that the contagionists formed any party; he came open to conviction; he came, not only willing, but anxious to believe in the non-contagious nature of the disease; but he could not shut his eyes to facts, nor resist the force of evidence. He would not follow truth so far, and so far only as she concurred with prejudice, but was ready to give up all preconceived opinions, and follow the path her unerring finger should point out, well knowing, that although both Socrates and Plato are worthy friends, that the better friend is truth.

MR. KING appealed to the society if his was not the true view of the matter. All diseases were contagious. Was not lues venerea a contagious disease? Very well. And typhus? and phthisis? If nobody rose to dispute it with him, he would take silence for consent (loud laughter). He was ready to prove it, if he was to stand there on his legs till three in the morning (cries of question). With regard to cholera, he had only to say that he was proud of agreeing with Dr. James Johnson; and denied that that gentleman and he had changed their opinions as somebody had stated (laughter.)

A member proposed that a resolution to that effect should be adopted by the society; but this was very properly overruled.

After much desultory conversation, which protracted the time considerably beyond the usual hour for breaking up, it was moved by DR. J. JOHNSON, and carried unanimously, that on next Saturday (this) evening, the Nature and Treatment of Cholera should be discussed.

NOTICES.

Mr. Addison's paper has been received, but is unavoidably postponed, owing to the great press of matter on the engrossing subject of cholera.

The communication of Dr. Baron came to hand in time, but it was impossible to comply with his request by inserting it this week.

The papers of Dr. Marshall Hall, Mr. Key, Dr. Campbell, (Edinburgh), Mr. Battley, Dr. Wilkinson, a Constant Reader, Philalethes, a Chemist and Druggist, Y. Z. M. Copland Hutchison, a Correspondent (whose name we cannot make out), dated *Helston*, Mr. Bennett, Mr. Spence, Non-Nemo, Mr. Ward, Mr. Knott, Dr. Badham, Mr. Bushell, Mr. Crowdy, Mr. Slater, Mr. Green, Physicus, Dr. Stanley, Mr. Lawrance, have been received.

A Pupil (Borough) next week.

Philomeides: we are sorry that at present we cannot.

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LECTURES
ON
THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

BY DR. ELLIOTSON.

PART I.—LECTURE XXXIII.

Remittent Fever.

I NOW enter, gentlemen, upon the subject of remittent fever, which resembles both intermittent and continued fever, and partakes of the character of each. Essentially it is the same as intermittent fever. It arises from the same cause, I believe—malaria; but either through a modification of the malaria, or the influence of such causes as predispose to, or excite, continued fever, we have only remissions;—not intermissions, but remissions. The disease is really continued, although remitting, having a relaxation of severity at different hours.

This disease occurs in hot climates particularly, and in cold climates during hot seasons; it therefore occurs when and where there is the greatest abundance of vegetable matter, and all circumstances that are the most favourable for decomposition and the production of *aguish* fevers; and it also, you see, occurs when there is the greatest prevalence of other causes which are likely to excite *continued* fever—excitement and relaxation of the body, and disturbance chiefly of the abdominal organs.

Duration.—When remittent fever is acute, it seldom, I believe, lasts more than six weeks. There is continual fever day and night, and every day, but the patient is much less hot at one time than at another, so that, although he has constant feverishness, he feels himself far less ill at different intervals.

Remittent Fever marked by paroxysms of Ague.—Sometimes, besides the continual fever, there is, in addition, a regular paroxysm of ague. I have seen a person hot every day and every night constantly, except perhaps every other day, when he had a cold fit, and then the heat would become more intense, and sweating occur; yet when the sweating was over there was no intermission, but the patient was hot again.

The usual form is continued feverishness, with a great aggravation of it at particular periods; or, if you choose to put it in another way, the symptoms of continued fever, but a relaxation of these symptoms at particular periods. But besides, I have seen, although I do not recollect it being described—I have seen continual fever, with a paroxysm of ague coming on at regular intervals in the midst of it.

Marked by Chilliness or Sweats.—Sometimes, instead of remissions, or an aggravation of the feverish symptoms, you will have during this continual fever the remittent nature of it shewn only by occasional chilliness. I have frequently discovered that continued fever, as it appeared, was remittent fever, by noticing, that although there was continual feverishness, heat, and thirst, yet the patient in the midst of it was frequently very chilly; and on other occasions by observing that, in the midst of the heat, there was from time to time profuse sweats. The nature of the disease was pointed out to me either by the occurrence, in this continued fever, of chilliness—even rigors—from time to time, or the occurrence of sweating from time to time. The correctness of this opinion has been proved, by giving the patient quinine, and getting him instantly well. I think, so far as my own observation goes, that remittent fever shews itself sometimes in these two forms.

Intermittent fever will become remittent, if the causes of continued fever be superadded after intermittent fever has began. Supposing a patient has ague, and goes through great fatigue or want of rest, or is exposed to wet or cold, he may be thrown into continued fever. These circumstances alone would not cause ague, but ague existing, the disease becomes remittent fever. The causes of continued fever are applied to a constitution labouring under intermittent fever, and then you have remittent fever. Frequently, however, the disease is produced from the very first.

Chronic Remittent Fever.—Dr. Macculloch imagines that intermittent fever is often a chronic affection—that it is often mistaken for hypochondriasis—and that the paroxysms are not observed, because they occur in the night. Now, I am persuaded that he is correct; I am persuaded that many persons have long-continued remittent fever—frequently for many, many months, and, its true nature not being detected, it is thought to be some indescribable complaint. I have now under my care a patient who appears to be in this state—a man who is frequently very hot, complains of heaviness in the head, is affected in his mind, and has great depression of spirits. I believe the observation of Dr. Macculloch to be right. This man was taking five grains of sulphate of quinine three times a-day, without any effect; but on increasing the dose to ten grains, I found him better. I have seen many such cases, and they long puzzled me—oppression of the head, heat occasionally in the night, and great depression of spirits.

Intense Remittent Fever.—The acute disease frequently occurs in a dreadfully violent form, and it is as dreadful in rapidity as in violence. It is in hot climates that this particularly occurs. This form of the disease is so violent, and so acute, that there is at first delirium and violent vomiting, with an absolutely roasting heat at the stomach, great pain, tenderness, and agonizing distress, at the epigastrium.

Besides these local symptoms of the head and the epigastrium, there are all the signs of a violent inflammatory fever; and all these symptoms, both local and general, are soon followed by the most awful debility, and by all the signs of typhus.

The fever of the East Indies is a bilious remittent fever—a violent and acute remittent fever, with great bilious symptoms, and at Bengal it arises from the overflowing of the Ganges, and the consequent fall of the waters in November and December, acted on by a high temperature. After the Ganges has overflowed, the waters fall in November and December, and, through the heat, such emanations are produced that they cause this fever.

After these violent symptoms of delirium as to the head, and pain and vomiting as to the epigastrium, there is, after a certain time, a remission and a sweat, which occur about twelve or fourteen hours after the attack; and the pulse, from being 130, may fall down to 90, and the mind is then restored and the delirium ceases. The remission, however, is but short; the symptoms soon reappear, and then they are succeeded by the awful debility to which I have referred. If the disease prove fatal, there is very soon no remission at all; the gastric symptoms become more violent than before, the tongue becomes black, the pulse small, and the patient sinks. If it be fatal, it generally proves so, we are told, (of course I am only speaking from information which I have derived from authors), between the third and the seventh day, but occasionally it has been known to last fifteen or even twenty days.

The fever of the East Indies is a violent bilious remittent fever, accompanied by great derangement of the liver and stomach, and it occurs in various other parts of the East than Bengal. This disease has appeared with some other symptoms in the Mediterranean, in North America, and in the West Indies. In the Mediterranean it has appeared at Rochfort, Bourdeaux, and at Lisbon; in the south of Spain, at Cadiz, Gibraltar, and Minorca. There is here, in addition to the symptoms I have already enumerated, yellowness of the skin, and a vomiting of black matter. The disease, thus varied, has been called *yellow fever*, from the yellowness of the skin.

In America it is said that generally thirty-six or even seventy-two hours elapse before the remission takes place.

Black Vomit.—This black vomit, which is as much characteristic of yellow fever as the yellowness of the skin, is thought to be bile a little changed from blood—imperfectly formed bile, or even blood only a little changed; indeed, some suppose that, of the two, it would be more proper to call it blood than bile. It is imperfectly formed bile, or blood but slightly changed; indeed it is said that it has been found on the surface of the stomach when there was none in the duodenum, as though it had not come from the liver.

This black vomit is said to be by no means acrid. Dr. Physie, of America, says it is tasteless, and that he dropped some into his eye, and the organ did not become irritated; that, indeed, he found an abundance of it in the stomach, when a different fluid was observed by him both in the gall-bladder and the gall ducts. It is decidedly of the appearance of charred blood—of blood that has undergone certain changes, and is poured forth from a mucous membrane. Another gentleman has carried his experiments with the black vomit farther. Dr. Ffirth tells us that he procured two ounces from a patient, (whether dead or alive I do not know,) and that he drank it undiluted, and found it harmless. Before this, he had drunk a large draught of it, mixed with water, with impunity; but, in its genuine state, he took two ounces, and said he was none the worse for it. He wished to try it in a solid form, and therefore made an extract into pills, and he says that he found as little inconvenience from them as he had previously done from his black draughts. The fluid and concretion of melanosis is also generally inert.

Cause of the Yellowness of the Skin.—In regard to the yellowness of the skin, it is ascribed by some to bile pervading the system, but there are very great doubts as to whether that is its origin. The bile in the disease is very abundant, and may be regurgitated by violent vomiting into the veins, through the compression of the parts; and, indeed, bile is abundant in the urine. There is a great collection of bile in this disease, and it may find its way into the veins, and more or less will go into the circulation, but there is no obstruction. At any rate it is certain that, if it be bile, it is not obstruction that produces regurgitation of it, for the fæces are well tinged with bile. One reason for saying that it is not bile is, that it will occur in patches. It is not more visible in the eyes and nails than in other parts of the body, and it is not diffused pretty equally throughout the body, as we observe in jaundice, but it occurs in patches, as ecchymoses would do; and it would rather appear to owe its origin to the blood being changed—to the serum, with a few red particles, escaping more or less into the cellular membrane, the blood having undergone a change. It is altogether analogous to ecchymosis when nearly gone, which does not arise from bile, but from an effusion of blood that is subsequently in a great measure absorbed, the portion which remains giving a yellow colour. Sir Gilbert Blane says that this yellowness has spread from bed to bed without any increase of the malignity of the disease; that when yellow fever has prevailed in the West Indies, he has observed it spread from bed to bed, and bearing no proportion to the malignity of the disease; that it appeared in two vessels, the Alcides and the Royal Oak, when there was only a slight indisposition. He also says, that, unlike jaundice, it begins usually about the parotids; there is usually fulness and flushing of the face and neck, but particularly about the parotids: here there is the most blood, and here it is that the yellowness appears in the most marked manner.

Influence of Temperature. — This yellow fever (or *causos icterodes*, as some call it) is apparently another form of bilious remittent fever, and it occurs only in countries and in seasons where the heat is such as would destroy or arrest the plague; and it occurs, too, in hot seasons, such as would usually put an end to contagious typhus fever, when that is prevailing. Yellow fever, or bilious remittent fever, are always extinguished by cold weather; as soon as the cold sets in, they cease. It is said that the blacks, in America, are usually less violently affected than the whites; but that if they go into a cold country, and reside there for a time, and then return to America, they suffer equally with the whites. Persons who are unaccustomed to a high temperature, suffer, it appears, the most from this disease. Blacks coming from hot countries, or having descended from parents born in hot climates, suffer less; but if they reside in cold climates their constitution partakes in some degree of that of the white, and then they suffer far more than others of their black brethren.

Cold countries, and the cold seasons of hot climates, are free from this disease. It generally occurs only in the tropics, and elsewhere when the heat is as high as in the torrid zone; but very intense heat in cold climates has been known to occasion it. If the temperature have accidentally been as high as in a hot country, then even a cold climate has been subject to it. Sir John Pringle, for example, says that he has sometimes seen yellowness of the skin in Flanders. Dr. Brocklesby mentions seeing it in 1758 in the Isle of Wight. Dr. Home saw it in 1743 at Worms, in Germany. All those places which are subject to it have a quantity of wet dead stagnant vegetable matter. It prevails in the West India Islands, in Charlestown, Norfolk, Providence, Philadelphia, New York, Baltimore, and Boston. Those places that have the largest quantity of vegetable matter for decomposition when the hot weather comes, and have the most moisture, are most subject to this yellow fever. It occurred at Lisbon till the earthquake happened. It occurs also at Cadiz, Bourdeaux, Rochefort, Seville, and Gibraltar. Now the latter place is situated high enough; but although high in reference to the sea, the town is low in reference to the mountains. The rain, it is said, that runs off 4000 acres streams towards the town, bringing with it from thence a quantity of vegetable matter, as well as fragments of vegetable refuse from the markets. Even straw has been said to afford the source of this disease. Sir John Pringle says that the straw was very injurious to the army. The rocky soil here increases the mischief, as it causes the water to remain.

Notwithstanding, however, it would appear that yellow fever is the product of malaria: there has been a difference of opinion entertained on this subject at different times. A Mr. Vines, a planter and physicker in the West Indies in 1647, writing from Barbadoes, ascribes the yellow fever to the Lord's heavy wrath, and not to any thing in the climate. The island was not cleared for a considerable time after he wrote. The disease prevailed then to a greater extent than it does now. A Spanish officer named Armesta, who ascribed the disease to local situation and atmospheric causes, was actually at one time arrested, and obliged to retract his views, as being "false, dangerous, and seditious opinions." So violent was the opinion entertained at one time, that it had nothing to do with local situation, that 1500 copies of this officer's book were burned. In 1800, the government had a different opinion, and ascribed it to something in the atmosphere, which really is the case—that is to say, to miasmata.

Contagion.—A great question has arisen in the western hemisphere, and in the Mediterranean, as to whether yellow fever is contagious or not. Dr. Ffirth, of Philadelphia, who has tasted the black vomit in the way I have stated, says that he has frequently inoculated himself with it, and also with the serum, saliva, &c. of patients labouring under yellow fever, but that no prejudicial effect was produced. It is said that it is never propagated a mile from Philadelphia, and that therefore it must owe its origin to local causes. Dr. Rush, who was a celebrated American physician, and a violent contagionist, contended that this was a contagious disease. He afterwards retracted his opinions, and begs the forgiveness of the friends of science and humanity if the publication of his former opinions had had the effect of increasing the miseries attendant upon the disease; and, indeed, he says, such is the pain he feels in recollecting that he ever entertained or propagated the opinion of its being contagious, that it will always lie heavy at his heart, and deprive him of the pleasure that he might otherwise have derived from a review of his attempts to fulfil the duties of his public station, although, he says, he was misled by Dr. Lining, and other writers. "I am aware," he says, "of the influence which changes in opinion have upon a medical man's reputation, but still I would cheerfully make a sacrifice of that kind, could it avert the evils which are connected with the belief of its being a contagious disease." The mode in which a false belief in contagion does harm, is by preventing the healthy from attending upon the sick, by stopping commercial intercourse, and by allowing the patients to remain in the unhealthy spot which really causes the disease.

Dr. Stevens's hypothesis.—Now it is impossible for us living here to know any thing about these matters, for there is as much evidence on the one side as on the other. In the East Indies it is never thought to be contagious, but in the West Indies it is so considered by a large number of persons. The fever in Gibraltar gives rise to a diversity of opinion, and there are as many protestations as to its being contagious as there are as to its arising from malaria. Dr. Stevens thinks that he has discovered the reason of all these discrepancies of opinion. He says there are three distinct yellow fevers in the western hemisphere. In the first place, there is one in which there is no contagion at all, which is a climate fever, and arises simply from excessive heat; that it occurs to new comers, who are operated upon violently by the intensity of the temperature; that great bilious symptoms arise, but that there is nothing contagious in it. He contends, next, that there is the remittent fever, the yellow fever, arising from the local cause of malaria and heat together, which is seen by every body, and acknowledged to exist by most. Besides that, he says there is a yellow typhus fever—a contagious fever, which he has been able to trace to the negroes in America who have come from Africa: it is a contagious disease, attended with yellowness of the skin, and brought there by the Africans. He says that all these three fevers are attended with yellowness of the skin. What his observations are, or how numerous they may be, from which he has drawn these conclusions, I cannot tell. He says that the symptoms of these are all different at the first, but that after a time they are all the same; and that although the general treatment of the whole is the same, yet that in their minutiæ they all differ; they all require a modification of treatment. I cannot say more upon this subject—I give no opinion upon it, but I should think it likely that it is the case; that there are two or three different kinds of fever, but the intensity of the heat occasions them all to be characterized by great yellowness of the skin, some by a depraved state of the blood poured forth under the skin, and others by jaundice. I have no doubt but that Dr. Stevens's facts will be numerous, and that many of his opinions will be found to be well established. I believe that the greatest authorities in America are satisfied that the yellow fever, as it prevails there, is, for the most part, not contagious; but that it is sometimes contagious, one would think appears very certain.

I made a note of a conversation which I had with Dr. Stevens the other night, for the purpose of communicating it to you. He mentioned one instance of yellow fever occurring in a family situated a great distance across the sea from any place where the disease had prevailed. The captain of a vessel was in a port where yellow fever prevailed, and being continually among the people, his clothes and his linen were impregnated with it, and he left the place. I am not sure whether he had had the disease himself, but he had been in the midst of those who laboured under it. When he left he cheated the quarantine, and went to another place, where no fever had prevailed, with his linen unwashed. According to the quarantine regulations, his linen should have been washed; but I understand that, under under those regulations, washing is very dear, and that every thing is charged double. He, therefore, to save this double expense, contrived to take some of his dirty linen with him, satisfied that the disease was not contagious. The linen was taken out by his family in this place where yellow fever had not prevailed, and in a very short time one died, and another died, of the sisters and maids who washed it for the captain, and an old woman, who lived in another part and came to help them, was also seized with the disease, and it spread from house to house all through the town, and produced very great devastation. Dr. Stevens says, that he knows it was the contagious form; that, in fact, it was what he calls *African typhus.* He took a voyage for the purpose of inquiring into the circumstances. The sisters said that it arose from the dirty linen; and it appeared that he was able to trace it with the greatest accuracy. He also mentioned his opinion that cholera was contagious, but he is satisfied that the contagion is not efficient at first; that you may be exposed to a person labouring under African typhus, as he calls it, and that you may be exposed to a person labouring under cholera, without any fear of catching it, while you are in his first emanations; but that if you be exposed to these after they have passed from him many days, then you stand a chance of getting the disease; that what proceeds from the body is not at first efficient, but undergoes a change, which enables it to produce the disease; that it is not a perfect poison when it is first produced. He reasons in this way: that many persons who have visited cholera patients have escaped, but they have suffered as soon as they have been exposed to fomites, in which the secretion was contained. How that may be, I do not know. He also says that he has ascertained another circumstance, that, before the disease breaks out, the blood will shew the disease. He says that this may also be remarked in scarlet fever. A medical man found that he was ill, and he was then bled. The blood was shewn to Dr. Stevens, who found that it would not turn red with the usual neutral salts. He stated that he was satisfied the gentleman was about to have a contagious disease, and, in three or four days after this, scarlet fever occurred. He says that he has seen yellow fever in the blood before the disease appeared; and that, in all instances where the disease subsequently appeared, he found the blood was of such a character that it would not redden on the application of a mixture of neutral salts. Of course, all these things are exceedingly crude at present; but it is the opinion of many eminent chemists, that we are upon the eve of some important discoveries with regard to the nature of all these matters; that great light will be thrown upon the animal changes which take place pathologically in the various fluids of the system, and on the nature of many diseases, by the treatment which Dr. Stevens has recommended. I feel satisfied with the truth of much that has been said, and I feel satisfied that a strong and new light will be thrown upon a number of these matters, respecting which we are all at present in the dark.

The latent period of this yellow fever is thought to be from two to ten days.

Treatment.—With regard to the treatment, the best writers agree that there should be copious venesection in the first instance. Whether it be the fever of the western hemisphere, or the mere bilious remittent of the east, cold affusion is always serviceable, or cold ablution. Great attention should be paid to any local inflammation that may occur, and local bleeding may be demanded. As to emetics, they cannot be employed if there be any tenderness of the epigastrium on pressure. Mercury is found useful in the first instance, but not afterwards. When you have symptoms of violent inflammation it is very useful, but still a person may die with his mouth sore, if it be given too late, or a proper degree of bleeding is not practised. Of course moderate purging is necessary; but as soon as the remission occurs, sulphate of quinine should be poured in in great abundance; and Dr. Stevens says, that when the symptoms of typhus fever come on, after venesection, and mercury, and cold affusion have been freely had recourse to, no time should be lost in pouring in neutral salts. He says he has great evidence to shew, that where these fevers have proved fatal to a frightful extent, the mortality has been reduced almost to nothing by practitioners having listened to his advice. By giving neutral salts, as the carbonate of soda, the oxymuriate of potass, and common salt, every hour during the disease, the beneficial effect has been infinitely beyond what any one could have anticipated. I know that this gentleman states nothing as a fact but what is strictly true. If he merely give an opinion, of course, like other men, he may be wrong; but upon whatever he states as a fact, you may place the greatest reliance; and if he say that a certain number of cases have been cured out of the whole number, you may depend upon it that they have been cured.

Sol-lunar Influence.—Before I conclude the subject of these fevers in hot countries, I must mention that in the East Indies the sun and moon appear to have a great influence upon them. Dr. Balfour has written a work upon what he has called the sol-lunar influence, and he proves that at new and full moon bilious remittent fever is most easily taken; that at new and full moon relapse is most common; that at new and full moon the intensity of the disease is greatest; and that those suffering from pernicious fever, which has destroyed the constitution, are particularly affected at that period. This may appear incredible or whimsical, but I have no doubt of the fact. I have seen medical men who have practised in the East Indies, and they say it is well enough for us not to believe it, but they have suffered enough to know that the matter is perfectly true. I do not suppose that it is to be ascribed to the direct influence of the sun and moon upon the body, but that it arises from the operation of these upon the tides. The new and full moon, with respect to the elevation of the water, may have very great effect; and that, again, may have an effect, by causing more or less malaria to be disengaged, so that the atmosphere is more impregnated with it at those periods than at others. I believe that there can be no doubt whatever on the subject, that at new and full moon in the East Indies fevers are most easily taken, relapse is most common, fevers are most intense, and those who have suffered most from these fevers feel a strange sensation about them, just such as we continually find here when the east wind blows upon a person who has previously laboured under ague.

I forgot to say that many epidemics described in ancient books as plagues and pestilences, were really remittent fevers.

ON THE STRUCTURE OF THE HUMAN PLACENTA, AND ITS CONNEXION WITH THE UTERUS.

By Robert Lee, M.D. F.R.S &c.
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(From the Philosophical Transactions.)

In the year 1780 Mr. John Hunter presented a paper to the Royal Society, in which he laid claim to the discovery of the true structure of the placenta and its communication with the vessels of the uterus. The following is the history of the appearances which he observed in the dissection of a woman who had died undelivered near the full term of utero-gestation, and from which appearances his conclusions were drawn respecting the natural structure of these parts. The veins and arteries of the uterus having been injected, an incision was made through the parietes, at the anterior part where the placenta adhered to the internal surface. Between the uterus and placenta lay an irregular mass of injected matter, and from this mass regular pieces of the wax passed obliquely between it and the uterus, which broke off, leaving part attached to that mass; and on attentively examining the portions towards the uterus, they plainly appeared to be a continuation of the veins passing from it to this substance, which proved to be the placenta. Other vessels, about the size of a crow-quill, were seen passing in the same manner, although not so obliquely. These also broke on separating the placenta and uterus, leaving a small portion on the surface of the placenta; and on examination they were discovered to be continuations of the arteries of the uterus. The veins were next traced into the substance of what appeared placenta; but these soon lost the regularity of vessels, by terminating at once upon the surface of the placenta, in a very fine spongy substance, the interstices of which were filled with yellow injected matter. He then examined the arteries; and tracing them in the same manner towards the placenta, found that, having made a twisted or close spinal turn upon themselves, they were lost on its surface.

On cutting into the placenta, he discovered in many places of its substance yellow injection, and in others red, and in many others these two colours mixed. The substance of the placenta, now filled with injection, had nothing of the vascular appearance nor that of extravasation, but had a regularity in its form which showed it to be naturally of a cellular structure, fitted to be a reservoir for blood.

From these appearances Mr. Hunter infers, " that the arteries which are not immediately employed in conveying nourishment to the uterus go on towards the placenta, and proceeding obliquely between it and the uterus, pass through the decidua without ramifying. Just before entering the placenta, after making two or three close spiral turns upon themselves, they open at once into its spongy substance, without any diminution of size, and without passing behind the surface, as above described.

"The veins of the uterus appropriated to bring back the blood from the placenta, commence from this spongy substance by such wide beginnings as are more than equal to the size of the veins themselves. These veins pass obliquely through the decidua to the uterus, enter its substance obliquely, and immediately communicate with the proper veins of the uterus. This structure of parts points at once to the nature of the blood's motion in the placenta. The blood detached from the common circulation of the mother moves through the placenta of the fœtus, and is then returned back into the course of the circulation of the mother, to pass on to the heart*."

Dr. William Hunter's description of the vascular connexion between the uterus and placenta coincides with that of his brother; "for it seems incontestible (he observes) that the human placenta, like that of the quadruped, is composed of two distinct parts, though blended together; viz. an umbilical, which may be considered as a part of the fœtus, and an uterine, which belongs to the mother; that each of these parts has its peculiar system of arteries and veins, and its peculiar circulation, receiving blood by its arteries and returning it by its veins; that the circulation through these two parts of the placenta differs in the following manner:—in the umbilical portion the arteries terminate in the veins by a continuity of canal, whereas in the uterine portion there are intermediate cells, into which the arteries terminate, and from which the veins begin†."

It is a singular fact, that these celebrated anatomists should both have asserted their claims to the merit of what they supposed to be the discovery of the true structure of the human placenta, and its connexion with the uterus, and that their controversy on this subject should have loosened those bonds of affection which had united them together from their earlier years‡.

Noortwyck, Rœderer, and Haller, had previously investigated this subject, by injecting the blood-vessels of the gravid uterus: their researches, however, did not determine, in a satisfactory manner, that a vascular connexion exists between the uterus and cells in the placenta. The opinions of the Hunters were generally acquiesced in at the time they were promulgated, and their accuracy has not been called in question by any anatomist of reputation in this country for the last forty years.

In the communication which I have now the honour of presenting to the Royal Society, I propose to describe certain appearances which I have observed in the examination of six gravid uteri, and many placentæ expelled in natural labour, which seem to demonstrate that a cellular structure does not exist in the placenta, and that there is no connexion between this organ and the uterus by great arteries and veins.

If an incision be made through the parietes of the gravid uterus, where the placenta does not adhere, the membrana decidua will be observed lining the internal surface, and numerous minute blood-vessels and fibres passing from the inner membrane of the uterus to the decidua. At the circumference of the placenta, the membrana decidua separates from the chorion and amnion to pass between the uterus and placenta, and thus forms a complete membranous septum, which is interposed betwixt these organs. The chorion and amnion cover the fœtal surface of the placenta; and between these two membranes and the decidua lie the ramifications of the umbilical vein, and arteries subdivided to an almost indefinite extent, and connected together by white slender filaments running in various directions. The placenta thus consists solely of a congeries of the umbilical vessels, covered on the fœtal surface by the chorion and amnion, and on the uterine surface by the deciduous membrane, and enclosed between these membranes; it adheres to the fundus, or some part of the uterus, by innumerable flocculent fibres and vessels.

On detaching the placenta carefully from the uterus, the deciduous membrane is found to adhere so closely to the umbilical vessels which it covers, that it is impossible to remove it without tearing these vessels. With the fibres uniting the placental decidua to the uterus, are mingled numerous small blood-vessels, proceeding from the inner membrane of the uterus to the decidua; and these vessels, though more numerous at the connexion of the placenta with the uterus, exist universally throughout the whole extent of the membrane. There is no vestige of the passage of any great blood-vessel, either artery or vein, through the intervening decidua, from the uterus to the placenta; nor has the appearance of the orifice of a vessel been discovered, even with the help of a magnifier, on the uterine surface of the placenta. This surface of the placenta, deprived of the deciduous membrane, presents a mass of floating vessels, its texture being extremely soft and easily torn; and no cells are discernible in its structure, by the minutest examination.

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* Observations on certain Parts of the Animal Œconomy, by John Hunter, 1786, page 127.

† Anatomical Description of the Gravid Uterus and its Contents, by the late W. Hunter, M.D. London, 1794, page 43.

‡ Their letters are preserved in the Archives of the Royal Society.

At that part of the surface of the uterus to which the placenta has been adherent, there are observable a great number of openings leading obliquely through the inner membrane of the uterus, and large enough to admit the point of the little finger: their edges are perfectly smooth, and present not the slightest appearance of having been lacerated by the removal of the placenta. In some places they have a semilunar or elliptical form, and in others they resemble a double valvular aperture. Over these openings in the inner membrane of the uterus, the placenta, covered by deciduous membrane, is directly applied, and closes them in such a manner that the maternal blood, as it flows into the uterine sinuses, cannot possibly escape either into the cavity of the uterus or into the substance of the placenta. The above appearances on the inner surface of the uterus have been accurately represented by Rœderer.

When air is forcibly thrown either into the spermatic arteries or veins, the whole inner membrane of the uterus is raised by it; but none of the air passes across the deciduous membrane into the placenta, nor does it escape from the semilunar openings in the inner membrane of the uterus, until the attachment in the deciduous membrane to the uterus is destroyed. There are no openings in the deciduous membrane corresponding with these valvular apertures now described, in the internal membrane of the uterus.

If a placenta be examined which has recently been separated from the uterus in natural labour, without any artificial force having been employed, its surface will be found uniformly smooth, and covered with the deciduous membrane; which could not be the case did any large vessels connect it with the uterus. The placenta, in a great majority of cases, is also detached from the uterus after labour, with the least imaginable force; which would be impossible, if a union by large blood-vessels, possessing the ordinary strength of arteries and veins, actually existed. Besides, a vascular connexion of such a kind would be likely to give rise, in every case, to dangerous hæmorrhage subsequent to parturition, a circumstance not in accordance with daily experience.

Noortwych, Rœderer, Haller, Dr. W. and Mr. J. Hunter, and Dr. Donald Monro, do not appear to have examined the gravid uterus and its contents in the natural state of the parts, but after fluids had been forcibly injected into the hypogastric and spermatic arteries. The laceration of the deciduous membrane covering the orifices of the uterine sinuses followed this artificial process, as well as the formation of deposits of injection in the vascular structure of the placenta, giving rise to the deceptive appearance of cells. That this took place in the examinations made by Rœderer * and Monro †, does not admit of dispute; and the following facts render it more than probable that the Hunters were also misled, by the effects of artificial distention of the placenta, from the extravasation of the fluids forced into the uterine vessels.

In the course of last autumn, the preparations of the gravid uterus in the Hunterian Museum at Glasgow, were examined at my request by Dr. Nimmo; and in none of them does it appear certain that any great blood-vessels pass from the uterus into cells in the placenta; but in many the deposits of injection, causing the appearance of cells, were observed evidently to be the result of extravasation. No preparation in the collection seems to have been expressly made for the purpose of proving or disproving the fact that the deciduous membrane passes over the uterine surface of the placenta; but in reference

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* Icones Uteri humani, Observationibus illustratæ. J. G. Rœderer, 1759.

† Essays and Observations, Physical and Literary, read before a Society in Edinburgh, 1754. Vol. I.

to preparation R. R. No. 139, it is observed by Dr. Nimmo that no vascular openings are visible in the membrane interposed between the uterus and placenta.

No. 178 "is a small section of the uterus, with the veins injected green, and broken off where they were entering the placenta." The surface of the injected matter is smooth; the edges of the openings defined, and quite unlike ruptured vessels; their form in general elliptical, seeming as if they were holes cut in the side of a convolution.

No. 125. "A portion of uterus and placenta, the latter injected from uterine vessels." There is an opening which seems to be natural, corresponding to one of those in the uterus; but the majority of those whereby the injection has passed into the placenta, seem to be mere lacerations.

No. 101. "A section of uterus with veins injected black, and the injected matter protruding by irregular plugs into the cavity of the uterus." The holes are semilunar and elliptical, with defined edges, and nothing resembling the continuation of vascular tubes to be seen.

R. R. 121, is described in the printed catalogue as follows: "A small portion of placenta and uterus, where the cells of the placenta have been injected from the veins of the uterus. The veins are seen very large, entering the substance of the placenta."

Dr. Nimmo makes the following observations on this specimen: "This preparation seems to be most in point. I would describe it differently. The cellular substance of the placenta has certainly been filled from the uterine vessels. These, however, instead of passing directly into the placenta, are distinctly seen applying their open mouths to the membrane of the placenta, where the injection in some instances stops. The membrane is thinner here than where no vessels are applied, consisting, so to describe it, of one layer, while a second layer covers all other parts. Where the injection is passed into the substance of the placenta, it has evidently been forced to the side between the layers, and found some weak point, whereby it has entered into and been diffused throughout the cellular texture of the placenta *."

In the Museum of the Royal College of Surgeons of London, there is a preparation of the uterus with the placenta adhering to the inner surface, which is supposed to have been put up by Mr. Hunter himself nearly fifty years ago. The vessels both of the uterus and placenta have been filled with injection, and the parietes of the uterus, placenta, and membranes, have all been divided by a vertical section into two nearly equal portions. By permission of the Board of Curators, I have been enabled to examine one of these portions, and to have a drawing of it made. In the interstices of the muscular fibres I observed the veins of the uterus, which ran in great numbers towards the part where the placenta adhered. They were of an oval form, their long axes being in the long axis of the uterus. The muscular fibres ran longitudinally from the fundus to the os uteri.

The deciduous membrane was every where covered with minute, tortuous blood-vessels, proceeding from the inner surface of the uterus, and filled with injection. There was no appearance of vessels of any magnitude passing between the inner surface of the uterus and placenta; but flattened portions of injection were observed in this situation, having in many parts the form of thin layers, which had obviously escaped from the orifices of the uterine veins. Elsewhere the injection had lacerated the deciduous membrane, and formed deposits in the vascular part of the placenta.

The facts which have now been stated warrant, I think, the conclusion, that the human placenta does not consist of two parts, maternal and fœtal; that no cells exist in its substance; and that there is no communication between the uterus and placenta by large arteries and veins. The whole of the blood sent to the uterus by the spermatic and hypogastric arteries, except the small portion supplied to its parietes and to the membrana decidua by the inner membrane of the uterus, flows into the uterine veins or sinuses, and after circulating through them, is returned into the general circulation of the mother by the spermatic and hypogastric veins,

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* My friend Samuel Broughton, Esq. F.R.S., during a recent visit to the Hunterian Museum at Glasgow, examined the preparations of the placenta and uterus at my request, and authorizes me to say that his observations fully confirm the accuracy of Dr. Nimmo's statements.

without entering the substance of the placenta. The deciduous membrane being interposed between the umbilical vessels and the uterus, whatever changes take place in the fœtal blood must result from the indirect exposure of this fluid, as it circulates through the placenta, to the maternal blood flowing in the great uterine sinuses.

Since the preceding paper was forwarded to the Secretary of the Royal Society, the following valuable communication has been received by the author from Mr. Owen, to whom portions of the gravid uterus and placenta were submitted for minute examination:—

"Lincoln's Inn Fields,
17th November.

"My dear Sir,—During the time you were examining the Hunterian preparation of the uterus and placenta in the Museum of the Royal College of Surgeons, your observations on the obscurity produced by the extravasated injection led me to think of some less objectionable mode of demonstrating the vascular communication between the uterus and placenta, if it existed; or of proving more satisfactorily than the appearances you pointed out in that preparation seemed to do, that there was no such communication.

"You have since afforded me the means, through the kindness of Mr. Alex. Shaw, of examining, in the manner I wished, the anatomical relations between the placenta and uterus. This has been done by dissecting the parts under water before disturbing them, either by throwing forcibly foreign matter into the vessels, or by separating the placenta from the uterus, to observe the appearances presented by the opposed surfaces,—a proceeding which if done in the air is liable to the objection of the possibility of having torn the vessels which were passing across, and the coats of which are acknowledged, by those who maintain the existence of such vessels, to be extremely delicate.

"The mode, therefore, which was adopted to avoid these objections, was to fix under water, in an apparatus used for dissecting mollusca, &c. a section of the uterus and placenta, and, commencing the dissection from the outside, to remove successively and with care the layers of fibres, and trace the veins as they pass deeper and deeper in the substance of the uterus in their course to the deciduous membrane; in which situation, as the thinnest pellicle of membrane is rendered distinct by being supported in the ambient fluid, I naturally hoped in this way to see the coats of the veins continued into the deciduous membrane and placenta, and to be able to preserve the appearance in a preparation, if it actually existed in nature. But in every instance, the vein, having reached the inner surface of the uterus, terminated in an open mouth on that aspect; the peripheral portion of the coat of the vein, or that next the uterus, ending in a well-defined and smooth semicircular margin, the central part adhering to, and being apparently continuous with, the decidua.

"In the course of this dissection, I observed that where the veins of different planes communicated with each other, the central portion of the parietes of the superficial vein invariably projected in a semilunar form into the deeper-seated one; and where (as was frequently the case, and especially at the point of termination on the inner surface) two or even three veins communicated with a deeper-seated one at the same point, these semilunar edges decusssated each other, so as to allow only a very small part of the deep-seated vein to be seen. I need not observe to you how admirably this structure is adapted to ensure the effect of arresting the current of blood through these passages, upon the contraction of the fibres with which they are every where surrounded.

"On another portion of the same uterus and placenta, (which were removed from a woman who died at about the fifth month of utero-gestation,) I commenced the examination under water by turning the placenta and deciduous membrane from the inner surface of the uterus. In this way, the small tortuous arteries that enter the deciduous membrane were readily distinguishable, though not filled with injected matter; and as it was an object to avoid unnecessary force in the process of separation, they were cut through, though they are easily torn from the decidua. But with respect to the veins, they invariably presented the same appearances as were noticed in the first dissection, terminating in open semicircular orifices, which are closed by the apposition of the deciduous membrane and placenta. This membrane is, however, certainly thinner opposite these orifices than elsewhere; and in some places appeared to be wanting, or, adhering to the vein, was torn up with it; but in these cases the minute vessels of the placenta only appeared, and never any indication of a vascular trunk or cell commensurate with the size of the vein whose terminal aperture had been lifted up from the part.

"The preparation which accompanies this letter shows the termination of a vein on the inner surface of the uterus, and an artery of the decidua cut through, with the corresponding appearances on the surface of the placenta; also the valvular mode in which the veins communicate together in the substance of the uterus.

"I remain, yours very truly,
"RICHARD OWEN."

SINGULAR CASE OF FUNGUS CEREBRI, FROM A GUN-SHOT WOUND.

To the Editor of the London Medical Gazette.

SIR,

I AM not aware that any example of fungus cerebri is on record, in which the protrusion took place from the base of the skull, and came through the wound, from so distant a source as in a case which I have lately met with. If you think it deserving of a place in the Gazette, it is at your service.

I am, sir,
Your obedient servant,
CÆSAR HAWKINS.

31, Halfmoon-Street,
May 17, 1832.

On the 2d of May, W. P., a fine boy of eleven years of age, being in a room where there were several pistols, took down one of them and looked into it, in order to ascertain whether it was loaded; in doing which it went off, and the contents entered the face just below the right eye, making a small wound, and remaining in the head. There was considerable hæmorrhage from the wound, as well as from the nose and mouth, and from the right ear. When I saw him he was faint and cold, and in a complete state of collapse, but sensible to pain when he was touched, and he had vomited slightly. The right eye seemed to be uninjured, and the left was observed to be a good deal turned inwards to the nose, but the pupil acted readily. From the probable course of the ball, and the quantity of venous hæmorrhage from the ear, it appeared to me that the temporal bone was probably fractured, so as to rupture the lateral sinus; and the extreme degree of collapse rendered it not impossible that the ball might have gone through the bone into the cerebrum. His alarming state induced me to avoid any violence with a view to the extraction of the bullet, lest any internal hæmorrhage which might be going on should be increased: he was left, therefore, perfectly quiet, directed to be kept quite cool, and to take nothing but cold liquids.

During the remainder of this and the next day he continued in a state of restlessness, turning from one side to the other, but sensible to pain, and capable, when roused, of answering, and giving a clear account of the manner in which the accident happened. He vomited occasionally the contents of the stomach, mixed with a good deal of blood, which appeared to come from the wound, as the bleeding from the ear still continued. The circulation had risen a little, but he never required depletion, the pulse being for the most part not more than from 80 to 90, and weak. The face and eyelids were swollen, but without much tension.

On the third day he was perfectly quiet and rational, the swelling began to subside, and suppuration took place freely from the wound and from the ear. A probe passed from the wound towards the ear, to a depth of about four inches, through a quantity of splinters of bone, and at the bottom struck what appeared to be the bullet; and a probe passed also through the meatus, in the midst of similar splinters of broken bone; so that, from both examinations, about one inch inwards and forwards from the ear seemed to be the situation of the bullet. It was clear, therefore, that part at least of the temporal bone was fractured, yet he heard perfectly on this side, so that the labyrinth must still have been uninjured.

During the next three days he went on very well, without any symptoms attributable to injury of the brain. The swelling of the face and fauces, which had produced some difficulty in swallowing and speaking, wholly subsided, and several pieces of bone, and a small portion of lead, were taken away through the wound and through the ear. On the 9th, however, he again became restless and irritable, and started much in his sleep; he complained of pain in the back and abdomen: the whole of the muscles of the trunk were in a state of spasm, so that even if his head were moved the whole body was turned with it, though, at the same time, the muscles of the legs and arms, and the lower jaw, were perfectly under his control. He afterwards became hurried in his manner, then delirious, singing and talking incessantly, and attempting to get out of bed, and at last was quite unconscious of what was passing around him. At the time these symptoms of irritation commenced, the discharge from the wound and from the ear became dark-coloured and bloody, and some blood was discharged with pieces of bone. On the evening of the 9th a protrusion was observed, apparently of cerebral matter, through the wound in the face, which increased to the size of a walnut, but was afterwards pushed off. He died on the 10th, just eight days from the accident.

It was found that the bullet had passed through the junction of the malar and maxillary bones, breaking up completely the floor of the orbit; thence it passed to the inside of the lower jaw, through the external pterygoid muscle, having so far injured the joint that both the glenoid cavity and the condyle of the jaw were denuded of cartilage and periosteum, and a little piece of the inside of the condyle was broken off. The bullet had then broken up the junction of the sphænoid and temporal bones, in the fossa between the sella tunica, the petrous portion of the temporal bone, and the lesser wings of the sphænoid. The glenoid cavity and the meatus were of course loose, and a fracture extended across the petrous portion of the temporal bone, a sharp point of bone having pricked the dura mater and entered the lateral sinus. The inner part of the petrous portion was thus detached, but the carotid artery and the jugular vein were uninjured; the ball itself, which was much flattened, being situated just below this inner part of the temporal bone, where it had been perceived by the probe.

Through the wide opening thus made in the sphænoid and temporal bones the brain protruded in considerable quantity into the cavity made by the bullet, and was in the usual vascular and pulpy state of fungus of the brain: the substance of the cerebrum around this softened part was vascular, and of the yellow colour generally found in such cases. A small layer of coagulated blood was perceptible on the surface of the right hemisphere, and upon the tentorium.

The veins of the brain were full of blood, but there were little signs of inflammation, except around the junction of the optic nerves, which were slightly covered with lymph.

It may seem doubtful whether the dura mater and the brain had been directly wounded by the bullet at the time of the accident; but, as the symptoms were at first those of concussion only, and the fatal symptoms occurred suddenly, after the boy had been gradually getting better, I think it more probable that the blow had produced contusion only in the first instance, giving rise to the small effusion of blood, which has been mentioned, and that the dura mater being bruised, and deprived of its support, had sloughed, so as to allow of sudden protrusion of the brain at the time the delirium and other signs of irritation were observed. And it seems probable, that, but for this protrusion, the boy might have got well, as he had recovered from the concussion, and had no inflammation, the pulse being soft and natural, and only 80, just before the protrusion took place, and the wound itself being healthy, and allowing plenty of room for the extraction of the remainder of the bones and of the bullet.

Fracture of the bones of the cranium, allowing of the formation of fungus cerebri, has been observed in every part of the upper surface and sides of the head, even so low down as to admit of the escape of brain from the side of the middle lobe through the ear; but I am not aware of any instance in which fungus took place from the base of the brain, as far inwards as in this instance, by the side of the sella tunica; and the case seems on this account curious. It shows how necessary the support of the cranium is to prevent fungus cerebri, as it took place through a wound of four inches in depth; and it shows also how distinct the symptoms arising from protrusion of the brain are from those of inflammation, although, no doubt, the fungus cerebri is often accompanied by inflammatory symptoms.

Another point deserving notice is the preservation of the sense of hearing, although not only the tympanum was empty, and the external ear completely separated, but the whole temporal bone so shattered that the fractured portions left not enough entire to include the whole labyrinth, but must actually have broken across one at least of the semicircular canals.

INJURY OF THE GREAT ISCHIATIC NERVE.

To the Editor of the London Medical Gazette.

Liverpool, May 14, 1832.

SIR,

I BEG to enclose the particulars of a case of severe injury of the great ischiatic nerve, which has caused a good deal of speculation here as to the consequences, and of course has excited some interest. It is a case of rare occurrence, and may be considered singular, inasmuch as the patient had scarcely one bad symptom. I shall feel obliged by your giving it a place in the next number of the Medical Gazette.

Yours, very respectfully,
J. M. BANNER.

I was requested on the 22d of March last to visit Robert Lunn, a coachman, aged 45, who had met with a severe accident. On my arrival at the house, I found Dr. Brandreth in attendance. We were informed that Lunn had been cleaning the drawing-room window; that his foot had slipt from its ledge; that he had fallen a distance of seven or eight yards, and had alighted on the spikes of the area railing. He is a man of full habit of body, and had hitherto been remarkably healthy.

On examination, we found two punctured wounds; a triangular one, an inch in extent each way, situated in the back part of the right thigh, about three inches below the tuberosity of the ischium; the other on the right side of the anus, the sphincter being partially divided. Both wounds were of considerable depth; there was hanging from that in the thigh a number of filaments of the ischiatic nerve, much lacerated. The patient was very restless, and complained of great pain in the thigh, with numbness which was in the calf of the leg, and more complete in the heel and sole of the foot. Dr. Brandreth and myself determined on the removal of the lacerated portion of nerve. We thought there would be much less danger in so doing than in returning it into the wound. Accordingly, it was cut away with a pair of scissors; the edges of the wound were brought together with adhesive plaister, and slight pressure made with compress and bandage. The filaments of nerve removed measured three and a half inches; they were much separated, except at the part that was divided by the scissors; here it measured in circumference near the half of an inch.

The patient was visited again in four hours; he complained of increased pain, shooting from the wound in the thigh to the loins, and down the front of the leg to the foot, with occasional spasmodic twitchings; there was an increase of numbness in the calf, heel, and sole of the foot; and his friends observed an unusual irritation in his manner. The pulse was now full and quick; (soon after the accident it was low, from loss of blood;) he had lost a considerable quantity of venous blood from both wounds, particularly from that near the rectum. Eighteen ounces of blood were taken from the arm; an aperient draught was given; and cold lotion applied over the bandages.

23d.—I found him with all the symptoms of the preceding night; he had not slept; the pain was great; the spasmodic affection and numbness continued; he had had slight *rigors*; the tongue was furred; the pulse continued full; there was a dark-coloured discharge from the wounds, the edges of which were puffy; and there was considerable inflammation and swelling of the surrounding skin. The blood taken yesterday was buffy, and slightly cupped. He was ordered to poultice the wounds with linseed-meal; V.S. ad ℥xiv. After the bleeding he felt faint, and much relieved; he slept occasionally during the day; when in sleep there was twitching of the hands and distortion of the face; the bowels had been well acted on.

24th.—He had passed a restless night; complained of a sensation of burning at the root of the toes; the numbness in the calf of the leg was less, that in the heel and sole of the foot continued the same; the pain in front of the leg was much abated; he had been frequently affected during the night with spasmodic twitchings in the thigh and calf of the leg; there was a dark-coloured matter discharged from the wounds; sloughing had taken place between the rectum and wound, so as to lay the gut open three inches in extent; he was suffering from considerable irritative fever; the bowels were constipated. V S. ad ℥xvj.

℞ Mag. Sulph. ʒiss. Inf. Rosæ, ℥iss. M. ft. Haust. 4tâ quâque horâ sumendus.

A strong opiate embrocation was ordered to be applied to the foot occasionally.

25th.—He had passed a better night; he had had very few returns of spasm since the last bleeding; the numbness continued much as at the last report; the pulse was not so full or frequent as on the preceding day; irritative fever much abated; bowels had been acted on; the wounds looked less inflamed; the discharge was of a lighter colour; the wound into the rectum was now dressed to the bottom with lint.

In the evening of this day I found him complaining of a return of pain in the leg, extending to the foot; the sole of the foot was very sore to the touch; he had a sensation of cold water trickling down the back of the leg, and felt slight numbness on the top of the foot; there was increased pain at the root of the toes, and very acute pain at the edges of the foot; he complained also of pains shooting from the wound to the bottom of the back; the pulse had risen.

V.S. ad ℥xvj.

After this bleeding he felt immediate relief. An anodyne draught was ordered for bed-time.

26th.—He had passed a restless night; he had felt an inclination for sleep, but was prevented by the spasmodic affection in the calf of the leg; the sensation of pain and heat in the toes continued; pulse was quick and full; tongue slightly furred; the wounds were healing; the blood was buffy.

Continued the Draughts, with the Sulphate of Magnesia. V.S. ad ℥xij.

28th.—There has been a cessation from pain and spasm since the last bleeding; the wounds were looking clean, and dressed with a solution of the nitrate of silver; the blood was slightly buffy; pulse regular; the numbness in the calf of the leg and top of the foot nearly gone; that in the heel and sole of the foot continued much the same.

30th.—There had been little alteration since the night of the 26th, until this morning, when he complained of acute pain at the sides of the foot, so great, that on passing the finger lightly over them, he called out: his wife states, that during sleep he opened and shut his hands often, and that there was a frequent spasmodic action of the muscles of the face, as if in pain. He stated, however, that he did not feel any pain unless the foot was touched; in other respects he was much better: he was gradually regaining sensation in the sole of the foot; it was nearly perfect in the calf and top of the foot: a strong opiate embrocation was ordered for the foot.

31st.—The sensitiveness of the foot was much lessened.

April 2d.—He has progressively improved, and there has been very little deviation until to-day; he complains of languor and thirst; the pulse has fallen from 82 to 48 beats in the minute; his spirits are depressed; in consequence of this alteration, I improved his diet, and allowed a little ale during the day: he also took the following three times a day:—

℞ Inf. Cinchonæ ℥iss.
Sp. Amm. C. ʒss. M. ft. haust.

10th.—He has not had any unpleasant symptom since the last note; the pulse has become regular, at 74; he now leaves his bed, and is able to walk without much difficulty; he states that his feelings in the affected leg are very different to those in the sound one; it feels much heavier, and not so firm; that, in walking, if he places his foot upon a raised stone, or uneven ground, it causes great pain to shoot from the foot to the knee; the sense of feeling is nearly restored to every part, with the exception of the sides of the heel; here it remains very slight; the wounds are healing fast.

30th.—The wounds are quite healed, and he has resumed his duties as a coachman: he has not a proper use of the limb; it still feels heavier and less firm than its fellow; the sense of feeling is returning to the heel; in the other parts it is quite perfect.

May 13th. — Lunn visited me this morning; he is each day gaining a more perfect use of the leg; the sensation is much more perfect in the heel; in other respects he remains quite well.

May we not infer that at least one-half the diameter of the ischiatic nerve must have been divided, and that three inches and a half in length of those filaments which form the tibial nerve, must have been cut away? It will be observed, from the foregoing symptoms, that the parts to which the tibial nerve goes were numb from the time of the accident, and that numbness came on the top of the foot on the third day: this led me at first to suppose that the filaments which were removed might have composed, in part, both the tibial and fibular nerves; this latter symptom, however, may have been caused by pressure on the nerve, from effusion by inflammation, near the wound, which at that time was considerable.

I was induced to adopt the antiphlogistic treatment from a supposition that there was inflammation of the nerve existing, by the symptoms of spasmodic twitchings and excruciating pains that were felt soon after the injury. These symptoms, however, may have been merely sympathetic; my greatest fear was in their terminating in tetanus, and consequently was anxious to reduce all symptoms of inflammation.

Is it not possible to account for the returning sensation (which began to take place in so short a time as the third day after the accident) from the anastomosis which takes place between the tibial and fibular nerves; and by continuing sensation to the parts supplied by the tibial nerve, by means of the connecting influence, which might thus supply that nerve with its former power?

Or can the return of sensation be accounted for by the cutaneous branches of the ischiatic?

PARALYSIS OF THE FACE.

To the Editor of the London Medical Gazette.

SIR,

EXAMPLES of partial paralysis of the face are not, according to my observation, of very frequent occurrence. The subjoined case, which is of that description, furnishes additional pathologic evidence of the functions of the "respiratory nerve of the face," as established by the experiments and observations of Sir C. Bell. The seat and cause of disease were easy of detection, and the symptoms yielded to obvious treatment. If the case appear to you worthy of being laid before the readers of the Gazette, I will thank you to insert it when convenient.

I am, sir,
Your obedient servant,
R. ARROWSMITH, M.D.

Coventry, April 15, 1832.

January 9, 1832.—W. Hyde, æt. 12, states, that six weeks since an abscess formed within the right eye, attended with great pain, loss of hearing, and fever. About a fortnight previously he had fallen into the water, and thus taken cold. The pain in the internal ear was relieved by the discharge of matter, but the cheek and eye of that side have since remained in the state now to be described. That side of the face generally, but the mouth in particular, is drawn, as in a paralytic person, very much towards the left side; he cannot whistle, nor can he smell through the right nostril. In laughing, the right side of the face remains completely tranquil. Mastication is performed equally well on either side, but on the right the food collects between the teeth and the cheek, whence he dislodges it by pressing the outside of the cheek with the fingers. Sensation is perfect. He cannot close the right eye; if requested to do so the eye is rolled upwards, so that the cornea is nearly covered by the upper eyelid. The eye appears larger, in consequence of the dragging downwards of the lower eyelid. The conjunctiva is inflamed, and the eye affected with lachrymation. At night, when in bed, he closes the eye by drawing the eyelids together, and immediately turns his face upon the pillow. Vision is perfect. The attempts to frown produce squinting, the pupil of the right eye being directed somewhat upwards.

There is now no discharge from the ear, but occasional pain and a "sawing noise." He has headache; sleeps well, but dreams a good deal. The pulse is natural, the tongue clean, the bowels open, and appetite good.

He was directed to apply four leeches every other day close to the ear, a blister behind it, to take calomel and jalap every other night, and to abstain from animal food and stimulating fluids.

12th. — No change, excepting that there is less pain in the ear.

16th.—The eye is less inflamed, and he can close it somewhat more. 'In laughing, the right side of the mouth is much drawn towards the left. He cannot whistle, nor can he smell through the right nostril, because, he says, there is "a stoppage in the head." There is no pain in the ear. The leeches and powders to be continued, and the blister kept open with savine ointment.

20th.—He can close the eye still more, and whistle imperfectly, the mouth being less drawn towards the left side. The deafness continues. He can neither frown nor elevate the eyebrows.

February 3d.—He can draw up the angle of the mouth on the right side a little, and can whistle better, and quite easily, if he hold the right angle of the mouth. He can also nearly close the eye.

22d.—The ear is free from pain and discharge; the hearing not quite perfect. He can close the eye almost completely. The face is not at all distorted; he can whistle easily, and smell perfectly through the right nostril. The blister continues to discharge.

March 15th.—The hearing is quite restored. He is free from all remains of the affection.

To the Editor of the London Medical Gazette.

SIR,

I AM induced to send you the following case of partial paralysis, from its similarity in many of its symptoms, and in its result, to the case which Dr. Arrowsmith has drawn up for insertion in your journal; and if you consider it worthy of being laid before the public, I shall be obliged by your giving it a place in the Gazette.

I am, sir,
Your obedient servant,
CHAS. B. NANKIVELL,
One of the Surgeons to the Coventry Benevolent Dispensary.

Coventry, May 15, 1832.

William Lucas, æt. 44, states that he always enjoyed good health until the latter end of March, when he became affected with headache, and a singing noise in the left ear; the bowels were at the same time rather constipated. Soon after the accession of these symptoms, he experienced a slight impediment in his speech; he could not close the left eyelid, the external canthus of which seemed lower than that of the opposite lid, and the tears ran down the left cheek. On the 11th of April, when he applied at the Dispensary, there was complete paralysis of the whole of the left side of the face. It was quite immoveable by the action of its own muscles, and the lips were only acted upon by the muscles of the opposite side, and by the right half of the orbicularis oris, which had drawn the mouth upwards, and to the right side, so that the lips were nearly in a line with the right meatus auditorius externus. The tongue could be protruded straight forwards. In laughing, and in attempting to whistle, the left portion of the lips did not touch each other, and he had considerable difficulty in the pronunciation of labial letters. Sensation is unimpaired. The left meatus auditorius is deficient in ceruminous secretion, but the sense of hearing is unaffected. The tongue is rather furred; the bowels costive; skin hot and imperspirable; urine high-coloured; pulse full, and rather hard. He was bled freely from the arm, took calomel and colocynth, with cathartic mixture, was placed upon low diet, and ordered to refrain from all employment.

April 13th.—Feels much relieved; no improvement in the state of the paralyzed muscles; pulse softer; febrile symptoms abated. Some degree of pain still remaining in the head.

Six leeches were directed to be applied below the left ear, and a blister to the nucha. Purgative medicine to be continued.

16th.—Pain in the head less; some improvement in the parts affected. The lips can be drawn nearer together, and the external corner of the mouth and of the eyelid is not so much depressed. Still some thirst and heat of skin; pulse soft and compressible.

> ℞ Mag. Sulphatis, ʒiss. Liq. Ammon. Acet. ʒiij. Antimon. Tartar. gr. ij. Aq. Menth. Sativæ, ʒv. M. Cap. Coch. iij. 4tâ quâque horâ. Sumantur Pil. ij. Hyd. Sub. et Ext. Col. c. omni nocte.

18th.—Considerable improvement in all the symptoms.

> Emp. Lyttæ pone aurem sinistrum imponendum. Perstet in usu Pil. et Misturæ.

From this period he progressively recovered the use of the affected parts, and now continues in every respect convalescent.

May 11, 1832.

INJECTION
OF
SALINE SOLUTIONS INTO THE VEINS,

Adopted with success in Malignant Cholera.

[WE have been favoured with the following curious and interesting document, addressed to the Central Board of Health.]

Sir,—I conceive it to be my duty to let you know, for the information of the Central Board of Health, that the great desideratum of restoring the natural current in the veins and arteries, of improving the colour of the blood, and recovering the function of the lungs in cholera asphyxia, may be accomplished by injecting a weak saline solution into the veins of the patient. To Dr. Thomas Latta, of this place, is due the merit of first having recourse to this practice. He has tried it in six cases; three of which I have seen, and assisted to treat. The most wonderful and satisfactory effect is the immediate consequence of the injection. To produce the effect referred to, a large quantity must be injected, from *five to ten pounds* in an adult, and repeated at longer or shorter intervals, as the state of the pulse, and other symptoms, may indicate. Whenever the pulse fails, more fluid ought to be thrown in to produce an effect upon it, without regard to quantity. In one of the cases I have referred to, 120 *ounces* were injected at once, and repeated to the amount of 330 ounces in 12 hours. In another, 376 ounces were thrown into the veins between Sunday, at 11 o'clock A.M. and this day, (Tuesday) at 4 P.M.; that is, in the course of 53 hours, upwards of 31 pounds!

The solution that was used consisted of two drachms of muriate, and two scruples of carbonate of soda, to sixty ounces of water. It was at the temperature of 108 or 110°.

The apparatus employed for injecting was merely one of Reid's common syringes, (the fluid being put into a vessel rather deep and narrow) with a small pipe fitted, that it might easily be introduced into an incision in the veins of the usual size that is made in bleeding. It may, however, be well to keep in mind that in the event of the operation being frequently repeated, it may be advisable to inject by different veins.

I forbear at present to enter further into the particulars, nor have we had sufficient experience to speak decisively on the subject. I may, however, mention that the idea of having recourse to this remedy in cholera occurred to Dr. Latta, from being convinced (which I am also) that the evacuations upwards and downwards are in reality the serum of the blood; that it is the duty of the physician to replace it as speedily as possible by injecting a fluid, as similar to the serum as can be formed artificially, directly into the veins, which has been done here with wonderful, and so far as we can yet judge, excellent effect. An immediate return of the pulse, an improvement in the respiration and in the voice, an evolution of heat, an improvement in the appearance of the patient, with a feeling of comfort, are the immediate effects. The quantity necessary to be injected will probably be found to depend upon the quantity of serum lost, the object of the practice being to place the patient in nearly his ordinary state as to the quantity of blood circulating in the vessels.

I have, &c.

(Signed) ROBERT LEWINS, M.D.

Fellow of the Royal College of Physicians, and
Member of the Leith
Board of Health.

Leith, 6, Quality-Street,
May 15, 1832.

To W. Maclean, Esq.
Secretary to the Central Board of Health.

Sir,—I did myself the honour to address a letter to you lately on the effect of injecting a saline solution into the veins of a patient labouring under cholera. We have not frequent opportunities now of trying this, which I denominate, admirable remedy, as the disease is decidedly less frequent here; but I have seen it employed in two other cases, in the course of the last two days, with the same excellent effect. Sixty ounces are generally thrown in at once, and repeated at the end of three or four hours. In a case to-day, where I saw fifty-eight ounces injected, (being the third time of performing the operation) the patient's pulse at the commencement was 180, very small, and very feeble. She was excessively restless, with a feeling of great weakness and tormenting thirst. Before twelve ounces were injected, the pulse began to improve; it became fuller and slower, and it continued to improve until, after fifty-eight ounces had been injected, it was down to 110. Before I left the patient (a woman) her condition was altogether amazingly ameliorated. There was a fine glow, and a slight perspiration on her face; the veins on the back of her hand were well filled; the restlessness was removed; and the feeling of excessive weakness gone, and the thirst ceased. The pulse was under 100, and free, full, and soft! Verily, sir, this is an astonishing method of medication, and I predict will lead to wonderful changes and improvements in the practice of medicine. I have addressed you upon the subject as the organ, from your high official station, of most speedily and effectually disseminating a knowledge of the extraordinary facts referred to. It will, of course, give me great pleasure to enter further into particulars upon any particular point on which you may require information in reference to the cases that have come under my observation.

I have, &c.

(Signed) ROBERT LEWINS.

6, Quality-Street, Leith,
May 18, 1832.

W. Maclean, Esq.
Secretary to the Central Board of Health.

In the hands of a man of ordinary dexterity, the common injecting apparatus alluded to in my last will be found to answer the purpose perfectly well; but if the practice I recommend is, as I hope it will be, generally adopted, it will, I conceive, be expedient to advise that a regular and perfect transfusion apparatus be used, at all events, to warn those who inject to beware of allowing air to get into the vein. The tubes, of course, must be filled with fluid, as well as the pipe in the vein, before commencing, and considerably more fluid than it is intended to use ought to be in the vessel from which it is pumped.

R. L.

CHOLERA WITH ABORTION,

Black Discharge from Uterus becoming Florid under the Saline Treatment.

To the Editor of the London Medical Gazette.

Sir,

I am induced thus publicly to communicate a recent case of cholera successfully treated by the *saline practice*, and under circumstances which may, perhaps, render its recital not altogether uninteresting to the profession.

May 12th.—I was consulted in the afternoon by Mrs. L., æt. 42, the mother of twelve healthy children, the eldest twenty-three and the youngest three years old. Supposes herself about three months advanced in utero gestation; complains of diarrhœa of two or three days' standing, with great prostration, and cramps in her lower extremities. Pulse infrequent, and feeble.

Ordered ℞ Mist. Cretæ, f. ℥iv.; Tinct. Opii, ♏xl. capiat 4tam partem statim et repetatur post singulas dejectiones.

In the evening her daughter came to say her mother had taken all the mixture without experiencing any benefit; indeed, to her former symptoms were superadded vomiting of a violent character.

Ordered the mixture to be repeated, with the addition of Tinct. Catechu, f. ℥ss.

13th, 6 A.M.—Mixture all used, but the patient is considerably worse. Has passed a very restless night; countenance much sunk; voice peculiar, and little more than a whisper; stools fæculent; a bilious fluid was also ejected from the stomach, and complains of a bitter taste in the mouth.

> Ordered Pulv. Cretæ, C. c. Opio, ʒj.; divide in Pulv. iv. to be taken in the same manner as the mixtures were directed.

Noon.—The powders have all been taken, without the slightest improvement in any one symptom. Stools liquid, and now, for the first time, of a rice-water colour. The fluid which she vomits, however, is still bilious; tongue flabby, coated, and cold; pulse scarcely perceptible at the wrist; complains much of headache.

> ℞ Liq. Opii Sedat. ʒj.; Ammon. Carb. ʒj.; Syrupi Aurantii, f. ʒvi.; Aquæ, ℥v. misce, capiat Coch. ii. secundâ quâque horâ cum Acid. Tart. gr. xv. in statu effervescentiæ.

10 P.M.—No better. Some discharge from the uterus of *black blood*, with bearing-down sensations, as if about to abort.

14th, 7 A.M.—Miscarried in the night; appears to have been very correct in her calculation as to her period of gestation. Purging and vomiting not at all relieved; extremities cold; pulse gone, and appears to be fast approaching to a state of dissolution. Ordered a Seidlitz powder to be taken directly, and repeated at pleasure; also one of the following powders to be taken in twenty minutes after the Seidlitz, and repeated every hour:—

> ℞ Sodæ Carb. ʒss.; Sodæ Mur. ℈j.; Potassæ Oxymur. gr. vii. misce.

Noon.—Vomiting has ceased; purging less frequent; pulse begins to be perceptible, but small, slow, and tremulous; some return of heat on the surface. One Seidlitz powder has been taken; also four of the other powders; all of which have been retained. These were ordered to be continued.

10 P.M.—Nine of the above powders have now been used, and retained. Considerable re-action has taken place; voice and countenance much improved; pulse 80, and begins to be more full. *Lochia beginning to be florid in appearance*, and of the usual quantity. Only one dejection since my last visit. Powders ordered to be continued every hour. From this time she rapidly improved, and is now out of danger.

It is worthy of remark, that the eldest daughter and the husband of this woman have also been attacked with diarrhœa and cramps in the extremities, for which Pulv. Cretæ C. c Opio was given, without affording the slightest relief. After persisting in their use for some time, and as the symptoms were evidently becoming worse, recourse was had to the above saline remedies, and speedy recovery was the almost immediate result.

You will observe, sir, I was very tardy in putting these patients under the saline treatment; but I must beg to observe that this did not arise from a conviction on my part that they were not cases of cholera, for, perhaps, a better marked case than the first was never witnessed; but I was anxious to try whether, while bile continued to pass into the duodenum, the diarrhœa and vomiting could not be arrested by any other means than the saline medicines, as recommended by Dr. Stevens, which I had seen used with so much success in the prison at Cold-Bath Fields. I think, however, you will agree with me, that I gave chalk and opium (the usual remedies) a very fair trial in the above cases. I am the more particular in pointing out this, as my belief *now* is that the non-purgative alkaline salts are, as Dr. S. asserts, more useful in relieving the sickness at the stomach, and checking the diarrhœa, than common astringent or absorbent medicines. I may also observe, that these cases shew that the rice-water evacuations are not invariably present in the early stage of the Indian cholera; and from what I have seen of this malignant disease, though I believe that no treatment will be successful in every case of collapse, yet my thorough conviction is, that a much greater number of patients will be saved by the saline treatment than by any other practice that has yet been tried.

I have been the more induced to communicate the above facts, as I observe that others (even the Central Board of Health) are still recommending the use of medicines which have been long used in cholera, fairly tried, and found to be not only useless but actually injurious.

I am, sir,
Your obedient servant,
HENRY WHITMORE.

Cold-Bath Square, May 21, 1832.

I may add, that on last Sunday night,

just before midnight, I was called on to attend another female, who resides in the same parish, and at no great distance from the above patients. This woman had been suffering for two or three days from vomiting and purging, but when I saw her, for the first time, she was in a state of collapse. She was also exceedingly emaciated from previous bad living. She was immediately put under the saline treatment; re-action soon came on, with general amendment in all the symptoms. She has since continued to improve; the kidneys are again acting, and I have now great reason to hope that she may recover. She is, however, in a very low state, not merely from the effect of the disease, but also from previous ill health. She is suffering also from great mental anguish, for the loss of two children, one of whom had died on Saturday and the other on Sunday, the same day that she was herself attacked. I did not see either of these children, but both of them were reported as having died from confirmed cholera.

Thursday evening, 9 o'clock.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

A Critical and Experimental Essay on the Circulation of the Blood; especially as observed in the Minute and Capillary Vessels of the Batrachia and of Fishes. By MARSHALL HALL, M.D. F.R.S.E. M.R.I. M.Z S.&c. &c.

DR. MARSHALL HALL, dissatisfied with the investigations of preceding inquirers on the subject of the minuter portions of the circulating apparatus, lately armed himself with one of Dolland's achromatic microscopes, and investigated the transparant parts of various animals with great care, and we must also say with novel and interesting results. Among the principal objects which engaged our author's attention, may be enumerated the minute anatomy of the capillaries in the extremities and in the lungs of some of the batrachia—the powers which circulate the blood—the influence which the brain and spinal marrow have upon the circulation—the influence of other organs on the heart and circulation. We have one or two additional points of inquiry, or description; and among the latter is an account of the caudal heart of the eel, which Dr. Hall has also minutely represented in an engraving, which exhibits this curious provision with extreme fidelity. The delineations, indeed, of the different parts, as they appear through the microscope, are excellent: we speak as eye witnesses, having seen most of them displayed in this manner. The physiologist who fails to make himself acquainted with this essay, will remain in ignorance of some of the most interesting observations which have hitherto been made on the subject to which they relate. Some idea of their importance may be gathered from the subjoined epitome of the matters discussed:—

1. The distinction between the ultimate minute arteries, the true capillaries, and the first roots and minute trunks of the veins;
2. The successive divisions of the minute arteries, the continual conjunctions and redivisions of the capillaries, and the successive conjunctions and occasional anastomoses of the veins;
3. The characteristic rapid flow of the blood along the arteries, and its retarded flow along the capillaries and veins;
4. The singular differences in the form and distribution of these vessels in the systemic and pulmonary systems; especially,
5. The more abrupt divisions of the arteries, the more crowded number of the capillaries, and the abrupt formation of the veins, in the latter;
6. The extensive power of the heart in the circulation, the irritability of the arteries, the want of evidence of irritability in the true capillaries, and the effect of the respiratory and other muscular motions upon the course of the blood along the veins;
7. The doubt whether the true capillaries be real vessels or mere canals;
8. The temporary independence of the action of the heart and of the minute and capillary circulation, of the brain and the medulla oblongata and spinalis;
9. The power of the heart to continue the circulation in the minute and capillary vessels, after its entire removal from the body, in opposition to the opinion of Legallois;

10. The independence of the circulation of respiration and of that part of the medulla on which respiration depends, in opposition to the opinion of M. Flourens;

11. The independence of the capillary circulation of a part, upon that part of the spinal marrow from which it derives its nerves, in opposition to the opinion of Legallois and the original opinion of M. Flourens;

12. The extraordinary difference of removing the brain and medullæ, at once, and in successive portions at distinct intervals;

13. The erroneous mode of explanation of this fact, given by Legallois; another suggested;

14. The temporary independence of the circulation in the minute and capillary vessels, of the entire nervous masses, brain, medullæ, and gangliæ;

15. The effect of opium and alcohol upon the batrachia;

16. The effect of alcohol applied to the brain and spinal marrow, upon the action of the heart and the circulation;

17. The effect of crushing the brain and spinal marrow; compared with

18. The effect of crushing other organs or parts, upon the circulation;

19. The general sympathy of these different organs proved by these experiments; and

20. The want of any physiological deduction as to the natural functions of the parts themselves individually;

21. The effects of irritants applied to the web, upon the vessels which pass between its membranes;

22. The impossibility of forming any deduction from this experiment, upon the nature and function of the true capillaries;

23. The singular phenomenon of a caudal heart or ventricle in the eel;

24. The test of muscular structure afforded by water of temperatures moderately higher than that of the blood.

CAUTION TO DISPENSERS OF MEDICINE.

On Wednesday last a verdict of manslaughter was found against the assistant of a chemist at Brighton, for dispensing a prescription by which Captain Burdett, of the Royal Navy, lost his life. It appears that oil of tar was substituted for a black dose.

MEDICAL GAZETTE.

Saturday, May 26, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—Cicero.

THE LATE BARON CUVIER.

While France and learned Europe are still engaged in deploring the loss of this illustrious man, and in pronouncing splendid eulogiums upon his memory, we shall take the opportunity of paying our simple tribute, in the shape of a summary of his life and labours: even the briefest record of these must constitute no mean monument of his fame.

George Cuvier was born at Montbelliard, in the year 1769. Yet he was, strictly speaking, not a Frenchman. Montbelliard, at the period of his birth, and for more than a quarter of a century after, belonged to the Duchy of Wurtemburg; it was eventually included by republican France in the newly-formed department of the Upper Rhine. The father of M. Cuvier was a Swiss: after serving for forty years in the French armies, and attaining the rank of a chevalier, he retired on a pension as Commandant of Artillery at Montbelliard.

The capital of Wurtemburg was chosen as the scene of young Cuvier's education: at Stuttgard he devoted himself to the study of mathematics, philosophy, law, and jurisprudence; and it deserves to be noted, that the chief object of the courses laid down for him was to qualify him for a place in the administration, for his family is said to have had some interest with, and some claims on, the reigning Duke Charles. But the French Revolution produced great changes. We find our student suddenly transferred from Stuttgard to the coast of Normandy. At Stuttgard he is known to have derived, from the mountainous scenery and other local peculiarities, many of those impressions which were afterwards so admirably developed in his maturer views of natural history; and at Stuttgard too he had tried his strength in the various learned societies which were found there. Removed now to his Norman residence, in the 20th year of his age, as a tutor in the family of a French nobleman, he had his first opportunity of indulging in those favourite pursuits which ultimately stamped him as perhaps the greatest naturalist that the world ever saw. It was not long till he had dissected, described, and drawn, almost all the fishes that were to be met with on the coast of La Manche; and here it was that he made his grand and original observations on the structure of the molluscous tribe.

We next find him in Paris, engaged as a private lecturer. He was soon appointed as an assistant to Professor Mertrud, in the courses of comparative anatomy at the Museum of Natural History; and immediately admitted a member of most of the learned societies of the French metropolis. Of the Institut, which was organized in 1796, he was one of the earliest members; and the numerous memoirs and discoveries which he laid before that body, rapidly extended his reputation, while they formed the bases of many of his subsequent publications. Before he was 29 he received the Chair of Natural History at the Ecole Centrale; and in 1798 published his first separate work—the *Tableau Elementaire de l'Histoire Naturelle des Animaux.* This was the basis of the *Règne Animal*; it presented a new arrangement of the whole animal kingdom—the principles of which, however, had been already given by him in a memoir on a new division of the white-blooded animals, written at the age of 26, when *he first* ventured to attack Linnæus's division of the invertebrata into the meagre and confused classes of insects and worms. His own division, at the same time it should be noticed, of the invertebrated animals into the three now well known classes, was not adopted by him until the year 1812.

In the meantime, the materials of his courses of lectures on comparative anatomy were attaining a vast extent, and he determined to give them to the public. With this view they were reduced to form by his two able assistants, MM. Dumeril and Duvernoy; and, in 1800, were accordingly published as the first two volumes of the *Leçons d'Anatomie Comparée.* M. Cuvier was himself engaged, during the compilation of those volumes, with the excavations of the gypsum strata of Montmartre, and the comparison of the bones discovered there, with the skeletons in the Museum. In the remainder of the work, which appeared in 1805, the parts which relate to the organization of insects and molluscous animals were exclusively his own, and probably no other anatomist then living was equally qualified to undertake those obscure and difficult subjects of zootomy. The Leçons altogether are known to comprise the most perspicuous, correct, and complete systematic view of the science to be found in any language.

He succeeded Mertrud, in 1802, in the professorship of comparative anatomy: and here we cannot help pausing for a moment, to contemplate the wide — the magnificent — range which he allowed himself, in the pursuit of his favourite science. Had his researches in comparative anatomy been conducted only in connexion with human, his illustrations and his contributions to the science could scarcely have extended beyond a few classes in the animal kingdom. If in connexion with geology alone, his knowledge would have been mostly limited to the hard parts. Zoology, unaided by his general acquirements in comparative anatomy, would perhaps have been in danger of suffering additional confusion of arrangement. But with his unrivalled combination of qualities of mind, and ardour in the pursuit of all those collateral investigations which should be brought to bear on the one great end in view, M. Cuvier was enabled to attain a degree of excellence, as a comparative anatomist, which has never yet been equalled, and, in the lapse of ages, may possibly be long ere it be surpassed.

When Buonaparte returned from Egypt, and, after having been declared First Consul, was vain enough to permit himself to be chosen President of the Institut, Cuvier, who was one of the secretaries of that body, was necessarily brought into frequent intercourse with his future sovereign. The grand scheme of organizing the schools of France made this intercourse still more intimate, and Cuvier was appointed one of the six Inspectors General, whose business it was to establish Lyceums in the thirty principal cities of the kingdom. It was while on this mission, at Marseilles, that he so profitably employed his leisure moments in investigating the ichthyology of the Mediterranean.

Those examinations of the Montmartre strata, constitute an æra in geology: they were the continued labour of many years, and were crowned by the publication of the *Recherches sur les Ossemens Fossiles*, in seven volumes, 4to. 1812—1824. In this great work M. Cuvier embodied not only the information which he derived from his personal inquiries in the neighbourhood of Paris, and on his several visits to the interior of France, and his missions to Italy and Holland, but the abundant stores of knowledge which he procured from the correspondence of the most distinguished naturalists in all parts of Europe. The success of his labours in identifying the fossil bones of the mammalia, deservedly obtained for him the highest celebrity, while it afforded the most convincing proof of the utility of zootomical knowledge, as applied to geological investigations, that has ever been found in the records of science. No naturalist, indeed, ever enjoyed such opportunities as M. Cuvier for those particular researches; but perhaps no man ever possessed, in a more eminent degree, all the requisite qualifications for their successful employment. His indefatigable industry and enthusiasm—his extensive general knowledge—the beauty of his language, and the elevated tone of his descriptions, with the grandeur of his views, and the sublime truths which they unfold, regarding the past revolutions of the animal kingdom and the globe we inhabit, must convince every one that there was really no other individual living who could have so successfully performed what he accomplished in regard to the restoration of extinct species.

We shall not attempt, in this limited sketch, to introduce any further remarks relative to the *Ossemens Fossiles*; nor do we think it necessary to more than allude to the eloquent *Preliminary discourse* with which the work commences,—a discourse which, even had its author never written any thing else, would have perpetuated his name to a late posterity. It has been translated into several languages, and the English version, by Professor Jameson, has gone through several large editions.

In 1808 Cuvier was appointed one of the Councillors for life of the newly-erected Imperial University—an office which gave him frequent occasions of discussing political affairs before the Council of State and in presence of the Emperor. He was sent, in 1809-10, to organize the academies of the Italian states; and his arrangements at Turin, Genoa, and Pisa, were found so advantageous that, on the return of the sovereigns of these states, the numerous measures adopted by Cuvier were preserved inviolate. Subsequently, in Holland and the Hanse-towns, the changes and nominations which he made were similarly respected and preserved; and, what is still more worthy of being recorded, though a protestant by country, birth, and education, he was yet sent, in 1813, to reorganize the University of Rome; and many of his improvements in La Sapienza were retained even after the return of the Pope.

It might be thought that the labour of collecting materials for such a work as the Fossil Organic Remains, and the task of arranging and describing them, were sufficient to have occupied the greater portion of M. Cuvier's life: but we have seen what some of his other occupations were, and to these we shall now add one fact more relative to the work just alluded to:—M. Cuvier found leisure not only to execute the drawings for, but even to engrave with his own hand, some of the most detailed osteological plates which adorn the several volumes.

But we have still other works of his to notice: his writings and researches were by no means confined to topics connected with the animal creation. He was in the habit of collecting, in his *leisure* hours, the most interesting historical facts illustrative of the progress of science, and of examining the scientific labours and composing biographical memoirs of the most eminent members of the Institut for many years back. Of these memoirs a collection has been published, in three octavo volumes, and they serve perfectly to establish his claim to an acquaintance with *all* the physical sciences, and the march of discovery in each of their various departments. The style in which they are written is easy, elegant, and perspicuous; and not a murmur has ever been breathed against their strict impartiality and truth.

The *Règne Animal* made its first appearance, in four volumes, in the year 1817—a work which is universally allowed to constitute the best outline of the present state of zoology and comparative anatomy. It was here that he may be said to have ultimately adopted his quadripartite division of the animal kingdom, after an experience of thirty years. Faults have been found, it is true, with this somewhat arbitrary division; it has been even characterized as not very uniform or philosophical, and as perhaps too much fettered by the author's early ideas of classification. But, on the whole, it is allowed to be a classical work, and is justly adopted as the great standard of reference by the ablest naturalists of every country. We need be the more brief in our notice of it, in consequence of its being, after the Preliminary discourse, that work of M. Cuvier's which is perhaps best appreciated by the English public.

It is pretty generally known, that the great undertaking on which his heart was set to the last moment of his life, was a complete system of comparative anatomy. For this vast enterprise, which he intended to surpass all that he had yet achieved, and to which he has alluded in all his publications—which was, in fact, the great study of his life, and the idol of his thoughts during nearly his whole career—he had prepared an immense series of drawings and engravings; and, indeed, the Museum of Comparative Anatomy owes its origin to his strong desire of accomplishing this beloved object. What he has done for the class of fishes—and which he always looked upon as by far the most important of all his contributions to the natural history of vertebrated animals—may be contemplated as an earnest of what he would have done. It is said that he and Valenciennes, his colleague, had collected materials for an account of above 6000 species of fish, 5000 of which, it was calculated, would occupy the space of not less than twenty octavo volumes.

We have now noticed, however briefly, the principal scientific occupations of M. Cuvier—but vast and all-engrossing as they seem, they must be absolutely considered as the pastime of his leisure when taken in connexion with his functions in the state. Those functions, in fact, he always considered as his original destiny—the business to the conduct of which his early education was directed. He was about to be named a Councillor of State by Napoleon, when the events of 1814 occurred, to prevent that high distinction: it was, however, delayed only,—for in that same year he was named to the office by Louis the 18th. In 1815 he was retained as a member of the Commission of Public Instruction, and charged with the Chancellorship of the University. During the "hundred days" his connexion with the Council of State was suspended—but even then he continued to hold his important situations connected with public instruction.

From all cabals and political intrigues M. Cuvier ever kept aloof—a course of conduct which was sure to gain for him, as it did, the insidious hostility of numerous partisans. He was chiefly engaged with state business relating to the administration of the interior, and the regulation of the non-conformist religious sects; and besides this, was intrusted as King's Advocate, to defend before the Chambers all the principal projects of law.

It was while on a scientific visit, some years ago, to this country, that he was apprized of his having been elected during his absence one of the Forty—constituting the Académie Française—the highest distinction that pre-eminent talents can obtain in France, or in Europe. Soon after, he did not hesitate to decline the Ministry of the Interior, being dissatisfied with some of the terms on which it was offered to him. In 1819 he was named President of the Interior of the Committee of the Council of State—a place which he was anxious for, as unconnected with intrigue, but demanding method, constant activity, rigid discharge of duty, and a thorough acquaintance with the laws and principles of administration. He was created a Baron after this—a spontaneous mark of favour from his sovereign; and received several Orders, both from the King of France and the King of Wurtemburg,—his early protector.

But we will not attempt to enumerate the various honours and marks of distinction heaped upon Baron Cuvier from all quarters, as for a like reason we have not ventured even to allude to all the various publications to which he contributed his powerful assistance; either attempt would fill several pages of our journal. We must even now draw our brief notice rapidly to a close.

With all his mighty achievements in science, and almost every branch of human knowledge, no man, were one to judge from his personal bearing, would seem to have achieved less. In the enjoyment of competent wealth, influence, profound learning, and the highest fame, M. Cuvier's easy and unaffected manners never failed to command the love and esteem of all who had the honour of enjoying his acquaintance.

Many in this country—which he again visited not very long ago—will ever remember him with veneration and delight. His very look was calculated to impress respect and admiration. In his domestic circle, his deportment is described as having been amiable in the highest degree. His younger brother, Frederic Cuvier, is very favourably known in the annals of French science: he was appointed keeper of the Menagerie at the Jardin du Roi in 1805, and rendered much valuable assistance in organizing the Museum of Comparative Anatomy. As a naturalist he stands high in the estimation of his contemporaries.

The library of the late Baron is a rich and vast collection, and his private museum is said to be of great value: both will be purchased, it is understood, at the expense of government, and the proceeds rendered available to the widow. Madame Cuvier, no doubt, will be competently provided for: she is to have, along with a pension for life of 6000 francs, the town residence of her late husband, his house in the Jardin du Roi.

The circumstances of the death of Baron Cuvier we shall append in some detail. His funeral honours were performed with unusual magnificence: they were attended by not less than between four and five thousand persons—including all those who were of any literary or scientific pretension in Paris; and the orations pronounced over him by the representatives of the different learned societies with which he was connected, were remarkable for their number and impressive eloquence. Their theme could not have been a more fertile one; for their illustrious subject was universally admitted to have been guided in his great career by nothing but the pure love of knowledge, and the ardent desire to benefit his race. His labours, in a word, will merit the gratitude and command the admiration of all posterity.

The last illness, and death of Cuvier, were attended with some very remarkable circumstances. On Monday, the 7th instant, he complained to his friend, M. Dumeril, for the first time, of an uneasiness at his stomach, with looseness of the bowels, for which he was advised to keep his room next day, and to take some remedies suited to the circumstances. This he declined doing, because he had to preside at a council of state, a duty which he accordingly discharged. On his return, he met with the physician who usually attended him, M. Allard, and whom he took the opportunity of consulting. Cuvier had experienced at breakfast that morning a considerable difficulty in swallowing, in consequence of which M. Allard thought it necessary to advise the application of leeches to the anus. On returning home, however, his patient betook himself to his scientific labours, in which he remained engaged from two o'clock in the afternoon till half-past five, when he was summoned to dinner. It was only then that he informed Madame Cuvier of M. Allard's having prescribed for him, intimating his intention of postponing the application of the leeches till his usual bedtime, being at that moment, he said, more inclined for his repast. He sat down, accordingly, to dinner; but scarcely had been helped to some soup, ere he found that he was almost entirely unable to swallow. M. Allard was now sent for, as were MM. Dumeril and Orfila. Leeches were applied in the evening; and in the course of the night he was bled from the arm; but neither of these means was followed by any relief. Next day, MM. Dupuytren, Biett, and

Koreff, were added to the attendants, and joined them in consultation. On Wednesday morning, it was resolved to administer an emetic, and four grains of tartarized antimony were dissolved in some spoonsful of water, but only a small portion of this could be got down. At this time M. Cuvier had no appearance of illness; he had no uneasiness, and no fever; but merely complained of extreme difficulty of swallowing, and of a gradually increasing inability to move the upper extremities, without there being the slightest perceptible change in these parts. The emetic did not act upwards, but produced several evacuations from the bowels. Twenty-four grains of ipecacuanha were afterwards injected into the stomach by M. Dupuytren, by which, however, no effect was produced; and some hours afterwards, four successive doses, of twelve grains each, were similarly introduced, but without any emetic effect resulting. The difficulty of moving the hands and arms went on gradually augmenting. Leeches to the upper part of the spinal column were now recommended; and next day (Thursday) cupping-glasses were applied along the back, but scarcely more than four ounces of blood were obtained. The upper extremities were now perceived to be slightly tumefied. Various plans were suggested in consultation, particularly the actual cautery, and blistering to the spine, the latter of which only was adopted, and even that it would appear but imperfectly. By Sunday the paralysis had extended, and in fact had become general, implicating the respiratory muscles; the lower extremities were absolutely motionless, and the stomach and bowels seemed to have ceased to perform their functions. M. Cuvier, however, retained his mental faculties unimpaired, and was fully aware of the danger of his situation, constantly expressing his regret at leaving so many works unfinished. "This hand," said he repeatedly, "this hand, which has performed so many dissections, and committed so many of the results to paper, is henceforth condemned to inactivity." Afterwards, as the symptoms continued to increase, he spoke, not merely of the loss of his limb, but of his death; and expressed, with deep regret, the necessity of leaving his Comparative Anatomy incomplete: in fact, he was engaged in remodelling this great work at the time he was struck with the fatal paralysis. On Sunday, at five P.M. he spoke with difficulty, and only to express the general uneasiness which he felt. M. Dumeril, when he quitted him, remarked that he would see him again early next morning; but Cuvier shook his head in such a manner as to express his belief that they should meet no more. In a few minutes after this interview he began to sink, and expired between ten and eleven at night.

The general impression among the medical attendants was, that M. Cuvier's complaint depended upon some lesion of the spinal marrow, attended with pressure; and this expectation was the more probable, as it appears that their patient, not long before his death, had met with a fall, in which he had strained himself in the effort to recover his balance. The body was examined on the 15th, when the following appearances presented themselves. The general aspect externally remained unchanged, except that putrefaction had already made some progress; the cranium was very large; the vertex flattened, and almost quadrilateral; the thickness of the parietes, particularly at the frontal sinuses, was very great; the right side projected more than the left; and the same was observed of the parietals; the occiput was also fully developed; the inner table of the frontal bone exhibited three nipple-like projections; the convolutions of the brain were very numerous and voluminous. The lateral ventricles were very much dilated, and contained a small quantity of reddish serum; the membrane lining them was slightly rugose; there were some calcareous depositions in the pineal gland; the origins of the nerves were sound; and the whole of the encephalon and spinal cord were without any alteration in texture, colour, or form. The œsophagus presented no appearance of disease, and all the viscera were sound. The processus dentatus was unusually large; and there existed a sort of bony projection at each point of junction between the vertebræ, along the whole anterior surface of the column. These, however, must have been long present, and cannot be regarded as connected with his death: indeed, they were supposed by the reporters, and with some probability, to be connected with the peculiar habits and gait of M. Cuvier. The entire absence of any postmortem appearance calculated to explain the phenomena attending M. Cuvier's death, has led some—particularly Magendie—to hazard the opinion that his illness was, in fact, but a modification of cholera, indicated in the first instance by diarrhœa, and subsequently by nervous depression, the intellect remaining unimpaired.

Sœmmering, as the extreme weights of the healthy human brain, gives two pounds five ounces and a half, and three pounds three ounces and three quarters—the great majority being intermediate between these two; and M. Berard, in some recent examinations, has arrived at nearly the same results. But the brain of M. Cuvier weighed three pounds ten ounces four drachms and a half, being much above the extreme weight mentioned by Sœmmering. Besides, the cerebellum and tuber annulare were compared with those of a male adult, and found to exceed them in weight only by a drachm and a half; so that, in M. Cuvier, the excess was almost wholly confined to the extraordinary development of the anterior lobes—that is, to the organ of the intellectual faculties. Again, according to M. Desmoulins, one of the characters of the brain with which superiority of intellect seems to be associated, is great extent of surface—resulting from the number and depth of the convolutions—so that a great expansion of this kind might be comprehended within a cranium of moderate dimensions. Viewed in this light, the brain of M. Cuvier was even more remarkable than with respect to its size, none of the distinguished anatomists who were present at the examination having ever witnessed convolutions so numerous, or enfractuosities between them so profound. It was at the upper and anterior part of the brain that this conformation was most strikingly developed.

APPEARANCES IN THE BODY OF M. PERIER.

The corpse retained an extraordinary degree of heat. The most remarkable circumstance about the head was *the thickness of the skull.* There was a little serous effusion under the membranes, but the brain was healthy. The stomach internally presented several portions of a bright red, from the minute injection of vessels in an arborescent form; similar spots presented themselves in various portions of the alimentary canal; and the mucous membrane was in several places very much attenuated. The lungs were healthy, the heart soft and flaccid, and the walls of the ventricles thinner than natural.

VACANT PROFESSORSHIPS IN EDINBURGH.

We understand that Dr. Christison is to have the chair of Materia Medica, as successor to the late Dr. Duncan. The professorship of Medical Jurisprudence, which will become vacant, is contested by Dr. Craigie, Dr. James Gregory, and Dr. Maclagan*. We earnestly hope that the selection will be made on grounds conducive to the interests of science—a consummation which, we fear, has been little advanced by some of the recent appointments in the Scotch Universities.

TREATMENT OF CHOLERA.

We have lately seen two letters from Dublin, in which it is stated that the saline treatment of cholera, as described in this Journal, has been extensively adopted, particularly in private families. In consequence of the statements regarding the Cold-Bath Prison being copied from our pages into some of the daily papers, many persons had provided themselves with bottles, filled with the salts, in due proportion, so as to be ready for immediate use. The mortality has recently very much diminished, and we observe in one of the latest reports, fifty-one recoveries and only four deaths. It will, perhaps, be remembered, that some discussion took place a few months ago, in consequence of the plan of injecting saline solutions into the veins having been recommended in this Journal as a *dernier ressort* in cases of collapse. Our readers will find at page 257 of our present number an account of the only instances, within our knowledge, in which this idea has been acted upon. The results are interesting and important.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

MIDDLESEX HOSPITAL.

There were admitted under the care of the surgeon, in the week terminating on the 8th of May—two cases of femoral hernia; one of gun shot wound of the thigh; one of aneurism by anastomosis of the lip; one of fractured ribs; one of scrofulous swelling in the axilla; one of morbus coxarius; one of diseased phalingo-metacarpal articulation of the middle finger; two of injury of the hip (bruised); one of injury of the shoulder, and two of ulcer of the leg.

Femoral Hernia—Death. Query: from inflammation of the peritoneum, or from the strangulated portion of bowel not having recovered its function?

M. C., 25 years of age, married, and without family, was admitted into Bird's ward on the afternoon of Tuesday the 1st of May, with femoral hernia of the left side. The patient had been subject to this complaint for five years, and had occasionally worn a truss, but so irregularly that she had sometimes left it off for months together, as had recently been the case. The swelling, never larger than a pigeon's egg, was only occasionally down, and upon pressure could always until now be returned. In the forenoon of Sunday last, the 29th ultimo, she was attacked with pain in the stomach, nausea, and vomiting, which she attributed to her rupture having descended that morning when she was putting up some lines. Failing in her attempts to return it, in the evening her medical attendant endeavoured to do so, and he thought he had succeeded. The following day she was tolerably easy, and was free from sickness; but in the evening vomiting recurred, and in the forenoon of the 1st inst. having again sent for the gentleman alluded to, he finding the hernia still present, after bleeding her to the extent of twenty ounces, and failing to reduce it, sent her to the hospital.

The tumor was the size of a small walnut, smooth, tense, and without pain; but there was some tenderness of the abdomen on pressure. Her countenance did not indicate suffering or anxiety; she was not sick, and had not vomited for some hours; she had had no motion from her bowels since the morning of the 29th; her pulse was feeble, and she seemed chilly. The taxis was employed ineffectually, and the effects of a clyster and a warm bath were [illegible]ed with as little success. The operation was therefore performed by Mr. Arnott, the assistant-surgeon. On opening the sac some brown-coloured serous fluid was discharged, and a knob of intestine of the size of a hazel-nut, of a dark red colour and smooth surface, was exposed. On relieving the stricture, which was very tight, and for which purpose a very small incision sufficed, the portion of bowel was easily passed back into the abdomen, and its reduction was followed by the escape of a considerable quantity of serous fluid from its cavity. This evidence of peritoneal inflammation, and the tenderness on pressure, led to the application of thirty-six leeches immediately on her removal to bed; and about an hour and a half after the operation she was bled from the arm until the pulse gave way. Three grains of calomel, were ordered to be taken every two hours; and blood was taken from the arm during the night, by Mr. Pogson, the house-surgeon. On the morning of the 2d instant she stated herself to be easier; the abdomen was less tender, bearing pressure, and there had been no motion of the bowels. A dose of castor oil was ordered to be given her, and a common enema. At half-past twelve it was found that the castor oil had not been taken, as she had been sick and vomited, but that ten grains of extract of colocynth had been given instead. She had had, since the vomiting, a recurrence of pains in the abdomen, and twenty leeches had been applied. The patient was now pale, looked distressed, and complained of great pain in the belly, increased on pressure; the pulse was frequent and somewhat hard, the tongue white and moist, the countenance a little flushed; no motion had followed the injection. nothing but a quantity of glairy mucus. She was again bled from the arm until she fainted, which occurred when she had lost about sixteen ounces of blood; the calomel was continued. At seven o'clock she was freer from pain, but the pulse was so frequent as not to be counted, and she was much exhausted. A grain and a half of opium was given directly. At eleven she had had some sleep; did not complain of pain; the tongue was moist and almost clean, but there had been no discharge from the bowels except some more glairy mucus. The castor oil was directed to be repeated. Early on the morning of the 3d, this patient, who had been brought in during the absence from town of her husband, had been by him removed to her own home, where she died in the course of the day, the bowels not having been opened.

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* Our Edinburgh correspondent, who supposes that this gentleman is a candidate for the chair of Materia Medica, is mistaken; it is for that of Medical Jurisprudence that his application at the Home Office has been made.

On examining the body the succeeding day, it was found that there had been a considerable discharge, after death, of serous fluid from the wound in the groin; and on opening the abdomen a small quantity of sero-purulent fluid was found in its cavity. The convolutions of the intestines were loose, and free from adhesions, and the portion of bowel which had been strangulated had receded to a distance of three or four inches from the crural ring; externally it presented an appearance of stricture, was of a dark red colour and smooth surface, the peritoneal coat being entire. The discoloured portion of bowel (not more than an inch in length, and not gangrenous) felt thinner than the healthy, and on slitting it up, the mucous membrane presented a breach of surface at the part corresponding to the strictural impression externally, transverse as regarded the course of the bowel, and with the exception of the portion where the mesentery is attached, the ulceration involved the whole circumference of the canal. The breach of surface was filled by firm, yellow, coagulable lymph. The canal of the intestines, above the portion which had been strangulated, was greatly distended with flatus, and its peritoneal coat in several places presented slight appearances of incipient inflammation—longitudinal red streaks formed by minute vessels.

Femoral Hernia—Recovery—Bowel in an unpromising state. Query: if less injured than in the preceding case, from the presence of a portion of omentum?

S. P., 65 years of age, was admitted into Bird's Ward, about 11 A.M. on the 8th inst. with a femoral hernia of the right side, which had been down since the afternoon of the 6th. The swelling was of the size of a large egg, felt tense, but slightly irregular, and was turned up over Poupart's ligament. After coming in, she had vomited some frothy matter, and bread and milk, which she had taken for breakfast. There had been no motion from the bowels since the 6th. There was no tenderness of the abdomen, but some tension and occasional pain. The pulse was 64. She had been subject to hernia for fifteen years, and had worn a truss, but not for some months. The hernia, when down, she had always been able to return until now, when she failed in effecting this. Being attacked with sickness and vomiting during the night of the 6th, the next day (that previous to her entering the hospital) she sent for a surgeon, who made repeated attempts to reduce it. This gentleman also bled her, and tried the effect of injections.

On her entrance, the taxis was ineffectually applied, and after a consultation it was determined that the operation should be performed forthwith; which was done by Mr. Arnott, who was acting for the surgeon of the week. On opening the sac, which was very thin, and from which no serum was discharged, it was found to contain a portion of omentum and a single small loop of bowel of a very dark-brown colour; the surface somewhat rough, with distinct appearances of coagulable lymph, and its coats enormously thickened, feeling quite fleshy. Such was its unfavourable appearance. It was suggested that it should be slit open, and attached to the edges of the wound; but this proceeding Mr. Arnott did not adopt. After dividing the stricture upwards and inwards, and drawing down a portion of the bowel from within the abdomen, shewing that there were no adhesions, he endeavoured, after pressing out its contents, to return the bowel, but failed — only the healthy portion which had been drawn down returning; the other, from the great tumefaction of its coats, could not be made to pass up. The hernia-knife was therefore re-introduced, and the stricture divided in another direction—namely, horizontally towards the pubes: but even after this, and when the point of the finger could be passed by the side of the bowel through the stricture, great difficulty was experienced in returning this single small loop of intestine, from the cause already mentioned. Ultimately, however, it was gradually and slowly reduced. The omentum was left in the wound, and, the skin having been brought over it and retained by two stitches, a compress and bandage was applied in the usual manner. A clyster which had been administered about half an hour previous to the operation, came away a quarter of an hour afterwards, and was succeeded by two or three discharges of fæculent matter;—by 10 at night she had had two or three more. At this time the patient was easy, the pulse 62, the abdomen soft and free from tenderness. On the morning of the 9th she complained of some pain in the belly, and Mr. Lonsdale, the house-surgeon, took ten ounces of blood from the arm, and directed sixteen leeches to be applied to the abdomen. As she had had no motion since the night before, a clyster was ordered to be given, and two grains of calomel with one-third of opium, every six hours. At 8 P.M. she was found to have shortly before vomited some orange pulp, which she had very imprudently, and contrary to order, eaten; and as the bowels had only been once scantily moved since the morning, seven grains of compound extract of colocynth, two of calomel, and three of extract of conium, in pills, were ordered to be given immediately, and repeated in six hours if necessary. This produced two healthy motions during the night, and some uneasiness which she felt in the abdomen was relieved by the application of a mustard poultice. In the course of the 10th, her pulse became more frequent, reaching to 120 in a minute, and so full that sixteen ounces of blood were taken from her arm; and at night, although her bowels had been relieved several times, her pulse was 106, so that eighteen leeches were ordered to the abdomen, and a pill, containing two grains of calomel, half a grain of powdered digitalis, and half a grain of opium, every four hours. On the 11th her pulse had got down to 90, and her bowels had been freely opened; the pills were discontinued. Towards evening she became so restless that fourteen ounces of blood were taken from the arm, and twelve leeches applied to the abdomen; the blood was thickly buffed, but not cupped.

On the 12th, though the tongue was clean and moist, and the abdomen soft, slight pain was experienced on pressure. The colocynth and conium pills were repeated, and acted. In the evening sixteen leeches were applied, and, as she had not slept the preceding night, thirty drops of laudanum were given to her at bed-time. The 13th she continued to improve.

As some redness of the integuments around the wound was now observed, the stitches were removed, the compress and bandage left off, and their place supplied by a poultice. For a day or two the discharge was thin, but it has now (May 21st) assumed a healthy character; the redness has disappeared, and the wound is partly healed.

Compared with the preceding case, the condition of the strangulated portion of bowel in this appeared much more unfavourable; yet it has done well. In what, then, did the most important difference between the two consist? In that first related, there was general peritoneal inflammation in existence at the time of the operation; in the latter there was not, nor did it seem that the inflammation ever extended beyond the incarcerated portion of bowel. The condition of the returned intestine was, it is true, a source of much anxiety; and it will be seen, by the treatment which was required, that some time elapsed ere the patient was out of jeopardy. In connexion with the favourable issue of this case, it may be worthy of remembrance that the pressure upon the bowel was shared by omentum, and, with the unfavourable termination of the former, that the bowel was exposed alone and unshielded to pressure, which produced ulceration of the mucous membrane.

Pistol Wound of the Thigh.

F. W. 16 years of age, was admitted into Percy ward on the 6th inst.; on that morning he had been putting a pistol, loaded with small shot, into his left hand breeches pocket, when it exploded. On examination, the skin on the upper and anterior part of the thigh, immediately below Poupart's ligament, was found burnt black, to the extent of a circle about two inches and a half in diameter, and beyond this there were some vesications of the integument. In the centre of the former was a ragged aperture, directly over the course of the femoral artery. On introducing the finger into this, it passed downwards and inwards, not between the muscles, but between the latter and the adipose tissue and skin. On the lad's admission, Mr. Lonsdale, the house-surgeon, felt the charge of the pistol lodged beneath the skin on the inner part of the thigh, about four inches from where it had entered; and having made an incision in this situation, he removed two pieces of wadding (thick pasteboard), and a quantity of small shot, as well as a piece of the pocket. On introducing the finger of one hand into this aperture, and a finger of the other into that in the centre of the burnt portion of skin already described, they could be made to meet.

The point of importance in this case was the vicinity of the femoral artery to the wounded parts; that it had escaped direct injury was evident from the course of the charge, and from its continuation being found pulsating in the ham; but as sloughing to a certain extent must necessarily ensue, it was possible that it might become subsequently involved. It was a great object, then, to prevent the extent of this slough, by the inflammation which would be set up. Leeches were therefore applied to the thigh round the wound. Sixteen ounces of blood, which was somewhat buffy, were taken from the arm, and in the evening it was found necessary to take fifteen ounces more. Salines, with antimony, were given every six hours. On the following day, although he had been purged, and continued to take the medicines prescribed, to which was added some tincture of digitalis, in the evening the pulse was so full, the lad feverish, and complaining of headache, that sixteen ounces of blood were again taken from the arm. On the 8th, the general symptoms were greatly relieved, but on account of the swelling and redness of the thigh, leeches were again applied. By the 10th this had considerably subsided, and the slough had begun to separate, attended with a very free discharge. By the 17th, the slough was completely detached, leaving a sore, presenting a healthy granulating surface. Simple dressing was now applied, and it is now (the 21st) healing.

Aneurism by Anastomosis of the Lip.

E. P. 20 years of age, was admitted into Handel's ward on the 4th of May, on account of a swelling in the under lip. It occupied two-fifths of this, and those adjoining the left commissure: this part was swollen, projecting considerably above the level of the healthy portion of the under lip; it was also somewhat everted, and of a purple colour. On laying hold of the part, its vessels were felt pulsating strongly. By pressure it diminished, but resumed its usual size when this was removed. The disease was developed in the substance of the lip, and not in its cutaneous or mucous covering; but on the latter there was an appearance of scales, leading to the apprehension that the part might here give way. The disease began a year ago without any cause, and when first observed, the swelling appeared to be the size of a pea. The diseased part was removed by excision, a triangular portion of the lip being enclosed in two incisions, as in the operation for cancer of this part. In doing this, Mr. Arnott employed a pair of forceps, (not unlike a pair of large dressing ones) one blade inlaid with wood, upon which the other and narrower one acted. Grasping the lip with this, he cut by the side of the narrow blade on the other, thus combining a straight with a rapid incision. One artery bled very freely, and after about ten ounces of blood had been allowed to flow, (the patient was a stout country girl) it was stopped by the touch of a hot wire. The parts were brought together by pins and the twisted suture; the wound healed kindly, and the patient left the hospital on the 16th instant.

REPORT OF CHOLERA, UP TO FRIDAY, MAY 25, 1832.

New cases in Great Britain (exclusive of London) since our last report ...	224
Deaths....	77
Total number of cases throughout Great Britain (inclusive of London) since the commencement ...	12837
Deaths...	5211

METEOROLOGICAL JOURNAL,

Kept at EDMONTON, *Latitude* 51° 37′ 32″ N.
Longitude 0° 3′ 51″ W. *of Greenwich.*

May 1832.	THERMOMETER.		BAROMETER.	
Thursday . 17	from 34	to 55	29 76	to 29·86
Friday. . . 18	38	62	30·00	30 09
Saturday . 19	29	66	30·07	30·11
Sunday . . 20	34	66	30·16	30·12
Monday. . 21	39	70	30·13	30·15
Tuesday . 22	44	70	30·15	30·13
Wednesday 23	40	71	30·16	30 22

Wind variable, S.E. prevailing. Except the 17th, generally clear.

Rain fallen, ·25 of an inch.

On the afternoon of the 17th instant we were visited by another storm of thunder and lightning, though not of so alarming a kind as on the 15th. At Enfield, a town about two miles N.W. of this place, hail-stones fell of an extraordinary size, measuring three-quarters of an inch.

CHARLES HENRY ADAMS.

NOTICE.

Mr. Middlemore's paper "On the Reproduction of the Chrystalline Lens" has been received, *but not* that "On the Malignant Diseases of the Eye."

W. WILSON, Printer, 57, Skinner-Street, London.

THE
LONDON MEDICAL GAZETTE,
BEING A
WEEKLY JOURNAL
OF
Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 22, 1832.

UTTERPARAH PUBLIC LIBRARY 1859

ABSTRACT OF A SECOND LECTURE ON INFLAMMATION;

Delivered during the Course of the Institutions of Medicine in Glasgow College, 1831.

By David Badham, M.B. Oxon.
And now Radcliffe travelling Fellow from that University.

On the Theory of Gangrene.

To attach some definite idea to the term inflammation, and, by separating it from affections with which it is often confounded, to assign to it a place, as well as a name, in our systems of nosology, is a task of which the difficulty has been constantly felt by the best writers on medicine. Inflammation is at present loosely understood to designate an "increased and altered mode of action in the extreme branches of blood-vessels," (Lawrence); but to wave at present any discussion respecting the truth of the first of these propositions—concerning which various pathologists have maintained different and even opposite opinions—it is impossible not to see that the diversified products of inflammation (as they are reputed to be) could never be brought about by any one altered mode of action—such action requiring to be as various and dissimilar as the assigned products themselves. These formularies, therefore, supply no definition of inflammation. But further, any disease may be attended with a changed action of blood-vessels; indeed almost every disease implies some abnormal vascular action in the seat of such disease, and hence the adoption of a definition applicable to almost all diseases cannot be said to fulfil the purpose it proposes. The very admission into medical language of such qualifying terms as suppurative inflammation, gangrenous inflammation, or ulcerative, may further shew the confusion of our ideas; they are evidently unphilosophical, and exhibit a want of simplicity and truth in our conceptions. To state that inflammation in one instance ends in gangrene, because the "travail inflammatoire" is carried to such a height that the part is incapable of sustaining it (Lawrence), or to conceive that, in another, suppuration results from a less degree of activity in the same agent, is to make, in the first case, an assertion contradicted by experience, and, in the second, to affirm that which is repugnant to reason; since it represents the only difference between suppuration and gangrene as being not of kind, but of degree;—whereas, if there was the least ground for such supposition, we might, by artificially exciting and controlling inflammation within the requisite degree, secure the manifestation of either of these states. Now this we know it is not in our power to do; nor does it depend on us to divert that state of parts which nature is preparing, to produce the one into the substitution of the other. But, moreover, inflammation (says Mr. Lawrence) by no means concludes when suppuration, ulceration, or gangrene, have taken place; for which reason he very justly objects to consider these as "terminations" of inflammation, while, to me, it seems equally objectionable to call them, with the distinguished author, "parts of inflammation." The frequent concurrence of inflammatory action with any stage of progress towards such terminations, ought not surely to conduct us to so considerable an error in language when we sit down to inquire into their pathology.

It was the object of the lecture which I had last the honour of delivering in this place, to inquire whether suppuration be, as commonly stated, a pure consequence or effect of inflammation, and not rather an action *sui generis* to which inflammation is not indispensible. The suppurating process is a secreting process; and therefore it may be further urged not inflammatory in its origin. We know nothing, it is true, of the actions which produce any secretion; we cannot even presume whether they are the same in the instance of each secreted product, nor, at present, can we even hope to arrive at the laws of that vital chemistry which must lend its aid to the mechanism of structure in giving origin to all new products. "We do not know how it is brought about (to use the words of a celebrated writer) that the capillary vessels of one part deposit the substance of muscle; those of another the substance of bone, and so on;" but though the present state of our knowledge afford no positive conclusions on these heads, yet we are agreed in never supposing an action of the nature of inflammation in any such case; nay, we see inflammation commence by destroying the secerning action—by arresting the further secretion of its fluid in a gland or membrane thus affected—and therefore derive the strong argument from analogy, that suppuration is not an inflammatory process, nor pus an inflammatory product.

We now proceed to the consideration of gangrene; and here, dividing what I have to say into two heads, under the first I must object to consider gangrene as a product of inflammation, and in the second I shall venture to offer my notion of its pathology. 1. Gangrene I hold not to be inflammatory in its origin, and though the onus of proving that it is rests with those who say so, I shall endeavour to meet the common-place arguments in favour of what I think a false doctrine—leading, as most false doctrines do, to corresponding mistakes in practice.

From the loose way in which many authors discourse on gangrene, it would appear to be merely the death of a part, or (to use the language of a French writer) "extinction of the life of a part, with re-action of the vis conservatrix of the surrounding ones," (Hebréard). Mr. Lawrence, with his usual clearness and perspicuity, speaks of gangrene as consisting in the death of a part, together with a peculiar change of structure in it—the result of a previous and peculiar vital action; "and so different from that death of a part which is involved in the death of the whole system, that some kinds of mortification are to be considered as complete preservatives against putrefaction:" hence also inferring the inutility of antiseptics in the treatment of gangrene, because the change which occurs in mortification is "not to be considered as identified with putrefaction." After reading these sentences, by the way, I cannot but wonder that the same eminent authority should elsewhere relapse into the loose notions of less informed and less accurate observers, and be found to say, that "when the cessation of vitality has taken place, the part then undergoes spontaneous or chemical change;" as if this change had not been stated by himself, and truly stated, to be of a very different nature.

Gangrene, unlike suppuration, is reputed to have various efficients—such as intense cold; the infliction of any serious local injury; the direct interruption of a supply of blood to a part by pressure or ligature; diseases of the heart; and, lastly, inflammation. Marjolin has assigned no fewer than ten distinct causes of gangrene, most of which may, however, be considered as referable to one of the above heads; and yet, correctly speaking, I conceive that no one of them is the direct efficient of gangrene, which, in my opinion, is to be sought in some common condition of all parts previous to their assuming that action which terminates in mortification. That inflammation, for example, is not an efficient, but only a remote cause of gangrene, I think quite clear; though the opposite opinion be the orthodox one. In fact, if it should, on a superficial view of the subject, appear that "in violent inflammation the disorder in the circulation is carried sometimes to a pitch which the part is incapable of sustaining, so that the blood stagnates and the part perishes," (Lawrence,) at other times the inflammation preceding gangrene is, we know, so very inconsiderable (as, for example, where it succeeds to the application of a blister, or of scarifications made to let out the fluid of anasarca), that nothing would be less philosophical than to attribute gangrene, in this case, to the "travail inflammatoire," to which it bears no proportion; while it seems equally repugnant to experience to explain it, with Mr. Lawrence, by "a comparison of the action with the degree of power in the part." This statement, indeed, which involves the notion that gangrene consists essentially in "excess of action over power," would preclude the necessity of "a peculiar vital action;" which the able writer whom I have so frequently alluded to had maintained in his very definition of the process.

In the next place, mere debility can never produce gangrene, whether such debility be determined to consist in the excess of action over power, or in an equal diminution of both. Consider, for instance, the condition of hybernating animals, and the examples afforded by parts of the body congealed by excessive cold. Here we have instances of a great depression in the energies of vital and organic life; yet, in the first example, gangrene never occurs at all, and, in the second, only when the parts are imprudently resuscitated;—neither does gangrene necessarily occur in constitutions weakened by scurvy, by valvular disease of the heart, or, in short, by any asthenic ailments; nor, lastly, does it always, or even frequently, occur in those cases of intense inflammation where the balance between power and action is wholly destroyed; thus it seems a fair inference that parts do not sphacelate in consequence of either excess or deficiency of their natural action, or by a destruction of the equilibrium which in health is maintained between action and power. They sphacelate in consequence of a new and morbid action being called into play, for which pathology has at present no name, but which might be called mortifactive, since mortification implies a passive state of parts, and the dead part must have been killed. In short, previous to "the death of a part, and re-action of the 'vis conservatrix' in the surrounding ones," some positive action of a destructive tendency must, but an inflammatory process need not, be supposed. Take in, too, the consideration that parts largely endowed with the nisus formations, or inherent power of repair, should have a corresponding fitness for inflammatory action, and yet that the muscular solid is little disposed to gangrene; while ligament, tendon, and bone, parts by no means equally capable of reproduction, or liable to inflammation, fall readily enough into gangrene; and reflect that it is in the extremities, where the action of parts, instead of being exalted, as in inflammation, is more frequently depressed, that gangrene most frequently supervenes, and I think we can hardly maintain this supposed relation of cause and consequence between the two processes of inflammation and gangrene. If, then, neither inflammation nor (by a process of reasoning nearly similar) those other conditions of body which precede gangrene, are in fact the agents which produce it, it remains for us to inquire, secondly, what that efficient may be. In order, if possible, to ascertain this, let us consider what is the state, and what are the appearances, presented by the part just previous to its mortifying.* Now, as inflammation often comes before gangrene, we may first give a moment's consideration to the state of inflamed parts. In local inflammations, we are told that "an increased quantity of blood is sent to the part;" as, indeed, the redness and the general congestion which we find throughout its tissues abundantly attest, besides which, Hunter proved the fact by actual experiment. But we are also taught by Mr. Lawrence, that "more blood actually circulates through the part than in a state of health;" which is, I think, not so satisfactorily to be made out. In proof of this fact, Mr. Lawrence calls our attention, first, to the state of fulness in the vessels. "If there were merely a larger quantity sent to the part, and it remained stagnant, you would not have the vessels distended, as you will find takes place in inflammation of the hand and forearm." Now it is quite certain that distention is not caused by motion, but by the quantity of material moved, so that the turgescence of blood-vessels is not the consequence of an accelerated, any more than it is of a retarded, circulation; it can have no other cause than a redundancy of blood, which would produce the phenomenon of distention equally in motion or at rest. But, secondly, Mr. Lawrence would, it appears, infer a greater circulation of blood through an inflamed part, from the circumstance of more blood flowing out and from a larger number of vessels yielding it, during the performance of a surgical operation on an inflamed than on an uninflamed portion of the body. "In a case where the hand and forearm were inflamed, I had an opening made (says this gentleman) in each arm; and I found that, within the same space of time, dividing the veins at the same instant, about three times more blood flowed from the vessels of the inflamed limb than from those of the sound side." But might not the more copious blooding which followed the division of the vessels of the inflamed arm be owing to the increased development of the size of some vessels and of the formation of new ones? When there is an "appel des fluides," the part must necessarily become more vascular, in order to accommodate the increased supply of blood which it receives. More blood would also, I conceive, continue to flow from the veins of an inflamed than of an uninflamed part, because large vessels, and these increased in number, would withdraw more blood to fill their canals out of the circulating mass, as a series of pipes will derive from the main trunk which fills them, more or less, in proportion to their number and to the sum of their respective capacities. I do, however, believe that more blood is actually circulated through an inflamed part than in the same part in health, and would cite in proof of this opinion, 1st, the florid red colour of the skin; 2dly, its increased heat; and, 3dly, augmented sensibility;—one and all of these being consequences of the larger presence of arterial, not of venous blood. But arterial blood becomes venalized by confinement; as may be proved by compressing the finger tightly—when the part beyond the ligature is seen to become blue, to lose its sensibility and temperature, and to afford dark blood on being cut. We may, therefore, safely conclude, that in inflammation which is characterized by opposite phenomena, that the arterial blood so copiously sent into the part affected is returned with equal readiness, and to the same amount, by the veins; in other words, that the circulation of the part is augmented, and this augmentation would seem, within certain limitations, to be in proportion to the intensity of the inflammation. But if "inflammation be carried beyond a certain point," the part may then come to mortify; because its circulation being now prevented, the vitality which is dependent on that circulation must also cease, and it perishes in consequence." Now two questions seem to occur here, which have not received that attention from pathologists which they deserve: in the first place it may be asked, How does intense inflammation lead to the suspension of the circulation?—and in the second, by what "immediate circumstance is the act of mortification called into existence?" In reply to the first, it may be sufficient to observe for the present, that although inflammation seems to be a stimulus to the minuter vessels, whereby they more readily forward their contents, yet, if the exciting cause be violent, and the "appel des fluides" so great as to overcharge and distend rather than to stimulate, then the coats of the vessels, being overstrained, will come either not to act at all, or to act so feebly as not sensibly to promote the flow of blood through them: this fluid, therefore, already accumulated, must shortly stagnate in a part so circumstanced; discolouration, loss of sensibility, and decrease of temperature, accordingly follow, and the tendency towards mortification has commenced. Thus we see how an organ under inflammation (that is, an organ of which the action is primarily exalted) may come to have no action at all, and how the congestion which occurs previously to gangrene must of necessity take place—for such congestion, or stagnation of the blood, I hold to be as indispensible to the production of all gangrene, whatever be the remote cause, as it is the obvious condition of a part tightly compressed—in which case the blood may go to the part, but cannot be returned from it. Gangrene may also occur from chemical causes; as from the blood being impoverished, or deteriorated, so as to act no longer as a stimulus to the blood-vessels. Ossification of some of the arteries is a mechanical cause of gangrene on the same principle. Here one of the circulating forces is destroyed, so that the blood can no longer be propelled with the requisite momentum through these tubes into the capillaries, as it does when the arterial structure is unimpaired;—in short, whatever has a tendency to produce stagnation of the blood, has a tendency to produce gangrene.

Let us now inquire what may probably be the immediate cause of that action which terminates in gangrene? A portion of the blood sent from the common source of animal temperature and of life, that is, from the lungs destined to carry vitality and heat over the body—is, under some of the foregoing circumstances, arrested in its return; it speedily imparts and parts with the vivifying principle to the texture in which it has become, as it were, included: it soon ceases to be arterial, and becomes wholly unfit for the support of life; by degrees the animal temperature, which is the measure of vitality, is found greatly to have abated; fresh arterial blood is kept from coming to the resuscitation of the part by the conditions alluded to, while the noxious influence of venous blood operates upon it after the manner of an applied poison. The vehicle of life has become that of destruction, and it is probably owing to the directly poisonous influence of *effete*, rather than to the mere want of fresh supplies of healthy blood, that mortification ensues. The part, then, does not die; it is killed. The process of mortification is a "morbus verenatus" set up in the organ by the noxious properties of undecarbonized blood; and by this principally, if not entirely, the parts are stimulated to the last action of which their expiring energies are capable.

Let me observe, in conclusion, that the circumstance of the circulation being nearly suspended in hybernating animals, and quite suspended in parts that have been frost-bitten (in the first of which cases mortification never happens, and in the second not of necessity), does not, in reality, make against my views concerning this deleterious influence of venous blood on parts where it is lodged, and its agency in the production of gangrene. In the first case (hybernation), the stagnation is never complete; the retardation of the circulation is uniform over the body, so that no part can be said to be congested; and, finally, the diminution of the actions of vital and organic life are also in strict relation with the diminished powers of the bodily organs. Hybernation is not disease. The second case (that of frost-bitten parts), where congestion of vessels may be supposed, these parts do not mortify, perhaps, exactly because they are frozen: their congealing is probably a provision against their sloughing—which it may prevent by rendering them incapable of action; and the nature of their tendency to mortification is well illustrated in the facility with which they are observed to fall into this state on being thawed.

PHENOMENA OF DOUBLE VISION.

To the Editor of the London Medical Gazette.

Hackney, August 28, 1832.

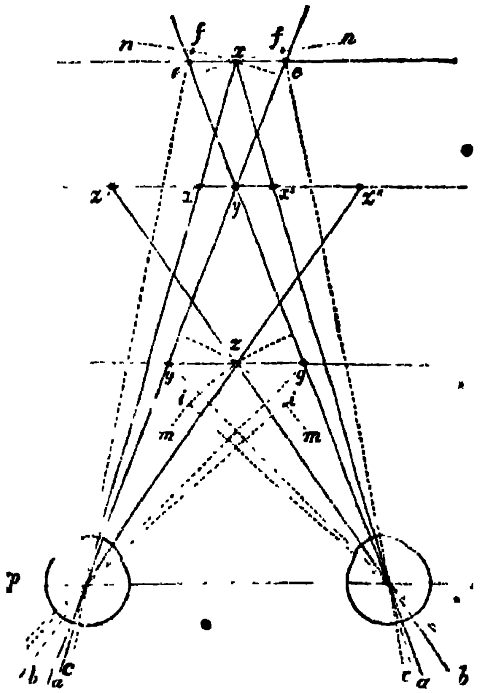
SIR,

I TAKE leave to offer a few remarks on a debateable point which has been set aside in pursuit of another object in your discussions with Dr. Graves; the Doctor having merely denied its probability, and yourself resolving to maintain it till a better hypothesis be substituted. I allude to a theory which you advanced at first, and which I have, in the following paper, endeavoured to disprove by reducing it to test, and demonstrating some difficulties connected with it; and have offered another theory grounded partly on my former letter, which, I think, will appear more plausible. I beg the favour of your examination and publication of both, when your pages may not be better engaged, and remain, sir,

Your very obedient servant,
GROVE BERRY.

The hypothesis advanced is in the first column of p. 551, beginning with the words, "the eye, as it appears to us, in viewing any number of objects at once, *projects* their images into one and the same plane, or sees them as if the pictures of those objects were represented to it in a determinate surface," &c. &c.; and is repeated in a condensed form at p. 615, and there advocated as the result of "further reflection." To enable one fully to appreciate the theory advanced, I have endeavoured to represent it in the following diagram, which, I think, gives a correct view of it, and then endeavoured to examine the effects of such a position of things.

The *entire* lines are all that are now referred to; the interrupted or *dotted* lines will be used afterwards.



Let p p represent the plane of the eyes drawn through their centres; x y z the objects at different distances from that plane; 1 x, 2 y, 3 z, the planes of ditto, parallel to p p.

a a the lines of the axes of vision, meeting on the middle object y, and forming the angle a y a.

b b the lines of the incident ray, or those by which the image of the near object, z, travels to the eye, and which, if the object be seen in the direction of these lines, must intersect with the object; and which, therefore, since light flows in right lines, must be that line by which z attains its place at z'' z'', the plane of the primary object y, on which the axes of vision meet, having attained these places "by projection."

c c, the lines of the incident rays, or those by which the images of the more distant object, x, reach the eye; but which images it is unnecessary that the eye should "project," as in their passage they have to cross the plane of y, the primary object; they are projected by the object itself, and the eyes are supposed to arrest or "intercept" them at the points x'' x''.

Now, first, if the images of z be seen by the eyes at z'' z'', they must be seen *through* the object itself; and, consequently, if the object be opaque, it cannot be seen at all: or in other words, the *image* must be "projected" *through* the object.

But, secondly, allowing that they were seen at those points, they are carried so far from the eye to attain such a position, that the object they represent should appear, although in the same plane, to be at a still greater distance from the eye which receives the impress than the object y itself appears to be, and which we know to be further off, and does not fail to appear to be so; and this is the consequence of the greater declination of the line b z z'', which makes it longer than the line a y, this latter being nearer to the perpendicular of the planes. But especially if the point of decussation of the lines of visible direction be still anterior to the object, while its images yet attain the primary plane, their declination from the perpendicular must increase, and the apparent distance of the object must also increase in like manner.

Thirdly, by actual experiment, if that described at p. 635 be repeated, the image of z will never appear to be so far removed from the line of axis of the eye as x'', which this diagram would represent it to be.

Again: by parity of reasoning, x, the most distant object of all the three, (allowing that the eye *can* arrest or "intercept" its image, certainly the only way in which it can attain that plane,) x should actually appear to be the nearest; because x'' x'' are its images, supposed to be brought to the plane of y; and the line $c\,x''$ from the centre of either eye, is shorter than $b\,x\,x''$, or even $a\,y$, measured from the same point, because it approaches most nearly to the perpendicular of the planes, or its declination is less.

It is, therefore, an inseparable consequence, from this theory, that the relative distances of objects viewed under the given circumstances, should be reversed, the nearest appearing farther off, and *vice versa*; and yet this is the only way in which the images of all objects, whatever their distances be, *can* attain one and the same plane—that of the "primary" object; because it is allowed that the images of the near object do cross, and it has been shewn that the images of the more distant object do not and cannot cross.

There may be other reasons that I have not hit upon, but these appear to me sufficient to invalidate the theory.

The following is that which I offer as a substitute; and the dotted lines of this diagram are those I use for explanation. I have purposely used straight lines, wishing to give an entire diagram, and avoid the confusion which must have arisen from the use of refracted lines in so small a compass.

I imagine each image to be in the plane of the object which it represents; and this I humbly conceive more rational than a removal from that plane would be—at least to the extent required, as I have shewn, by the former theory. The old diagram, at p. 636, will shew in what way I conceive this *may* occur, and I shall aim to shew *why* it may do so. But that diagram also shews that the image is not *at precisely the same distance from the eye as is the object*; this is a necessary consequence of the different degrees of declination of the line of visible direction and the line of the incident ray. The difference, however, as I shall aim also to shew, can be but barely appreciable at even a few feet distance, and not at all so at one hundred yards.

First, with regard to the nearer of two objects, as z, the further object, y, being the "primary" one.

It is shewn in the diagram at p. 636, as well as in this, by the dotted lines from g to the focus, or rather central point, in what way and where the images cross so as still to be in the plane of the object; the point of their decussation being, *not in the* object, as the above hypothesis requires, and as Dr. Graves's diagram serves unquestionably to shew, though his arguments disprove it; but this point would be considerably *anterior* to the object and nearer to the eyes; which must necessarily be the case when the images, while yet in the plane of the object, are carried away from it along that plane.

I conceive the image of the object z to be seen at g, which is situated at the point of intersection of the line of visible direction with the line of the plane, and also (as it happens, and will always happen when the object is equidistant from either eye) also with the line of axis of the other eye; and I think this to be the point because it will always measure (according to a rule which I mentioned in my former letter, at p. 636) the same distance from the object as the object itself does from the axis of the eye which apprehends the image. There is one other reason I shall give when I have mentioned an experiment on which it is grounded. Now, if this *be* the place of the image, it manifestly represents z to be farther off than it really is; and I think the following experiment will shew such a representation to be correct in point of *fact*, though it is difficult to say whether it is so in point of *degree*.

If a person hold a book, for instance, in one hand at his ordinary reading distance, fix his eyes on a particular spot on the page; now hold a finger of the other hand within an inch, or half an inch of his nose, on a level with the eyes, and equidistant from either; if he now shut one eye, keeping the other fixed on the book, the thumb will actually appear to move away to a much greater distance than we know it really is; so far will it move, that it will seem to be opposite to the other eye, or on the line of its axis of vision. But while the thumb moves laterally along its plane, it does not appear to leave its plane, and come nearer to the other eye in point of anterior distance, as may be proved by adjusting one of the straight printed lines of the book to its movement; but yet which it ought to do if its *true* distance were represented, and which I will now endeavour to show.

If the image which we receive of *z*, give the eye a correct idea of the distance of *z*, the image should be seen, not opposite to the other eye, but somewhere in the arc *m*, *i*, *z*; and as the image appears (as shewn in the former letter) to be just as much removed from the object as the object itself is actually distant from the line of axis, the point *i* should be the spot where the eye sees *z*, and the lines of visible direction would thus decussate still nearer to the plane of the eyes than in the former case. But I think the above-mentioned experiment will prove that the image of this near object, at any rate, does not give an accurate idea of the distance of the object. On the other hand, I think it will also shew that the image is seen, as nearly as we can judge of it, at the point *g*. Whether the explanation I have advanced for its being at that spot be correct and satisfactory, I leave others to judge. (If it should be said here, on the part of the former theory, that the line by which an image is projected into the primary plane, be either of these dotted lines—see *b*, *b*, in the explanation of the diagram—then the idea of the object's distance given by an image in such a direction, must be still more inaccurate, because the declination of either of these lines is still greater than that of *b z z*.")

And now with regard to the more distant object, *x*, the "primary" object being *y*, a nearer one.

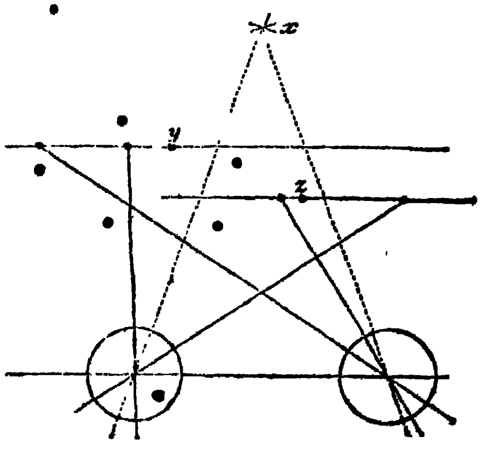
For the same reasons that I have considered *g* to be the place of *z's* image, do I consider *e* to be the place of *x's* image; for though the axes of vision are now differently disposed, because the object is not within the angle which the two axes form, yet the measurements will prove to be the same, because the object is equidistant from either eye. The point *g*, however, represents *z* further off than it really is; but *e* represents *x* to be nearer than it actually is: both are the consequences of the difference in degree of declination from a perpendicular to the planes, that each line assumes.

It has been, I think, proved that we do not gather a correct idea of distance in reference to the *nearer* object; but it is difficult to prove such to be the case in reference to a *distant* object, because the degree of declination of the line of its image must be so extremely small. In the diagram, however, where the distance of *x* from the eyes is but small, the degree of inaccuracy of distance may be shewn.

If in this case, also, we do gather a correct idea of the distance of an object, its image must be situated somewhere in the arc *u x*; and the law, applied before to the other case, now applied to this, shews that point to be *f*. But even at the distance of these few inches the deviation is but trifling, and how much more must it be diminished when the distance is increased! To explain what I mean: suppose the centres of the eyes to be two inches apart, the lines of their axis meeting on a point at six inches perpendicular distance from the middle of that base line, the angle they form would measure about 18° 30′; if the point be carried to nine inches, the angle would measure only about 12° 30′; if to twelve inches, the angle would now diminish to about 9° 30′: how few, then, must be the minutes, or even seconds, to say nothing of degrees, which would be required to measure the angle if the point were at 100 yards distance!

The deviation then, if it *do* occur, must in reference to distant objects be quite inappreciable; and thus, though our idea of the distance of such an object seen under these circumstances be not mathematically correct, yet it would be impossible for the unassisted eye to detect the error, and much more so the amount of that error.

The same rules, with certain modifications, will obtain in reference to objects that are not equidistant from both eyes; and if the experiment be tried, matters will be found to assume such conditions as I have represented below.



x represents the object on which the axes meet.

z, an object at a shorter distance, and within the angle; its images are represented on the plane of the object, as seen by each eye.

y, an object out of the angle, but still nearer the plane of the eyes than x; its images are represented both on its plane.

Though this may not be the philosophical mode of explaining the relations of images to their objects, which ought to be in accordance with various optical laws I have not noticed; yet if this method, or examination, be correct, (and the line of direction always appears perpendicular to the point of the circle or concave retina from which it proceeds) it may be adopted as an exceedingly ready and easy one—the relations of the images, *on* the plane of the objects, to the fixed lines of the axis of vision.

I have chosen to anticipate the objection to the foregoing theory as to distance, because it could not fail to be detected in examining the whole; and I have entered into the subject to shew *how much* it would interfere with its application, though it does not immediately belong to it. A comparison of the two theories will shew how much *less* it interferes with that I have advanced, than it must do with that I have combatted.

Lest I should be thought to have been unnecessarily precise in my explanations, and thus chargeable with prolixity, I would urge that I conceive our utmost precision in the use of the most apt language to convey an idea, is nothing in comparison of the extreme precision with which we adjust our two eyes to an object, in order to acquire distinct and correct vision; the former being a matter of *reason*, the latter of *instinct*.

[It is remarkable that Dr. Graves's theory, Mr. Berry's, and our own, give three several places for the images of objects seen double. This is differing not merely in matter of opinion, but in matter of fact. The readiest and simplest—and perhaps most convincing—way, would therefore seem to be, to try the question by actual admeasurement. So far as we were able to satisfy ourselves on the point, when our attention was directed to it a short time ago, the result favoured, and indeed founded, the theory which we ventured to propose. But we will not be positive: when we can command a little more leisure than we have at present, we shall certainly return to the subject, and bestow our best consideration on the views of both our opponents.—Ed. Gaz.]

BOTANY OF NEW ZEALAND;

BEING A

Description of Trees, Plants, &c. indigenous to that country.

By G. Bennett, Esq. F.L.S. M R.C.S. &c.

[Concluded.]

Coprosma fetidissima, Forster.
Karamu, or *Patété*, of the natives of New Zealand.

Natural Family, *Rubiaceæ*.

Class, *Pentandria*. Order, *Dygynia*.

The Coprosma fetidissima is a shrub indigenous to New Zealand, and is named Karamu, or Patété, by the natives. I have usually found it growing on the banks of rivers, or in the vicinity of the sea coast; its leaves have a disagreeable smell when rubbed on the hand, which has occasioned its specific name; the flowers are solitary, and of a white colour; the berries are of a bright red colour, and two-seeded. I collected specimens in flower on the banks of the River Wykéré, Bay of Islands, on the 8th of July, 1829. The leaves of this tree are used in one of the ceremonies among the New Zealanders, in which the Rakau Karakia, or praying sticks, are employed, (Rakau signifying a stick, or piece of wood, and Karakia praying). They are used by the chiefs of New Zealand, through the Tohunga, or priest, to discover the will of the gods, or spirits, respecting war, &c. The ceremony is as follows:—

A stick, or piece of wood, is procured for every separate party, and a leaf of the Karamu is tied on each with the Vivi (Scirpus Sp.), or with flax. The Karamu leaf is on one side of the stick, a knot of the Vivi, or the flax which ties on the leaf, on the other; it is considered immaterial which is at first placed uppermost.

The sticks, or pieces of wood, are

then laid in order on the ground, after which the chiefs and people retire to some distance, and the Tohunga, or priest, places himself a short distance from the sticks, and prays for a short time, after which the chiefs are desired to approach. The sticks, on being examined, are found all moved from their original places; some will *disappear*, which is considered a certain sign that the party they had intended to represent will be destroyed. Others will be found turned over; if the knot should be found turned down, the sign is considered bad, because the Karamu leaf, which is supposed to represent the *spirit*, will be uppermost. On the contrary, if the knot should be uppermost, and the Karamu leaf underneath, the spirit is considered defeated; and it is therefore looked upon as a sign that the party represented by those sticks will be prosperous in their undertakings.

The Miro Tree, of the natives of NEW ZEALAND.

This is a tree of the natural family Coniferæ, but the specimens collected were neither in flower or fructification. It is named Miro by the natives of New Zealand, and attains the height of from thirty to forty feet, and from six to eight feet in circumference. The wood is hard, and of a reddish colour. It is not used by the natives for any particular purpose. A gum-resin exudes from this tree, which is of a dark-reddish colour, but is not used by the natives as a masticatory, like the other gum-resins, on account of its having a bitter taste. The tree is abundant on elevated land, and grows in good soil.

CRYPTOGAMIA.

Cyathea dealbata.
Ponga of the natives of NEW ZEALAND.

This beautiful arborescent fern grows abundantly at New Zealand, on the declivities of hills, and is usually found enjoying the shade afforded in the forests by the towering and wide-spreading branches of the more elevated trees; it attains the height of fourteen or sixteen feet, and the length of the fronds are from three to five feet. Above, the fronds are of a dark-green colour; underneath, of a beautiful silvery white, which gives to it when waving to the passing breeze a rich and beautiful appearance. The trunk in circumference is seldom more than a foot to a foot and a half, and is, as well as at the attachment of the fronds, covered with a profusion of chaffy scales.

Externally the trunk of this tree is composed of a black substance, as hard as ebony, which is also continued into the interior, intersecting the white pith-like substance of which the greatest portion of the interior is composed, and which is usually eaten by the natives. When the tree is cut down, an adhesive juice exudes in some quantity.

This fern is the Cyathea dealbata of botanists, deriving its specific name from the elegant white appearance of the under surface of the leaf; and it is named Ponga by the natives, who use the trunks as posts in the erection of their houses; and it is found very durable. The internal part, which is principally the medullary substance just mentioned, soon decays, but the external will remain for a great length of time undecayed.

I observed at New Zealand two other species of arborescent ferns, growing in marshy situations, but am unable to decide on their generic character, from their being destitute of fructification at the period of my visit (May 1829). These, in beauty of appearance, and magnificence of growth, surpassed even the species just described.

I had heard the natives speak of two other species, when they observed me admire and collect specimens of the C. dealbata; but, in answer to my inquiries, they stated that those were only found in marshy situations, but they were not at that time aware that any grew in the vicinity of our present anchorage (Wyshaki Cove, River Thames, New Zealand). A native, one of my *botanizing friends*, who usually accompanied me in my herborisations (and appeared to take some interest in my pursuits, either from a *natural love of science*, or *of the tobacco* with which he was usually rewarded after our daily excursions), directed me to a place, on the 23d of May, 1829, where I could observe them growing. On descending from a woody hill, by a rivulet encompassed by dense vegetation, I observed these tree ferns. The largest was about twenty feet in height, and about two feet in circumference. The principal object of attraction was the large size of the spiral stipes, and the enormous extent of the fronds; the trunk, stipes, and central stalks of the fronds, were of a beautiful shining black colour, the length of the fronds being from sixteen to eighteen feet, and the leaflets from two to three feet. This splendid fern was named Korau by the natives. Not far distant from it grew the other species, named Feki by the natives; it attains the size of the Ponga both in the trunk and extent of fronds, but the leaflets are smaller. The stalk and under part of the fronds are distinguished by being of a yellow colour. The two last species grow in marshy and shady situations.

Melicytus ramiflorus, FORSTER. *Myhoé* of the Natives of NEW ZEALAND.

Natural Family, *Terebinthaceæ.*

Class, *Pentandria.* Order, *Monogynia.*

This tree is the Melicytus ramiflorus of Forster, the Myhoé of the natives of New Zealand; it grows abundantly in, and is indigenous to, New Zealand. It grows to the elevation of fifteen or twenty feet, of small circumference and branchy; the fruit (the only state in which I have seen the tree) is very small, growing singly from the branches; is a berry, of a purplish colour, and containing several small seeds. This tree is figured in Parkinson's drawings, made during Cook's first voyage, now deposited in the British museum. The leaves are of a light green colour, ovate, and in a slight degree serrated at their edges.

From the wood of this tree, when in a dried state, the natives readily procure fire by friction.

Aralia polygama, BANKS and SOLANDER'S MSS. *Paté* of the natives of NEW ZEALAND.

This is an unpublished species of Aralia, the A. polygama of Banks and Solander's MSS. deposited in the British museum, and is accurately figured in flower in Parkinson's drawings, made during Cook's first voyage, deposited in the same museum. It is a tree of slender growth, attaining the elevation of about twelve feet, with a trunk small in circumference, and containing much pith. The leaves are in colour of a dark-green, waved and epinated at the edges, digitated, but vary in the number of digitations, some having seven, others nine. This tree is named Paté by the natives of New Zealand, and the wood is used by them for the purpose of procuring fire by friction. It is found usually growing on elevated land.

Kaiko-mako tree of the Natives of NEW ZEALAND.

From the specimens collected of this tree, being neither flower nor fruit, its generic character cannot be determined. It attains the elevation of twenty-five to thirty feet, and a circumference of two or three feet. The wood, when in a dried state, is used by the natives for the purpose of procuring fire by friction, but is not for any other purpose.

The wood of the trees just described, the Myhoé, Paté, and Kaiko-mako, is only used by the natives for procuring fire. When I asked them why no other kind of dry wood would be equally serviceable, the reply was, "that there was more trouble in procuring fire from the dried wood of other trees than from the three just mentioned."

Elæocarpus dicera, FORSTER. *Inau* of the natives of NEW ZEALAND.

Class, *Polyandria.* Order, *Monogynia.*

This tree, the Elœocarpus dicera of Forster, the Inau of the natives, is found abundant on the hills of New Zealand; the leaves are ovate and serrated; the fruit a drupe, small, ovate, and of a dark-brown colour, when mature. This tree attains the elevation of thirty feet, and eight or nine feet in circumference; the timber is heavy and hard, but not very durable.

The bark of this tree is used by the natives for dying a red (and afterwards a black colour, by the aid of mud), similar to the species of Phyllocladus, or Tanakaá, before mentioned. The mode of procuring a black colour by soaking an article previously died in mud, is not peculiar to New Zealand. At the Sandwich Islands the edges of the roofs of some of the principal houses are formed from fern leaves, which are dyed of a black colour by being steeped for some length of time in the Taro mud—that is, the mud in which the plant named Arum esculentum, or Taro of the natives, grows [*]. The natives, however, first dye the article red, with the bark of the Tui-tui, or Candle nut-tree (Aleurites triloba), previous to steeping it in the Taro mud. At Tongatabu, a similar process of dying red and black is adopted as at the Sandwich Islands.

Among a numerous collection of plants I made at New Zealand, were also the

Senecio tabidus (Banks and Solander, natural order, *Compositæ*), and found growing abundantly on the beach.

Veronica salicifolia. *Gorokiu* of the natives; growing abundantly, more particularly about the village of Kororadeka, Bay of Islands.

Corckorus sloanoides (Banks and Solander's MSS.); *Iwan* of the natives.

Cineraria dealbata (Banks and Solander's MSS.); *Boka-boka* of the natives. This species of Cineraria is a shrub of slender growth, of the natural order *Compositæ*. The leaves are oblong, of a light green above, and underneath white and tomentose. It is figured in flower in Parkinson's drawings, made during Cook's First Voyage, now deposited in the British Museum.

As the New Zealanders named our books and white paper Boka-boka, I at first thought that the white appearance of this leaf at its under surface might have caused them to call it after our white paper; but I found my conclusion was incorrect, for, on the contrary, they named our white paper Boka-boka from its resembling the under surface of the leaf of this shrub.

Avicennia resinifera. *Máá-noa* of the natives.—This species of Avicennia is a small shrub found growing in marshes to which the salt water has access; the leaves are thick, ovate-lanceolate, downy underneath, opposite, entire, acute, and of a dark-green colour above; whitish underneath. The peduncles are terminal, and loaded with a head of flowers.

Solanum lanciniatum, cut-leaved nightshade. *Koihohio* of the natives.—This species of Solanum is shrubby, attaining the elevation of six or seven feet, and bearing a small fruit of a red colour when mature, which is eaten by the natives. The taste of it, however, is insipid.

A species of the Palm tribe, (probably Seaforthia,) named *Enikau* by the natives.—I never saw this species growing to any elevation; it was observed in the woods, on elevated land. The natives use the leaves for thatching their huts.

Gualtheria antipoda. *Máá-kukáá* of the natives.—A small shrubby plant, found very abundant. "Stem shrubby, diffuse; leaves scattered, roundish, serrate-toothed." — *Forster's Prodromus*.

Passiflora tetrandra (Banks and Solander). *Po-hué-hué* of the natives.—This species of Passiflora is found in the New Zealand woods, and produces small orange-coloured fruit about May, containing numerous seeds of a beautiful crimson colour.

Two species of the Piper genus are found in the woods, a shrub, the *P. excelsum*, and a few small species, the *P. insipidum*.

Dracæna indivisa. *Ti* of the natives. This species of Dracæna differs from that found among the Polynesian Islands; it attains an elevation of ten or twelve feet, the summit terminating in a tuft of broad ensiform leaves, having no petioles, but are terminal, and half-clasping. The leaves form an excellent food as sea stock for cattle, &c. There is another species at New Zealand, the *D. Australis*.

Metrosideros florida. *Kahi-kahika* of the natives.—This species of Metrosideros I have found in the woods of New Zealand bushy, with opposite, ovate-oblong leaves, veined and glabrous, and attaching itself to other trees by offset roots, forming a bush around the tree.

Ceanothus Species. *Kumarahou* of the natives.—I collected numerous specimens of this plant in flower at Paihia, Bay of Islands, in July 1829.

Astelia Sp. *Kaha-kaha* of the natives.

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[*] If by accident an European, riding between the Taro patches, falls into one by his horse slipping, the mud stains the skin of a dark colour, and it is difficult to wash out. An analysis of the chemical properties of this mud would be interesting.

NOTE ON THE EXTERNAL USE OF THE COD-LIVER OIL, IN THE IMPETIGO SCABIDA, &c.

By Marshall Hall, M.D. F.R.S. &c.

To the Editor of the London Medical Gazette.

Sir,

If you think the following short account of a remedy for some very troublesome cutaneous affections worthy of a niche in the Gazette, it is much at your service.

The cod-liver oil is readily prepared at this very season: it is only necessary to place slices of the organ in a glass funnel; the oil flows, and may be received in a phial. The difference of specific gravity enables the preparer to separate the oil itself from foreign matters.

It is in some troublesome affections of the skin, especially of the hands, conjoining the characters of impetigo, with erysipelatous redness and swelling, and inducing the most severe suffering, that I have chiefly tried this remedy. After all others had been tried fruitlessly, it has speedily succeeded in restoring the cutaneous textures to a healthy condition.

For rhagades, or chaps, it is also a preventive and speedy cure.

I have also reason to know, that, in cases of eczema, and other diseases inducing excoriation and fissures of the skin, it is of great benefit.

My principal object in sending this paragraph to you, is to call the attention of your readers to a remedy hitherto little if at all known. The case which has brought the subject to my mind at this moment is that of a lady, whose hands had been literally crippled for several years, from painful, pustular, fissured, encrusted swelling of the fingers, palms, and back part (impetigo scabida). The case may be conceived from the mere fact of her having applied upwards of two hundred leeches. She experienced great advantage from the first application of the cod-liver oil, and she was speedily cured.

14, Manchester-Square,
Sept. 5, 1832.

P.S.—I have requested Mr. Howlett, of 4, Thager-Street, to prepare some of this oil.

OBSERVATIONS ON ANIMAL HEAT.

To the Editor of the London Medical Gazette.

Sir,

If you think the following of any worth, the author would be glad to see it inserted in your Gazette.

Your obedient servant,
A Student.

August 30th.

It is not a little remarkable that the important subject of animal heat should have received so little comparative attention, more particularly so, being a subject of personal interest, when considered in relation to that dreadful disease now traversing the empire, as well as an index of disease generally.

The striking analogy between respiration and combustion, in both of which oxygen gas is consumed, heat generated, and the substance under action materially changed in quality, first led the sagacious Dr. Black (to whom chemistry is so deeply indebted) to infer, that the production of warmth in the animal system depends solely upon chemical changes constantly going forward in the lungs, which changes, and the results as before observed, analogy favours as true.

The beautiful and ingenious theory of Crawford is built upon the same assumption, but enters more into detail; its consistency has likewise been admitted. He stated that animal heat primarily depends upon the condensation which takes place in the formation of carbonic acid gas and watery vapour during respiration; but since it is said, the lungs do not sensibly possess a higher temperature than other parts of the body, and we might conclude they would do, being in the immediate sphere of action, Crawford, in order to remedy this apparent defect in his theory, examined the capacities of venous and arterial blood for containing latent heat, and found the capacity of the latter to exceed that of the former in a ratio of 1030 to 892: this point ascertained, his inferences were, that the moment venous blood became arterialized, its capacity for specific caloric was increased; and that the heat rendered sensible by respiration, immediately became latent within it, reassuming its sensibility in the course of its circulation through the body, in proportion as this property became lessened.

This is the Crawfordian theory, based upon philosophical principles, and strongly aided by collateral evidence, which gained, and to a great extent still possesses, such notoriety and confidence, its extreme simplicity and apparent reasonableness preventing its validity being questioned by abstract reasonings; and it was at last only by a denial of one of its most important facts that an inquiry was awakened. Dr. Davy maintained, upon the authority of his own experiments, that the capacity for latent heat in venous and arterial blood differed so little, if at all, as to give no support to Crawford. If this assertion be true, the hypothesis is necessarily crushed; but the experiment against it, is one of the greatest nicety, extremely liable, in this case, to error: moreover, the supporters of Crawford say, does not the blood possess vitality? and who can say, no.

It is well known that venous blood, when brought into contact with oxygen gas, rapidly changes colour, and carbonic acid, heat, and watery vapour, are given off: moreover, these changes do not occur, in ordinary circumstances, when oxygen is excluded. We may, indeed, safely infer, that this conversion of colour and quality solely depends upon the presence of oxygen, which probably acts either by effecting some chemical change in the saline ingredients, or by relieving free carbon; but the latter supposition is generally admitted.

According to the estimation of Allen and Pepys, during the medium respiration of a healthy man, 26·6 cubic inches of carbonic acid are thrown off in a minute: this volume, when calculated upon, gives nearly 12 ounces of carbon, as the product emitted during 24 hours; a quantity seemingly at variance with our nature; but be it observed, unless a uniform supply of carbonic acid gas is made at every inspiration, there would be a lack of animal heat;—a portion of blood would circulate without being duly charged with latent caloric, according to Crawford's system. It is a leading feature of his theory, that heat rendered *latent* in arterial blood, becomes *gradually* sensible as it takes the venous form in the course of circulation: in agreement with this statement, its change in quality ought to agree in a ratio with the distance from the source of arterialization. Since, however, arterial blood taken from extreme vessels claims an identity with that just emerged from the lungs, we might reasonably infer that it would contain the same specific heat: moreover, with certain morbid signs, this theory will not bear a relative comparision. In inflammation or other affections of the lungs, where, from an increased vascularity of the membrane forming the air cells, respiration is carried on but imperfectly, and a quantity of venous blood is again thrown into the circulation, we might expect that the temperature of the body would sink to a degree directly as the quantity of venous blood thus morbidly circulated; that this, however, is not the case, is well known, but fever heat is the result, as in other inflammations.

These, and many other relative points which could be adduced as unfavourable to the theory, seem in a great degree to prove it erroneous, and at variance with the received doctrines of the profession, but they do not indisputably do so, still leaving a large field for argumental inquiry.

It is this circumstance which brings me to the decisive fact,—a fact, the simpleness of which when once observed has been the means of awakening an inquiry, and pointing out the inconsistency of Crawford's hypothesis of animal heat, when examined in conjunction with the principles of animal nature. That there is a great elevation of temperature caused by the condensation which takes place during the formation of carbonic acid gas, in ordinary cases, is proved by the unerring testimony of our senses; and that the same chemical action operates in the lungs during respiration, appears equally well founded. The practicability of heat, thus rendered sensible, becoming latent in arterial blood, has already been questioned.

The following observations, *if correct*, will place further debate on this point in the shade of uselessness:—I find by a set of simple experiments that the gaseous mass we throw off at each expiration, possesses a higher temperature than the body. From whence this accumulation of heat? Undoubtedly from the chemical changes constantly in operation in the lungs. It is familiar that air swiftly expelled from our lungs, or blown through small apertures, is colder than the surface of our bodies: this is occasioned by the compression which it is put under by the muscles of the mouth, and the rapid impulse by which its particles fall upon the surface; for it is equally familiar that, when expired without compression, an increase of heat is presently perceived. Bodies having a redundancy of heat, radiate with a rapidity proportioned to the temperature of other bodies by which they are surrounded. When we breathe upon the surface of our body it becomes warmer; this increase of warmth is not totally occasioned by the temperature of the air breathed upon it, but partially from an accumulation of its own heat, which it otherwise would have radiated, had it not been in contact with a body of a higher temperature than itself: hence the sensible warmth in this case is not to be attributed to one cause.

It appears doubtless, however, that the temperature of a part is materially influenced by the quantity of blood it contains, as we almost invariably observe in cases of great determination of blood to the head, or other part, that the heat is excessive there, while the lower extremities are cold; but if in these cases we consider a latent weakness to have existed previously in the part affected, we may refer the cause of this morbid accumulation to nervous reaction: hence the distention, and consequent excessive generation, of heat, are both sequents of one cause, viz. nervous irritation.

It is nutrition in its utmost comprehension, and of course entirely influenced by the nervous power, which supplies the heat of the animal body: the assimilation of matter constantly going forward in all parts, is attended with an increase of temperature, (as I believe has been before observed) and when we consider the large proportion of fluid in animals, and, consequently, the extensive field for chemical changes, there does not lack a source in the nutritive phenomena from which the warmth of the system may be kept up.

If the function of respiration is thus to be divested of one of its most important attributes, to what purpose must we refer its momentous action? It seems, from the intimate consent which exists between the action of the heart and breathing, that some very important change is to be effected in the blood before it circulates the body. Surely more than the mere evolution of carbonic acid, or change of colour, for probably the colouring matter is passive to animal life.

ANALYSES & NOTICES OF BOOKS.

'L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Dublin Journal of Medical and Chemical Science. No. IV.

THIS journal continues to be conducted in a very spirited style. The present number contains a variety of interesting papers, and among them we are glad to observe one by MR. CRAMPTON, *On the Treatment of Injuries of the Head.* We shall just barely notice this paper, and give an extract from it—all we have room to do this week. The author sets out with some excellent remarks on the utility of reporting cases in medical and surgical practice: this he calls the "autobiography of the healing art;" and adds, that the result of the publishing system carried on with this view in London and Paris, must be allowed to have diffused " not only a professional, but almost a personal acquaintance with the distinguished men whose modes of practice, and even whose modes of thinking and of speaking, they so graphically describe." He then alludes to the remarkable differences in surgical practice pursued in various places; and we must give what he says. With regard to the London and Dublin schools, " The chief difference," he observes, " appears to consist in this, that in fracture of the skull with depression of the bone, the trephine is less frequently employed in Dublin than in London. In Dublin we conform in general with the rule of practice as originally laid down by Mr. Dease (who preceded Desault by many years), namely, 'in fractures of the skull with depressed bone, *whether complicated with wound of the scalp or otherwise,* no attempt should be made to raise the depressed bone, *unless very decided symptoms be present of compressed or irritated brain.*' In London, on the contrary, the rule of practice seems to be, 'that if the depressed bone be exposed in consequence of a wound of the scalp, the trephine is to be applied to elevate the depressed bone, *whether symptoms of injury of the brain exist or not;*' either practice may be supported by the most specious arguments, and each has the sanction of the highest authority. It is plain, however, that in this case the question cannot be decided either by argument or authority, but solely by comparative experiments and observations conducted on a very extensive scale. I doubt, however, whether any individual, no matter how extensive his experience may have been, has instituted a sufficient number of experiments, or observed a sufficient number of facts, to establish definitively either of the opposite doctrines. In surgery, as in every thing else, extreme opinions, or opinions which are not qualified by exceptions and limitations, are seldom safe. I own, therefore, that the opinion of Mr. Dease, qualified as it is by the exception in respect to those cases in which there are symptoms of compressed or irritated brain, has ever exercised a strong influence over my mind, and I may add, over the minds of all those who, like myself, received their earliest surgical impressions from the lessons and practice of Mr. Dease. I am quite aware that early impressions in favour of any particular mode of practice, are extremely apt to influence our opinions in after life, and that 'to seek the testimonies of truth,' as Cicero beautifully expresses it, '*ex animis consuetudine imbutis,*' is neither philosophical or safe;—but, on the other hand, those who received their first impressions from the lessons and practice of Mr. Pott, are liable to a similar imputation of undue influence, operating in an opposite direction. We can only, therefore, approach to a solution of the problem, by placing in juxta-position the *practice* of those who in different countries cultivate the same field of inquiry on a great scale, although by different means."

Mr. Crampton's paper is not finished in the present number; but it breaks off with a case for which we must endeavour to make room.

Remarkable case of Gun-shot Wound in the Head.

"In the month of September, 1818, Mr. Henry Brougham (nephew of Lord Brougham), a fine, intelligent, and active youth, about eighteen years of age, received the contents of a fowling-piece, loaded with snipe-shot, in the centre of his forehead, by the following extraordinary accident. Holding his gun by the upper part of the barrel, he struck the stock obliquely downwards against the bar of a gate, which he wished to push open. The hammer was resting on the copper cap, which it compressed with the whole force of the spring; this pressure, aided by the vibration communicated by the stroke of the stock against the gate, was sufficient to make the detonating powder explode. From the oblique direction of the barrel upwards, the shot did not strike point blank against the forehead, but somewhat obliquely; it was, however, sufficiently direct to carry away an oblong piece of the integument of the forehead, nearly three inches in diameter—to fracture the os-frontis into small pieces—and to drive the greater number of them deep into the substance of the brain. Mr. B. was carried, in a state of insensibility, by some country people, to a cottage, who took some blood from his arm, but did not meddle with the wound. I saw Mr. B. on the following day; he was perfectly sensible; his pulse about 80, small and regular. * * * The head and face presented a frightful spectacle. The eyes were closed by the great swelling of the eyelids and cheeks. The skin of the face, which was perforated in many places by the shot, was covered with coagulated blood, rendered blacker by being mixed with gunpowder. The principal wound occupied the centre of the forehead, exactly where phrenologists place the organs of eventuality and comparison; here there was a wound of a circular form, above two inches in diameter, and nearly an inch in depth. The bottom of the wound, which rose and fell with every pulsation of the heart, was covered with a mixture of coagulated blood, disorganized brain, fragments of bone, and some of the wadding of the gun. Having cleared the face and head of the coagulated blood, in which they were in a manner incased, I proceeded to clear the wound of the extraneous matters which were lodged in it; but scarcely had I touched with the forceps a large fragment of bone which was buried in the brain, when the whole body was shaken by a convulsive movement, and he moaned deeply. Of course I desisted from all further attempts to extract the splinters, and determined merely to cleanse the wound with tepid water, of the wadding and gunpowder which lay on its surface; but scarcely had three drops of water fallen on the part, when he cried out in a voice more expressive of terror than of pain—"Oh, don't! Oh, what's that?" I asked him if I had hurt him. He said, "I do not know, but the sensation is dreadful." I now gave up all further attempts at even washing the wound, and merely covered it with a piece of soft lint. The rest of the treatment may be described in a few words. Leeches were applied in large numbers daily to the forehead and temples; iced water to the head, generally; attention was paid to the bowels; iced water and iced lemonade was the only sustenance which was allowed for six days; and at the expiration of this time he was conveyed (eight or nine miles) on a litter, from the miserable hovel in which he lay, to his mother's house in Sandymount. The local treatment was equally simple. The discharge from the wound was gently wiped away from the face as often as was necessary. This discharge contained for several days a large proportion of softened brain, mixed with blood; but the wound itself was not so much as washed for twenty-two days, and even then, nothing more was done than to lift off the small fragments of bone, as they became detached by the process of nature. This great wound was completely healed in two months, although small spiculæ of bone were discharged from time to time, for more than a year afterwards. The cicatrix when healed was deeply depressed, and pulsated like the fontanelle in the head of an infant. Mr. Brougham resumed his studies in Trinity College, and graduated at the end of the year. I met him by accident six or eight years afterwards, and recognized in my old patient one of the most amiable and valuable clergymen in Ireland."

A Lecture delivered over the Remains of Jeremy Bentham, Esq. in the Webb-Street School of Anatomy. By SOUTHWOOD SMITH, M.D. &c.

THE present production sets Dr. Smith before the public almost wholly in a new light—as an energetic expounder of the Benthamite or "felicitarian" philosophy. It would be rather beside our sphere, however, to analyse the curious moral and political doctrines with which about two-thirds of the lecture before us is filled; nor, indeed, would there be much novelty in setting forth the learned panegyrist's views of anatomy, with which the greater portion of the remainder is occupied; but we shall just extract one passage, in which Dr. Smith gives the reasons which induced the eminent deceased to bequeath his body for dissection. It ought to be known that the will in which Bentham first formally made this bequest, is dated so far back as 1769, when he left his body to his friend, Dr. Fordyce: only two months before his death, he revised the document, and solemnly confirmed that part of it which contained the act in question. "That the disposal of his body, which has brought it to this place, and which has called us together to express our admiration of the act and our gratitude for it, would give pain to some for whom he entertained a sincere affection, he knew; but he also knew that the amount of pain thus produced would be overbalanced by the good likely to result from such an example. He had a great regard for the science of medicine. How could it be otherwise with one whose thoughts were so constantly employed on the promotion of human happiness and the mitigation of human suffering? He knew that the basis of medicine is anatomy, and that the only means of acquiring a knowledge of anatomy is through dissection. He had an utter contempt of the prejudices which withhold the means of pursuing dissection. He was satisfied that there is but one way of putting those prejudices down—and that is, that those who are above them should prove it by giving their own bodies for dissection. He therefore determined to set the example. He was aware of the difficulties that might obstruct his purpose—he provided against them. He chose three friends to whom he was tenderly attached, and on whose firmness he thought he might rely. He prepared them for opposition and even for obloquy. He asked them whether their affection for him would enable them to brave whatever portion of either, or of both, might fall to their share in carrying his wish into effect? They assured him that neither opposition nor obloquy should deter them from performing what he required to the letter. 'Then,' said he, 'I charge you, by your affection for me, to be faithful to this pledge.' They have been faithful to it."

The personal traits of Bentham's character, which this pamphlet contains are vividly drawn; and the popular exposition which it gives of the utilitarian principles, cannot but render it highly acceptable to the public.

TREATMENT OF CHOLERA.

SULPHATE OF COPPER IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

BELIEVING that the following facts may lead to a more successful mode of treating the prevailing epidemic, I request that you will allow them to be made known to the public through the medium of your very useful publication.

On the morning of the 17th August, I visited a young woman, a dress-maker, 20 years of age. She had been under medical treatment all night for a severe attack of cholera. All the symptoms were present, and strongly marked. I had contemplated trying the sulphate of copper, in a few doses, in the first case I met with, having seen all the modes of treatment recommended fail. I ordered a scruple in an ounce of water to be given. The gentleman who was in attendance reported to me that it remained in the stomach a quarter of an hour, a strong proof of the diminished excitability of that organ. When the vomiting ceased, (which was occasioned by the sulphate) the skin became warmer, the pulse more distinct, and the severity of the spasms very much mitigated. The stomach was now quiet. Two grains of calomel, and one tablespoonful of beef-tea, were given every half hour; at night three drops of croton oil; soon after which the bowels began to act, exhibiting some improvement in the secretions. This patient very slowly recovered, but the urgency of the symptoms soon subsided after the vomiting produced by the sulphate of copper.

A. Cook, 30 years of age. Aug. 19.—In this case, likewise, all the symptoms were present, and very severe. The same treatment was pursued as with the former patient, except that she had neither sedative nor stimulant, both of which had been administered to the former in the commencement of the treatment. This patient was able to come down stairs in a week.

Alderidge, 68 years of age, a cooper, Aug. 26.—In consequence of a complete misrepresentation of his case, was ordered calomel, eight grains, with one and a half grains of opium. In an hour and a half after the spasms became so severe that I was sent for. I found it a case of well-marked cholera, *without vomiting*. As his stomach was quiet, I ordered a purgative draught, and desired they would wait the result. I saw him four hours after: the spasms very troublesome, rice-coloured stools, livid hands and face, &c. I gave him the sulphate, which vomited him in two minutes; soon after which the spasms ceased; indeed, he complained but once after. The calomel and croton oil were given as before, and he appeared to be going on well when I saw him last at 10 P.M. On the 27th he got out of bed at 3 o'clock, fancying that he wanted to make water. The attendant with difficulty got him into bed again. He died at half-past 5.

On examination of the body, the lungs were found to have suffered extensively from frequent attacks of inflammation. The abdominal viscera presented the appearances usual in cholera.

King, about 25 years of age, Sept. 1st: was found in a state of complete collapse by my assistant, Mr. Green, a very active and zealous young man. He administered to him the sulphate, which produced an effect like magic: this was followed by calomel and croton oil. At 7 o'clock, the family where he lodged thought him so well that they might get rid of him; they therefore sent him to the infirmary in Mount-Street, Grosvenor-Square. Mr. Green called there to report what he had done. There he was put under their own system, which, after a little time, was given up for one which succeeded in restoring him. This, I understand, was small doses of sulphate of copper and hydrocyanic acid.

It may be asked why I adopted the above treatment. My answer is, I saw that no reliance was to be placed on any plan of treatment hitherto adopted. I had often witnessed the prompt advantages gained by a brisk emetic in severe attacks of remittent fever, where the stomach had been so irritable as to reject every thing. I determined to try the effect of an emetic in this devastating disease. Past experience pointed out to me the sulphate of copper as a powerful emetic, and one, from its astringency, likely to restrain, in a great degree, the effusion, for secretion it cannot be called, into the stomach of the peculiar fluid which forms one of the characteristics of the disease. There are other advantages, which it is not necessary to enter upon. I hope I have said enough to induce others to try my mode of treating this enemy to the human race. I understand they are using the sulphate of copper now in the Mount Street Infirmary, but not in the same way.—I am, sir, yours,

JAMES MORRAH, M.R.C.S.

Sloane-Street, 10th Sept. 1832.

[The papers which follow are transmitted by the Central Board of Health.]

Large doses of opium before collapse—opinion of various remedies after collapse has come on —fickleness in the effects of remedies.

THE Central Board of Health have circulated a paper (No. 2,) in which they request those medical gentlemen who have had experience in the treatment of cholera, and are of opinion that they have been successful in their practice, to forward to the Secretary of their Board a short account of their respective methods of treatment of the epidemic.

"1st, When in the form of bilious diarrhœa.

"2nd, In that of rice-water evacuations.

"3d, In the stage of collapse."

I am not of the number of those who "are of opinion that they have been successful in their practice;" nevertheless, having been otherwise called upon, I proceed to present a very brief statement in reply.

In the hospital wherein I reside as medical officer, there has hardly been an opportunity of treating the disease in the two first forms, viz. in bilious diarrhœa, and in rice-water evacuations; but from a pretty ample observation of those states out of doors, as well as in my own treatment, as in that of competent medical friends, I have arrived at an opinion, which is somewhat at variance with that of many most able practitioners. This opinion is, that, at any reasonable period before the stage of collapse — even though there be rice-water purging — even though there be some vomiting—and although cramps may have come on—the whole disease may be stopped quickly, *safely*, and, with human certainty, by full and energetic doses of opium,—doses proportioned to the age, idiosyncrasy, and condition of the individual; but having for their object the principle of stopping all the discharges at once. Small doses do not avail; for, it appears to me, that some secretions are more easily stopped than others; that the hepatic, for example, is arrested by a smaller opiate than that from the intestines; consequently, if we give small opiates, the bile is arrested, whilst the intestinal excretion goes on, and the patient thus hastens on to collapse; but if a sufficient dose be given to stop every secretion, there has not, so far as I have witnessed, been any but the happiest result from it.

There is no advantage (the secretions being thus arrested) in being too hasty to act on the bowels by purgatives; they generally act spontaneously in a day or two, and the whole of the secretions are restored, without any active interference on the part of medicine.

If, however, they do not act, and circumstances seem to require it, some mild, warm laxative may be given,—an undue operation from which is to be guarded against by a small opiate.

This treatment does not apply to some other premonitory symptoms that more rarely arise; nor to those cases which, without previous diarrhœa, go rapidly into collapse. Plethoric and robust individuals will not bear so much opium as thin persons.

In the stage of *confirmed* collapse, I have seen no treatment so efficacious as to deserve especial praise; but some remedies have permitted patients to die more rapidly than others.

1st, The "Saline treatment" has increased involuntary purging, and done harm; or the powders have been immediately vomited, and have done no good.

2d, Excessive opiates have quieted the sufferings, but have appeared to hasten the deaths of those subjected to them.

3d, Spirituous stimulants, *in excess*, have oppressed whatever power remained, and have been of no avail: in moderation, they are not injurious.

4th, Those who have recovered from collapse—a few—have done so under frequent doses of calomel, with or without *minute* doses of opium; according to the degree of purging existing, frequent doses of ammonia, sinapisms, and a moderate portion of brandy and water, gin, or portwine, and the like.

This treatment, it will be perceived, is founded on the broad principle, that, in the cure of the malady, nature is the principal, and the physician her auxiliary; and is directly contraposed to that which seems to assume, that nature is the auxiliary, and that a very unimportant one to medicine.

A too meddlesome practice has been always fatal.

5th, Cold water, toast water, barley water, balm tea, are freely allowed to those who desire to drink: its rejection being no objection.

The consecutive fever bears no relation to the quantity of stimulants or opiates administered, some of the most severe cases of this fever having occurred in persons who had not taken a particle of either brandy or opium.

It appears to me that the spasmodic cholera assumes different types in different localities and at different periods: thus, the same remedy that is useful in St. Giles's shall fail in Whitechapel, and that which may have been beneficial in Clerkenwell shall prove injurious in Newgate; nay, the very remedy that was useful in a place six months ago, shall be useless in the same spot to day. In this, I think the analogy of puerperal fever may with benefit be consulted.

I am acquainted with an extensive parish, within the Bills of Mortality, whose surgeon has adopted one, little varied plan of treatment in cholera, from the beginning of this epidemic: for a few weeks nearly all his cases recover; and then, again, without any alteration of system, he finds that nearly all his cases for the next few weeks die: this has occurred more than once, without his being able to refer it to any tangible cause.

Not having the opportunity of communicating any important or useful fact to the Board, I have abstained from occupying their time at greater length, and have accordingly endeavoured to make my remarks as concise as possible.

ALEX. TWEEDIE.

September 12.

EFFICACY OF TARTAR EMETIC, &c.

To the Secretary of the Central Board of Health.

Droitwich Asylum, Sept. 14, 1832.

SIR,

ONLY two have escaped, and these I hope are impregnable, as they have been in the midst of infection hitherto without suffering. During the last five days (viz. on the 9th) we have had only one new case, and that, though a bad one, I am happy to say is convalescent. Those persons in constant attendance have all been attacked; those only occasionally in attendance, at the commencement of the disease, have escaped.

The progress and consequences of the disease, from its commencement (the 22d of August last), are as follow:

	Cases.	Deaths.
Between the 22d and 25th August	6	6
On the 26th (day of my arrival)	2	2
From 27th (3 A.M.) to the 13th September instant	45	13
	53	21

The treatment from 22d to 25th August inclusive, cal. and op. and creta mixture. The two cases on the 26th, being each between 60 and 70, incurable lunatics, were treated with opium, brandy, and tinct. capsici; and by this, one attacked the next day, æt. 70, was saved, and is still well. In many cases it was impossible to give solid medicine, and in three every thing offered was refused. From the 27th August to the 30th inclusive, the treatment was the same, viz. a large dose of calomel, with gr. j. opii, followed by small doses, with capsicum and camphor at short intervals, using, of course, the hot air bath, enemas, turpentine fomentations, sinapisms, &c. when necessary. Dissatisfied with the result of this treatment, particularly where vomiting was severe, I was anxious to attempt some plan which might promise more favourable results. I had frequently observed in the bad cases the utter impossibility of checking the sickness by any remedy usually employed. I had tried over and over again, and as often failed in administering it, the mustard emetic. I determined, therefore, no longer to attempt to check the sickness by the usual means. I accordingly in all cases, from the 31st of August inclusive, in which vomiting was extreme, gave ant. tart. gr. iij., and after the stomach became, as was the case, rather more quiet, I gave every hour a pill containing ant. tart. gr. iij. op. gr. j. In no cases thus treated, from the commencement, did the sickness last beyond four doses. Very often both vomiting and purging ceased; but where the last continued, I gave rhubarb and magnes. c. tinct. op. ʒss. followed by cal. and capsicum, c. op. ¼ gr. 3tiis horis, with a mixture of catechu, opium, and carbonate of soda. In one case, which happened on the 9th (the last reported), finding the purging and vomiting did not so readily yield, I gave at one dose ant. tart. gr. vj., opii gr. ij. This person was a woman of the name of Ball, a violent lunatic; it was therefore necessary to give it in coffee. After this the vomiting ceased, but the purging (the true rice-water dejection) continued, the pulse being still imperceptible, or at all events so feeble as to induce a doubt of its presence. I now gave ʒj. of tinct. of op. also in coffee, and repeated ʒss. in an hour afterwards. All the bad symptoms left, and she is now convalescent without other treatment.

The relative effects of the different modes of treatment to which I have alluded, will be seen by the following statement of cases and results:—

	Cases.	Deaths.
From the 22d to the 30th August, inclusive	26	17
From the 31st August to 9th September	21	4

Of these, two were violent lunatics, obstinately refusing medicine.

If you think the above worth publicity, and that it really possesses novelty, perhaps you will mention it to the Board; if, on the contrary, you judge it differently, silence upon the subject will oblige, sir,

Your obedient servant,

J. B. STEWARD.

UNION OF SALINES, CALOMEL, AND COLD WATER.

THE treatment which has been found most successful in the Lancashire Lunatic Asylum, unites the advantages of the saline method of Dr. Stevens, the cold water system of Dr. Shute, and the calomel treatment of our Indian practitioners.

When the patient is first seized, two table spoonsful of common salt are immediately administered, in about a tumbler of warm water. After the operation of the salt, two-grain calomel pills, with the saline powders of Stevens, are given every hour, or every half hour in very bad cases. An enema of sixty drops of tinctura opii, in a pint of gruel or warm water, is immediately thrown into the rectum; cold water is drank *ad libitum;* camphorated mercurial frictions are applied to the regions of the liver and stomach; and warm frictions are used assiduously to the extremities.

Brandy, opium by the mouth, and the mustard emetics, have been found to be productive of much mischief.

When the epigastrium is very tender, we apply leeches, with evident relief; but not otherwise.

Bleeding has been had recourse to, when the pulse could bear it, and when congestion of the brain was present, with the most marked benefit.

Of the cold water practice of Shute we cannot speak too favourably—the patients deriving the most comfortable sensations from its use, and frequently calling for water when they expressed the utmost aversion to all other fluids. In no case have we observed the slightest bad effects to ensue from its exhibition.

We have tried the croton oil practice, as recommended by Dr. Teggart, but hitherto we have not had sufficient experience of its effects, to enable us to speak in any decisive terms of its efficiency.

To the convalescents we administer small and repeated doses of castor oil, and allow mild, digestive, farinaceous diet, with marked advantage.

Almost every species of fumigation, that has promised any advantage, has been had recouse to; but nothing appears to have produced such efficient and rapid effects, as the fumes arising from the explosion of trains of the coarsest gunpowder.

It is worthy of observation, that the cholera, from its commencement on the 5th of September, up to the present period (the 17th), has been confined exclusively to the women, who amounted on the first irruption of the disease to 188, of whom 72 have been seized, without any distinction of age, bodily strength, or infirmities; the epileptic, fatuous, and furious maniac, not being more exposed to its ravages than the healthy, tractable, and cleanly monomaniac. The disease has pervaded every part of the female division of the hospital.

We have every reason to believe that the cholera originated in a female patient, brought from the Manchester

Workhouse, who was attacked on the night of her admission with diarrhœa and partial collapse, had fully developed symptoms of cholera on the third day, and fell a victim to the disease on the 11th day after her admission.

LAWSON WHALLEY, M.D.
Physician to the Asylum.
EDWD. D. DE VITRE, M.D.
WM. DAVIDSON, M.R.C.S.E.D.
Surgeon to the Asylum.

Lancaster County Lunatic Asylum,
Sept. 17, 1832.

P.S.—It is requested that the above communication may be forwarded to the Medical Gazette for insertion.

TARTAR EMETIC IN CHOLERA.

MR. LANGFORD, resident medical officer at one of the Cholera Hospitals in Manchester, thus writes:—

"I am aware the practice, as applied to cholera, is quite novel in this country, but its success has induced me not to delay, for a longer experience, this communication.

"The following is the mode of application:—Dissolve 10 grains of emetic tartar in 7½ ounces of water, with half an ounce of rectified spirits of wine; of which give half an ounce every two hours, until the biliary and urinary secretions are properly restored, and then gradually diminish its frequency. Allow toast and water, or whey, *ad libitum*; prohibit all heat and frictions, and give no other remedy. A *tolerance* of the medicines is in a short time established." And again:

"I continue to entertain the same favourable opinion of the successful administration of emetic tartar for the removal of spasmodic cholera. In the advanced stage of the disease, it will be found necessary in the first instance to increase the dose, to produce the action of vomiting, as well as in younger patients, to diminish it; and if this remedy is not successful, in the most malignant and fatal stage of spasmodic cholera, when the nervous energy is subdued, and the powers of life laid prostrate, it is not wonderful.

"Sept. 5, 1832."

DR. SHUTE ON COLD WATER IN CHOLERA.

DR. HARDWICKE SHUTE has transmitted to the Board of Health a third paper on the above subject, and has politely directed that it should be forwarded to us. It appears that the Doctor, owing to the favourable state of the public health in Gloucester, has had no additional experience on the subject; but, as the paper is farther explanatory of his views, we shall insert it next week.

DR. PIDDUCK ON SALT AS AN EMETIC.

THIS communication, addressed to the Central Board, contains statements and opinions so similar to those contained in the paper lately published by us, that we deem it unnecessary to do more than refer to the one already before our readers.

DR. WARD ON CROTON OIL, &c.

A PAPER also lies before us sent by our respected correspondent Dr. Ward, of Wolverhampton; which, however, we received too late to insert this week. The writer speaks very favourably of Croton oil, especially in young subjects, "of 15 cases of children," says the Doctor, "attacked with cholera in every stage, from the age of 5 months to 12 years, whom I have treated, with the oil and with that alone, I have lost but one, a boy of eight died comatose of secondary fever." The Doctor also uses the salines of Dr. Stevens to the exclusion, if possible, of any other kind of drink.

MR. BUSOINES ON CHOLERA.

THIS gentleman states that he has had fourteen cases in which there were rice-water evacuations and collapse: the first six, being treated with calomel, opium, and stimulants, all died. The other eight had calomel (quantity not mentioned) every hour, a wine-glassful of a saline mixture every half hour, and "as much cold water as they could possibly drink—in some instances a gallon every two hours." Six recovered, and two died under this plan of treatment. Want of space, however, prevents us from giving minuter details.

MEDICAL GAZETTE.

Saturday, September 22, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

SELF-SUPPORTING DISPENSARIES.

OUR remarks of last week on the condition of the poorer classes in various parts of the country with regard to medical assistance, were scarcely penned, when some papers reached us from Southam, in Warwickshire: they proved to be highly interesting. A society, it appears, has been formed in that town, with the provident design of alleviating the distresses which may possibly attend a visitation of Cholera in the neighbourhood, and this is to be effected by a fund collected for a three-fold purpose, — maintenance in sickness, funeral expenses, and a gratuity to be allowed to bereaved survivors. Their little regulations are so purely benevolent, and so sensibly devised, that it may gratify many of our readers to have an abstract of them :—

1. Each head of a family subscribing any sum from three pence to five shillings weekly, according to his ability, shall be allowed twelve times the amount of such subscription daily, from the time any of his family shall be attacked by Asiatic cholera, up to the time of their convalescence or death, and shall have the benefit of a medical attendant.

2. Of each parent who shall have subscribed any sum from three pence to one shilling weekly, the sum of three pounds shall be allowed to the family, in case of the death of either parent; and for each parent who shall have subscribed any sum above one shilling weekly, the sum of five pounds shall be allowed to the family in case of the death of either parent.

3. For each servant or child of a subscriber's family who shall die of cholera, the sum of two pounds shall in all cases be allowed, without any reference whatever to the amount of subscription.

4. The members shall agree to continue their subscription to the fund till the treasurer be reimbursed the payments he may be called upon to make beyond the sum he has in hand, and till such time as the cholera shall cease for twenty miles round Southam; on which happy occasion, or on any day of thanksgiving to Almighty God as shall be appointed by his Majesty in Council, it is proposed to divide the fund between the widows and children in such manner as a general meeting of the members, called for that especial purpose, shall direct: the widows to have their dividend at the time, and the children to have theirs deposited in the Savings-Bank, and allowed to accumulate till they shall respectively attain the age of fourteen years.

It is understood by every member signing these resolutions, that he is bound in honour not to retire from the Association until the same be dissolved.

These resolutions were unanimously agreed to at a large meeting of the inhabitants held in the course of the last month.

That such an admirable plan has been proposed and adopted at Southam, no person who is acquainted with the intelligence and moral worth of the people of that town will hear with surprise. It was in the same place, as most of our readers may recollect, that some ten years ago the first self-supporting Dispensary that this country could boast of was established; and since then, we find that the example has been followed with the happiest results, in no less than sixteen different quarters, — in Coventry, Birmingham, Walsall, Burton on Trent, Rugby, Derby, Lymington, Willesbourne, &c. The principle upon which these establishments are founded cannot be too universally made known, nor can it, perhaps, be better understood than by repeating the words of the late Dr. Gooch, originally published in this journal, and subsequently copied by Mr. Southey into the first volume of his Colloquies :—" In the year 1823, some opulent and benevolent persons held a meeting at Southam, a small county town of Warwickshire, near Stratford upon Avon, when Mr. Smith, a surgeon of the town, proposed the establishment of a Dispensary for the sick poor of that neighbourhood. The funds for the support of this Dispensary were to be drawn from three sources,—parochial contributions, the subscriptions of opulent persons, and, lastly, voluntary subscriptions from the poor themselves. There would be two classes of patients; those who would have a claim on the dispensary from the subscriptions of their parish, and those whose claim would depend on their own subscriptions. The latter were to have the encouraging appellation of the 'Independent poor': their annual subscription was to be 3s. 6d. for an adult, and 2s. for a child; and this would give them a right to medical attendance whenever they required it. The medical attendants on this Dispensary were to be all the respectable surgeons of the neighbourhood, who were to take care of the sick poor of Southam and the surrounding county within six miles; and for this, the income of the Dispensary, after paying its expenses, was to be divided among the surgeons, according to the number of miles travelled and visits made. Attendance was to be given at the Dispensary one hour every day (excepting Sunday), when those patients who were able to go received advice; those who were too ill to go out, were to be visited at their own houses. As they were no longer dependent on one surgeon, they were not likely to be neglected, for it was not probable that all would be busy, or out of the way at the same time. The Southam Dispensary has now (1827) been in operation four years, and the result appears to be highly satisfactory to the members. It has been wholly maintained by parish subscriptions for the pauper patients, and by the voluntary subscriptions of the independent poor, *without any assistance hitherto from opulent persons**. At a vestry meeting at Southam, in March of this year, it was resolved that of two hundred persons among the poor subscribers to the Southam Dispensary, *one half would have been on the parish if it had not been for this institution*; and they strongly recommend the formation of similar dispensaries all over the kingdom."

And so most heartily do we. It is in this, we take it, that the great benefit of such establishments consists—that, while they provide for the physical necessities of the poor subscribers, they provide also for their moral wants—by imparting to them a prospective feeling, and rendering them *self-provident* beings. How powerfully the existence of such a feeling in his poor patients—"hope springing eternal" in their breasts—conduces to the success of medical agency, every practitioner can testify—how it facilitates recovery, or soothes the sufferings of the bed of sickness. And which of our politicians is ignorant that the condition of the people can never be securely provided for, without due attention being paid both to their physical and moral necessities?—that the health of the body politic depends mainly on the body natural? We could run on with a series of suggestions on this topic, were it our cue to meddle with the councils of the politicians; but we shall only venture to allude to one more advantage derivable from the self-supporting dispensary system—and that because it concerns both government and people, and tends to remove a weighty responsibility from the one party, and a heavy burthen from the other;—we mean the economical improvement most certainly to be expected with regard to the poor-rates, from encouraging these provident little institutions. Our poor-laws—at once the glory and the blot of our political system—have, it is to be feared, in but too many instances, tended to denationalize our artizans and labourers, who are often not ashamed to assert that they have found in beggary "a better trade than the spade and the workshop." The blame, we are inclined to think, attaches in a great degree to those who, having the distribution of charitable funds at their disposal, are too indolent, too ignorant, or perhaps too reckless, to discriminate the wisest mode of discharging their duty.

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* We trust that this can no longer be said: we are almost sure it cannot.

Another argument in favour of these dispensaries is, that they tend to place the services of medical men in something like a proper point of view. The patients are taught to consider that, as well in their own case as in that of their attendants, "the labourer is worthy of his hire;" and they find themselves, we fancy, treated in a very different way from that which they experienced under the old system of being farmed out by the parish, at from *thirty to fifty shillings a hundred*, by the year. The medical men, it is true, are by no means adequately remunerated for their services; but the proper principle is kept up, which ought to be observed in every such case, for the mutual benefit of all parties.

The accounts from Coventry, where a self-supporting dispensary has been established within the last year, are very gratifying. During nine months, 1505 patents were admitted, 1189 were cured, 101 relieved, 186 remained under treatment, and but 19 died: there were 10 midwifery cases. The subscriptions amounted to 126*l.*; of which about 46*l.* were expended on medicine, and the remainder (80*l.*) fell to the surgeons! From Birmingham, we learn that there has been "not only a great increase in the total number of patients, but a more than proportionate increase in the independent class."

We have read Mr. Smith's "Address" with considerable pleasure, and give him great credit for his kind and humane intentions.

SCIENCE IN EGYPT.

The Pacha of Egypt has made great exertions to introduce the most important improvements in science generally, and in medicine in particular, into his dominions. For this purpose he some years ago induced M. Clot, a French surgeon, to settle in Egypt, and patronized the establishment of a Medical School at Abouzabel, to the proceedings of which we have repeatedly alluded. The Pacha has recently created his French *protégé* a Bey, so that he now figures under the somewhat ludicrous appellation of *Clot-Bey;* and his patron is about to dispatch him with 12 young Egyptians to Paris, where they are to be educated in the most complete manner, and on their return to be appointed Professors of the different branches of knowledge they have acquired. Who knows but it may be destined that science shall again, as in times of old, find a resting place in Egypt.

CHOLERA MEDALS.

The French government has distributed a number of medals to those among the inhabitants of Paris who were most conspicuous in their philanthropic exertions during the late epidemic.

MORTALITY FROM CHOLERA IN PARIS.

From the 26th of March (when cholera broke out in Paris) to August 31st inclusive, the number of deaths *reported* were 17,978. April and July were the most fatal months: the former gives 12,723, and the latter 2577 deaths, the mortality of the other three months taken together being but 2678.

CONTROVERSY REGARDING THE STRUCTURE OF THE PLACENTA.

As the following letters have never before been published, we think they may be interesting to our readers at the present time, when the attention of anatomists has again been directed, by a paper of Dr. Robert Lee, in the "Philosophical Transactions," to the relation subsisting between the mother and fœtus during gestation. The subject is one of peculiar interest and difficulty, and we trust that it will now be investigated in such a manner as to leave no ground for future controversy; in other words, that the doctrine of the Hunters will be either established or refuted. It appears to us that the most important points to be determined are the following, and we do not think they ever can be satisfactorily ascertained but by examining the parts in their natural situation, where they have not been disturbed by throwing forcibly foreign matters into the uterine vessels.

Do the uterine sinuses terminate in large, smooth, simicircular orifices in the inner membrane of the uterus, as represented by Dr. Lee? Are there openings in the decidua covering the placenta, corresponding with those in the inner membrane of the uterus? And, lastly, is it possible to demonstrate the existence of cells in the placenta without the employment of injections?

The best mode of investigating these subjects seems to be that first recommended by Mr Owen—viz. dissecting them under water; and we would recommend this mode, instead of forcing injections into the uterine vessels, to those who may enjoy the opportunity of examining the gravid uterus in the latter months. We would also recommend them to verify the statement of Dr. Lee, as to air not passing from the uterine sinuses into any part of the placenta; for it is hardly conceivable that air should not pass freely into the cells of the placenta, if the uterine sinuses were continued into these cells, as described by Professor Burns in his last paper.

Original Correspondence between John and William Hunter.

Windmill-Street, Feb. 3, 1780.

Dr. Hunter begs the favour, that the Secretary of the Royal Society will read to the Society what follows:—

Mr. Hunter's account of the structure of the human placenta, explaining the connexion and circulation between the mother and the *fœtus* in *utero*, which was read at the last meeting of the Royal Society, informs us that it was a discovery which he made with Dr. Mackenzie, and that it was not claimed by me.

The Society will be sensible that I am reduced to the necessity of taking notice of this mistake, when they are informed of the following facts:—

First. That the doctrine has been many years ago published in printed books as my discovery, and had been communicated as such by myself. See Baron Haller, for instance, in the second part of the eighth vol. (p. 220) of his great Physiology, in quarto, printed thirteen or fourteen years ago.

Secondly. Besides treating of it as my own discovery, in my lectures upon the subject, I have always done so for many years last past, in the very first lecture of my course, which is the most public of all, because the door is then open to every person whose curiosity prompts him to be present.

In the third place, occasionally both in what I have printed, and in my lectures, I hope I have not overlooked opportunities of doing justice to Mr. Hunter's great merits, and of acknowledging that he had been an excellent assistant to me in this and in many other pursuits. By doing so I always felt an inward gratification, shall I call it, or pride? I had given him all the little anatomical knowledge which I could communicate; and put him into the very best situation that I could for becoming what this Society has for some time known him to be.

May it be presumed, then, that I stand possessed of the discovery in question till proofs shall be brought to dispossess me?

I shall most willingly submit to the pleasure of the Society. If they signify an unwillingness that this emulation (I will call it) should go on, I shall acquiesce, and be silent. If curiosity, justice, or the laws and practice of the Society, should incline the Council to seek out and determine upon the merits, I shall be equally ready to obey their commands.

And, if it should appear reasonable to them, I would first beg to know the grounds of Mr. Hunter's claim, as I am too well acquainted with his abilities not to think that he must be able to support his claim by something that I am ignorant of. And if I should receive that satisfaction, I shall immediately shew that I am more tenacious of truth than even of anatomical discoveries. But if that information should not alter my thoughts upon the question, I shall shew to the satisfaction of the Society, if I can at all judge of my own employments and pursuits, that my pretensions arise out of a long series of observations and experiments made with a view to the discovery in question: that it was not a random conjecture, a lucky thought, or accidental occasion, but a persevering pursuit for twelve or thirteen years at least, the progress of which was always publicly known here, and admits of the most circumstantial proof.

WILLIAM HUNTER.

To the President of the Royal Society.

February 17, 1780.

SIR,

Though I know the constitution of the Society over which you preside too well to suppose that they will give their judgment on any subject, and respect it too much to think it a proper field for waging the war of controversy, I cannot avoid requesting you to lay before that learned body a short answer to the paper given in by my brother, Dr. Hunter, as silence on my part after his charge may be interpreted by my enemies into an acknowledgment that I have intentionally claimed to myself a discovery in reality his due. I am as tenacious as he is of anatomical discovery; and I flatter myself as tenacious also of truth.

The discovery was made in the manner in which I stated it in my paper. Dr. Mackenzie had injected the subject, and being unable, as I conceived, to explain an appearance which he had found in dissecting it, sent for me. I came to him; and having examined it further, explained the appearance in question, then, for the first time, to my own satisfaction and that of Dr. Mackenzie; and the evening of the same day, full of the discovery, I came to Dr. Hunter, and brought him with me to Dr. Mackenzie, to see and judge of the explanation which I had given, and Dr. Mackenzie had agreed to.

This is my state of the fact upon which I ground my belief of myself being the author of this anatomical discovery; but as my brother thinks differently, after a period of twenty-five years, I am content to abolish all remembrance of the successions of time in the course of that day, and to suppose that Dr. Mackenzie, Dr. Hunter, and myself, inspected the parts together, and together made the discovery: by which means the honour of it will be divided into three; one of which I may surely be allowed to take to myself, the other two may appertain to Dr. Mackenzie and Dr. Hunter, if they choose to claim and be content with them; though, in this division, we must make some reserve for the claim of several ingenious young men, at that time pupils, who were with us, and of course entitled to some proportional share in the discovery, though their present situations, settled at a distance from this town, have prevented them from getting early notice of the present claim, and of course from making application to the Society for their share.

However, I may here declare, that if Dr. Hunter will produce to me any claim which I can allow of his having discovered this anatomical fact at any period of time prior to this conference at Dr. Mackenzie's, I shall first declare, in excuse for having troubled the Society, that I was not before acquainted with it, and immediately after declare that he is entitled to the sole honour of it, at least in preference to myself.

I am, sir,
Your much obliged, and most
obedient humble servant,
JOHN HUNTER.

Jermyn-Street, Feb. 17, 1780.

THE SURGICAL PROFESSION IN IRELAND.

COMPARATIVE MERITS OF THE LONDON AND DUBLIN COLLEGES.

To the Editor of the London Medical Gazette.

SIR,

THE observations, on the Irish Grand-Jury Bill, contained in your number of the 18th August, have appeared to me to give rather an *ex-parte* view of the matter in dispute between the members of the London College of Surgeons practising in Ireland, and the licentiates, or members of the Dublin College; or rather, you have omitted to state fully the reason for the adoption of the *exclusive* clause.

Every one knows that the surgical profession in England and Ireland are upon a totally different footing. Till lately it was a fundamental article of the charter of the Irish College, that no member or licentiate of that body should dispense medicines: hence the almost total absence of a grade of the profession in Ireland corresponding to the

English general practitioner. In this country, on the other hand, the apothecary is all in all: possessed of the licence of the Hall, he is fully qualified to practise: he needs not the diploma of the College: nay, he has just as much right and title to practise medicine, surgery, and midwifery, without that diploma as with it. Hence the College, or more properly speaking, the Court of Examiners at the College of Surgeons, do not lean very heavily on the candidates that come before them. It is certain that they do not keep them in for any long time—fifteen minutes generally—sometimes perhaps twenty—nay, they may even go so far as the half hour—and I have lately been surprised to hear that some have been kept in a whole hour! Be this as it may, the fact is notorious that the students in general do not hold the examination at Lincoln's-Inn-Fields in much respect; and they smile if you compare that examination with the ordeal through which the worshipful Company of Apothecaries require them to pass. The truth is, that if the College required a considerable acquaintance with anatomy and physiology—did their examiners give a scrutinizing and practical examination in surgery, and require from the candidate an accurate knowledge of materia medica, and other collateral branches, the members' list would be reduced considerably—there would be a lamentable deficit in the examiners' fees, and the sphere of collegiate influence would be much contracted.

But I find, upon looking over my last paragraph, that I have made a statement not altogether correct: I have there stated that there is no grade of the profession in Ireland corresponding to the English general practitioner. There is, sir, as you are aware, another class, consisting of individuals who have obtained the license of the Irish Apothecaries' Company in the first instance. Now this license, or diploma, has hitherto in no way contributed to the respectability of its possessor. Obtained generally after a trivial and rather *pro formâ* examination, it afforded but little security to the public against ignorant practitioners. But this is not all: as soon as they have obtained the approbation of the apothecaries, they come over to London, and seldom fail to return dubbed M.R.C.S.L. a distinction which they take care to append to their names on all occasions, and which, doubtless, does not a little astonish the natives of some of the less civilized parts of the emerald isle. And these are the individuals who, under the altered (I cannot say improved) state of the law, would be admissible to the County Infirmaries.

And now let us inquire what system of education is laid down for those who, in order to be eligible under the existing law, must become licentiates of the Dublin College. To obtain this license, a student is required to go through an extended course of study, prescribed by the College, in evidence of which he produces certificates of hospital attendance for not less than three years, as well as attendance on lectures on the various branches of medical science. The minimum period of study is five years: those who have not served apprenticeships to members or licentiates of the College are limited to six, the College making a difference in favour of the apprentice, who is supposed to derive considerable advantage from having a master to instruct him. At the end of his probationary period, the student must submit to an examination on two several days, for an hour each day, in anatomy and physiology, in the theory and practice of surgery and medicine, in chemistry, and in the materia medica. This examination is generally, I may say always, held in the day-time; not, however, with closed doors, but in the presence of as many of the members and licentiates as choose to attend, nor by examiners paid for passing the candidate, but by men who work gratuitously, and often at personal inconvenience. The character of these examinations has been in general highly creditable to the examiners; indeed, some have considered them as too severe. Some years ago this might have been said of them with some justice; at present, however, they can scarcely be so considered; on the contrary, they rather appear to be less strict than they ought to be. The licentiates of the Dublin College of Surgeons are generally young men of very good education—many of them graduates in arts of Dublin University, and many graduates in medicine, either of that or Edinburgh University; a great number, too, avail themselves of the advantages of the splendid Lying-in Hospital of Dublin, and obtain, after examination, the diploma which is conferred by the master and assistant-physician of that institution.

You will observe, sir, that I take it for granted it matters not whether a man belongs to the London or Dublin College when the question is one of abstract merit. But on this very principle, the Londoners manifest a littleness of mind when they boast of their connexion with a College which numbers amongst its members a Cline, an Abernethy, and a Cooper, even supposing we could not meet them, and boast of our Dease, and Colles, and Carmichael. Were the public competent judges of professional merit, it might with safety be left to them to decide between the parties; nor need the Irish licentiates fear the competition; but in the absence of this criterion, I conceive the Irish College not only justified, but imperatively called upon, in the discharge of their public duty, to resist any measure which would remove that local advantage to which the possessor of the Irish diploma is justly entitled by virtue of his superior qualifications.

I fully concur with you that this is not a mere Irish question; nay, I would go farther, and say it is a national one, because it in a great measure may tend to direct the eyes of a reforming legislature, where the most glaring abuses exist. The desire of the Irish College is, and justly, to prevent the inroad into places of trust and emolument of persons whose education they disapprove of, and over whom they can exercise no control. Let the London College, say they, raise their standard of professional knowledge—let there be a full assimilation of the modes of education in the two countries, and then we withdraw our opposition. The examiners of Lincoln's-Inn Fields admit our licentiates to examination without further testimonials, thereby testifying their approbation of our mode of education: this we cannot do with the London members, because we do not approve of their mode of education. Should the examiners of the Dublin College, then, continue to be worthy of public confidence did they sanction the appointment to County Infirmaries of men whom they deem unworthy to be admitted as candidates for their diploma? If, however, reform take place on this side the channel, and the English system of education be raised in its character, then may the Irish College with safety admit that reciprocity between the two bodies which it is the desire of every good man to see established.

But, sir, I have my fears that the standard of surgical merit in Ireland will be gradually lowered: that the College, finding their banner deserted for that of their more easy rival, will be weak enough to compromise; and thus the assimilation will take place so as to bring back the surgical profession in Ireland to a position from which the well-directed efforts of a few enlightened individuals had but a few years ago with difficulty rescued it. If, on the other hand, the opposition of the Irish College receive the support of the legislature, the College here may be induced to require a more minute professional knowledge from the candidates for their diploma, and thus a lasting and permanent good will be effected for both countries, and the malcontent petitioners will have their causes of complaint removed.

I have the honour to be, sir,
Your obedient servant,
AN IRISHMAN IN LONDON.

London, September 1, 1832.

BIOGRAPHICAL MEMOIR OF THE LATE M. SERULLAS*.

GEORGE SIMON SERULLAS was born in 1774, at Poucin, a small village in the department de l'Ain; his father was a notary. Having been bound to an apothecary, the illustrious anatomist, Xavier Bichat, was his fellow apprentice. During the revolution his youth was spent in the military hospitals, where his great intelligence and desire of information showed that he was one destined for the improvement of his age. His talents were recognised by Parmentier and Payen, and at the age of twenty-two years he was appointed "Pharmacien Major;" but during the movement of the armies, Serullas had but little time or means for accomplishing his wishes for instruction; his genius was not as yet unfolded, and notwithstanding his having received medals of encouragement from the Society of Agriculture, and from the Society of Pharmacy, for some memoirs on grape sugar, he was obliged, in the capacity of "Pharmacien en Chef," to waste his ardour in the German campaigns.

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* Abridged from the Dublin Medical Journal.

At the destruction of the empire, he obtained an occupation more conformable to his taste. Appointed, in 1814, as Professor to the Military Hospital of Instruction, at Metz, and at length free to devote himself to his favourite studies, he precipitated himself into that career to which his talents called him. He soon felt the necessity of re-arranging his intellectual existence, and at the age of forty-two he commenced the study of Greek and Mathematics.

With Serullas, study was a passion; days and nights were successively consumed without his regarding fatigue, expense, or health. Thus he soon elevated himself, not only to the level of existing knowledge, but soon excelled his masters. He commenced public courses of lectures at the hospital at Metz, and the interest of the novel experiments which he demonstrated, attracted to his lectures the pupils of the School of Application of Military Genius, who had finished their education at the Polytechnic School.

Serullas met with immense difficulties—he overcame them all. In 1820, he recognized the presence of potassium in antimony fused with cream of tartar; he detailed the curious facts connected with this subject, and demonstrated the existence of arsenic in all the antimonial preparations, except tartar-emetic. He obtained from the last, heated in a close vessel, with charcoal, a true pyrophorus, fulminating by the addition of a single drop of water. His beautiful memoirs on the per-iodide and the proto-iodide of carbon, those on the iodide of cyanogen (a most deleterious compound to experiment on), attracted the attention of the Institute. He prosecuted his researches on bromine, by forming hydro-bromic ether and bromide of cyanogen. He bore away from the illustrious Davy the priority of the fact relative to the gyratory movements of alloys of potassium on mercury and on water, by proving that they depended on electro-chemical action.

It is difficult even to enumerate, in our limited space, the names of the combinations formed by Serullas, of iodine, bromine, chlorine, the hydracids, &c. amongst each other, or with carbon or the metals. Thus his perchloride of cyanogen, obtained in beautiful crystals of a brilliant white colour, is one of the most deleterious substances known—its odour alone is immediately fatal. His novel researches on sulphuric ether are not less interesting; he obtained a neutral sulphate of carburetted hydrogen, and a very remarkable crystalline substance formed of bi-carburetted hydrogen. Besides these numerous new bodies, Serullas succeeded in isolating cyanic acid; he examined the chloride and phosphide of sulphur; he obtained, also, the iodates of potash and iodic acid, by very ingenious processes; he shewed that this last acid is capable of being used in a test for morphia and some other vegetable alkalies. The memoirs on the perchloric acid, and the perchlorates, the crystallization of perchloric acid, and the use of this re-agent to separate potassa from soda; finally, the bromide of silicium, and the hydrobromate of phosphuretted hydrogen, are some of the new substances which we owe to his more recent investigations.

At the death of the illustrious Vauquelin, in 1829, the eyes of all were turned on Serullas; he was overflowing with researches and discoveries. Although destitute of any property but his situation, he spared no expense to procure the most exact instruments—the most rare and new substances, which he submitted to examination. Such was his ardour, that every day he rashly hazarded his life in dangerous experiments; more than once he had nearly perished in the vapours of those compounds of chlorine, cyanogen, azote, &c. Nothing stopped him for an instant; the happiness of a discovery atoned for all difficulties and dangers.

He operated on hitherto untried substances, and penetrated into paths of research before unexplored. Thus did he enter, triumphantly, the portals of the Institute. Far from slumbering in the Academic chair, each week saw him bring into light some train of research; the mine was open, and incessantly he extracted from it the most profound riches. No petty pride or jealousy contaminated his love for science. He animated by his praises the zeal of younger chemists, and mixed, without manifesting superiority, in the labours of the Society of Pharmacy.

Harassed by such unintermitting labours, first in the army and afterwards in his laboratory, his robust and energetic constitution had been broken; his bilious and ardent temperament consumed itself in its own fire. How often, neglecting his health, has he not been all but poisoned, or asphyxiated, in his hazardous experiments! From this negligence resulted a chronic gastritis, which forced him to renounce all stimulating food, and to confine himself to milk and vegetable diet.

From such a state of the digestive organs, we may conceive how predisposed he was to cholera; he was much afraid of it. When that destructive disease appeared in Paris, his temper, hitherto gay and joyful, became sad, and the future appeared to him covered with a sable veil. Thus affected, he went to the obsequies of Cuvier, to the Jardin des Plantes. At this time the chair of chemistry, vacant by the death of Laugier, was destined for him; a most brilliant career was opened for Serullas, and he had the hope of at once increasing his renown by new discoveries, and of being the cause of the comfort of his parents, as he had always been of their happiness.

Who, alas, would have thought, that that very day death should greet him on his return from that sad solemnity! He got wet and cold; the most energetic resources of art were in vain employed against an attack of cholera, which, on the 25th May, bore him away from science and from friendship, after an illness of nine hours.

He is buried near the great naturalist, whose death appeared to have broken the ties that connected him to life.

MIDDLESEX HOSPITAL.

Colica Pictonum produced by Cider.

W. D. a mason, aged 27, was admitted on the 3d of July, under the care of Dr. Francis Hawkins. The account which he gave of his complaints was, that six months previously he had been attacked by pain in the bowels, affecting principally the umbilical region, and attended with sickness and obstinate costiveness. At that time he had had no evacuation by stool for three days. Soon after this attack his hands became paralyzed. At the time of presenting himself at the hospital, he was able to raise the weight of the right hand by extending the wrist, but he could not extend the fingers of the same hand. On the left side he could neither extend the wrist nor the fingers. He was quite positive that he had not been exposed to the fumes of lead or of paint, and that he had made no use whatever of lead in any of his occupations. Upon further examination, however, it appeared that this attack had occurred to him in Gloucestershire, in the neighbourhood of Cheltenham, where he had been for about five months, drinking cider very freely. Once before he had suffered a similar attack of colic in the West Indies. The pain at that time lasted three days, but was not followed by paralysis. At present his bowels have become regular in their action, and his general health good.

He was ordered to take two grains of sulphate of quinine, dissolved in an infusion of mint and cloves, with the addition of a small quantity of sulphuric acid, three times a day. It was further directed that the right hand and arm should be electrified, and a splint applied to the left.

July 4th.—He states that the effect of the electricity has been very considerable, and that at first it almost restored the use of the right hand. He does not, indeed, retain so much power over the muscles, but a permanent sense of warmth remains in that arm which has been electrified.

7th.—A slight degree of purging and griping having been produced by the medicine, he was ordered to take pulv. cinchonæ, ℈j. three times a-day, and to submit both arms to the electric spark.

9th.—He is sure that electricity has been of service to the left arm, as well as to the right.

14th.—The wrist of the left arm can now be extended, as well as that of the right, and he was allowed therefore to lay aside the splint; but he has less power over the fingers of the left hand than over those of the right.

He was ordered to continue the use of the bark and of electricity, which he thinks by no means loses its effect, and to have the wrists and fingers rubbed with the compound camphor liniment.

Remarks.—The foregoing case has some interest attached to it; for this reason, that it illustrates closely, in more than one point, the able and laborious researches of Sir George Baker. To these researches it has been chiefly owing that the effects of lead upon the constitution have been exactly ascertained; that the mineral poison, and the symptoms which it produces, have been so clearly placed in the light and relation of cause and effect; that from the mere observation of the symptoms we can assign their origin with almost unerring certainty. It often happens that the patient suffering from saturnine colic, or palsy, will ignorantly or obstinately deny that he has in any way been exposed to the influence of lead. A case of this kind occurred to a physician of the highest eminence; on which occasion it was accidentally discovered that the patient, who was often engaged in shooting, had been accustomed to keep his charge of shot for some time in his mouth. In the treatment of this species of colic, it is a great advantage that we are at length acquainted with its true and sole origin. Before this had been ascertained, the cider-drinkers of Devonshire, like the subject of the present remarks, had long suffered from colica pictonum, not only with a less certain prospect of relief than might now be offered to them, but without the means of avoiding its cause, of which they were ignorant. It is remarkable that the subject in this case should have suffered previously from the same disorder in the West Indies, his case thus bearing again upon the discoveries of Sir George Baker, who observed that colica pictonum was not only often produced by the use of cider, but also by that of new rum in the West Indies and in America, because during the distillation of this liquor worms, and vessels formed of lead, or of pewter containing lead, were formerly in too frequent use.

The real cause of so many cases of colica pictonum was for a long time pertinaciously denied by those who manufactured rum or cider carelessly, or who fraudulently made use of sugar of lead for the purpose of sweetening subacid liquors. And no small praise is due to Sir George Baker for establishing an important truth by patient industry and perseverance, in opposition to the arts of fraud and interest, and in spite of the incredulity of those who unwillingly admit the truth of a doctrine which is propounded to them for the first time.

The progress of the foregoing case shews that electricity is a stimulant which should not be laid aside in the treatment of saturnine palsy. Strychnine, the remedy which has lately been so highly extolled, was not at once resorted to, because it had not appeared to Dr. Hawkins to effect any marked benefit in the treatment of some other cases of the same disorder in the Middlesex Hospital. One of these cases was that of a servant who had suffered, together with several of her fellow-domestics, in a family in the country, from the use of water out of a cistern containing some lead in solution. The circumstances were similar to those which the late Dr. Warren has related in the Medical Transactions as having occurred in the family of the Duke of Newcastle.

Case of Cynanche Tonsillaris, attended with Hæmorrhage.

J. B. æt. 19, a servant, was admitted on the 3d of July, under the care of Dr. Francis Hawkins. He had been ill four days with sickness of stomach, sore throat, and fever, having, as he believed, caught a severe cold. The right tonsil was observed to be considerably enlarged, and there was a small white speck on the left. Pulse frequent; tongue furred; no redness nor heat of skin. He was ordered to take the Infus. Menthæ Sulphuricum, with ʒss. of Sulphate of Magnesia, three times a-day, and to use a gargle, composed of the Infus. Lini Comp. with Sulphuric Acid and Tincture of Myrrh.

July 4th.—The difficulty of swallowing and breathing is not at all diminished, the tonsils being fully as much swelled as before. Pulse 120; he is rather more feverish and hot.

Hirud. viij. gutteri. Hydrarg. Submur. gr. iv. Pulv. Antimon. gr. vj. Pulv. Ipecac. C. gr. v. f. pil. iij. horâ somni sumend. Contin. Haust. et Gargarima.

5th.—On the evening of this day he was found spitting blood in very considerable quantity. He says that about 9 P.M. he felt something give way in his throat, when "a nasty discharge" was brought up, and followed by blood, after which about three pints of florid blood were poured from his throat. The right tonsil was now seen to be partially covered with a coagulum of blood. Pulse 110, with some power; skin hot; bowels open.

He was immediately bled to the amount of six ounces, when he fainted. A saturated solution of alum to be used as a gargle occasionally: at all other times to dissolve ice in his mouth.

6th. The hæmorrhage has nearly ceased; a large quantity of half-digested blood has been passed by stool; the glands of the neck on each side are hard and painful; pulse 96, not much reduced; bowels not relaxed.

To continue the Infus. Menth. Sulph. with the addition of ♏v. of dilute Sulph. Acid, ʒj. of Sulphate of Magnesia, and ʒss. of Syrup of Poppies, three times a-day. The use of ice to be continued also.

On its being subsequently intermitted, there was on the 7th a slight return of hæmorrhage, the sputa being sanguineous, and stools containing blood as before. He was then ordered to continue the draught, and to take at bed-time, Hydr. Submur. gr. ij. Extr. Coloc. C. gr. vj.

9th.—The white specks which were observed on the right tonsil are fast diminishing in size.

An alum gargle to be used, with Tincture of Myrrh.

14th.—The right tonsil remains enlarged, although the specks upon its surface have entirely disappeared.

Quinæ Sulph. gr. ij. to be added to the draught.

On the 23d he was discharged cured.

Remarks.—The foregoing case attracted the greater attention on account of the similarity which it bore to another case of cynanche tonsillaris, attended with severe and even fatal hæmorrhage, which had occurred some time ago at the Middlesex Hospital. The appearance of the tonsils in that case (of which Dr. Watson gave a full account in the 3d volume of the Medical Gazette) was precisely the same as in the one which has now been related; the cause also was the same—viz. exposure to cold; and the effect the same—namely, the formation of an abscess behind and below one of the tonsils, by which an artery must have been ulcerated and arterial hæmorrhage produced. But in Dr. Watson's case, the abscess (which was formed under the *left* tonsil) was larger and less tractable, and formed an external as well as internal opening. The hæmorrhage also was much longer continued; and, when the patient sunk, it was found that the trunk of the lingual artery had been divided by ulceration.

The history of these cases may serve to point out the situation in which an abscess is liable to be formed in consequence of inflammation of the tonsils and fauces—whether matter be formed in the tonsils themselves or not: viz. immediately behind and below one or the other of these glands;—and when such an occurrence has taken place, we see likewise the danger that may subsequently ensue.

In the latter of these cases we may observe the excellent effect which ice, suffered to dissolve in the mouth, has in allaying inflammation of the fauces and restraining hæmorrhage: for, upon the ice being accidentally withdrawn, in this case, for a time only, somewhat too early, the bleeding was immediately renewed.

TREATMENT OF CHOLERA AT LYNN.

We have received a letter, signed by seven highly-respectable practitioners at Lynn, in reference to a postscript appended to Mr. J. B. Whiting's paper, in our No. of the 8th instant, in which he says that the plan of treating the premonitory symptoms of cholera by means of opium and astringents was recommended to them by his brother, Dr. Whiting. The medical gentlemen whose signatures are appended, deny that they were influenced or guided in any way by Dr. Whiting; to whom, nevertheless, they mean no disrespect.

Mr. J. B. Whiting, hearing that such a letter was sent, has also addressed us. It appears that his brother, some time ago, wrote to him, advising the use of opium, and the letter in question was seen by almost every medical man in Lynn. We cannot make room for the letters, which are of no general interest; and we trust that, on consideration, the several parties will think we have adopted the wisest course.

CHOLERA AT ETON (?).

We have received a letter from Mr. Moss, in answer to the one from Mr. Okes, inserted in our last. Mr. Moss reiterates his former statements—says Dr. Fergusson coincided with him in opinion, regarding the case alluded to by Mr. O., and adds, that he furnished the addresses of upwards of thirty patients affected with cholera. We cannot admit farther correspondence on the subject.

LIBRARY OF THE COLLEGE OF SURGEONS.

To the Editor of the London Medical Gazette.

Sir,

Would you allow me, through the medium of your useful publication, to suggest to the Council of the College of Surgeons the great benefit which would accrue to the members residing in London if the hours for admission to their valuable library were altered or prolonged. At present many are precluded from availing themselves of consulting the works contained in it, as their time during the day is wholly taken up with their professional duties; whereas, if the library were permitted to be open a few hours every evening, they would have an equal opportunity of benefiting themselves by its contents as those whose engagements are less pressing. By doing this, sir, you will much oblige,

Your humble servant,
A Reading Member.

London, Sept. 6, 1832.

P.S.—It will be remembered that there are many young men whose time is taken up in a variety of other ways, during the day, besides that of the active practice of their profession, and who feel much the privation of access to good books during the leisure hours the evening generally affords.

WEEKLY ACCOUNT OF BURIALS,

From the Bills of Mortality, *Sept.* 18, 1832.

Abscess	5	Hooping-Cough	4
Age and Debility	81	Inflammation	36
Apoplexy	4	Inflammation of the	
Asthma	10	Bowels & Stomach	34
Cancer	3	Brain	6
Childbirth	7	Lungs and Pleura	2
Cholera	164	Insanity	3
Consumption	90	Jaundice	1
Convulsions	39	Liver, Diseases of the	10
Croup	2	Locked Jaw	1
Dentition or Teething	9	Measles	15
Diarrhœa	2	Mortification	1
Dropsy	15	Paralysis	2
Dropsy on the Brain	17	Small-Pox	33
Dropsy on the Chest	4	Spasms	2
Erysipelas	1	Sore Throat and	
Fever	24	Quinsey	2
Fever, Scarlet	12	Thrush	3
Fever, Typhus	4	Unknown causes	2
Gout	2		
Heart, Diseases of	1	Stillborn	13

Decrease of Burials, as compared with the preceding Week . . . 286

METEOROLOGICAL JOURNAL.

September 1832.	Thermometer.		Barometer.	
Thursday . 13	from 50	to 66	30 03	to 29·92
Friday. . . 14	45	64	29 90	29 80
Saturday . 15	41	62	29 90	30 04
Sunday . . 16	41	64	30 09	30·15
Monday . . 17	45	63	30·22	30·07
Tuesday . 18	50	61	29 94	30·04
Wednesday 19	35	57	30·10	30 36

Wind variable, S.W. prevailing.

The 19th clear, otherwise alternately clear and cloudy; rain at times on the 13th, 15th, and 18th.

Rain fallen, ·025 of an inch.

Charles Henry Adams.

W. Wilson, Printer, 57, Skinner-Street, London.

THE
LONDON MEDICAL GAZETTE,
BEING A
WEEKLY JOURNAL
OF
Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 29, 1832.

OOTTERPARA PUBLIC LIBRARY 1859

ST. GEORGE'S HOSPITAL.

CASES OF ABSCESS IN THE PELVIS,

With Clinical Remarks,

By Mr. Cæsar Hawkins.

1. *Diseased Sacrum—Fæcal Abscess.*
2. *Diseased Hip—Abscess bursting into the Bladder.*
3. *Diseased Sacrum—Paraplegic Symptoms.*

Case I.—*Diseased Sacrum—Fæcal Abscess.*

Elizabeth Bartholemew, æt. 28, admitted June 13, 1832, under the care of Mr. Hawkins. She was confined about twelve months ago, but has not nursed her child, having been obliged to wean it when about six weeks old, on account of sore nipples. About nine months ago she had inflammation of the bowels, which required the application of leeches, and she subsequently suffered from cholera, by which illnesses her health suffered materially. About eight months ago she first experienced pain and tenderness in the left side of the abdomen and groin, followed in a fortnight by swelling; and, about five or six weeks after this, a puncture was made, by which she says two quarts of very offensive pus were evacuated. The discharge continued to be very fœtid for a few days, and then became more healthy, and the orifice is frequently quite closed. She has not menstruated since her confinement, but she says that at each menstrual period she has acute pain in the back, with bearing down, and pain in micturition, with frequent desire to make water; and at those periods the abscess discharges, for a few days, a thin fluid unmixed with blood, and then heals up again. She says she has not become much worse in health since the abscess first opened, but is very thin and emaciated, and perspires much, and has a troublesome cough, with mucopurulent expectoration; she sleeps ill, has a bad appetite, and a quick weak pulse. There is no pain or tenderness in the back or loins, and she says she never feels any except at the supposed menstrual periods. Matter can be pressed down from the iliac fossa from a considerable sized cavity, through two openings on the front of the abdomen, near Poupart's ligament.

June 20th.—Some fæcal matter was observed to come through the openings.

July 17th.—This circumstance was again observed to-day.

July 26th.—She has much improved in health under a nourishing diet, with a small quantity of wine and porter, and the use of bark and quinine, with opiates to relieve her restlessness and cough, and once some chalk mixture, on account of diarrhœa. The openings into the abscess have been enlarged, so that the discharge has been free, and it has gradually diminished, as if the cavity was contracting, and she has had no pain since the bistoury was employed.

To-day the discharge has been more profuse, and mixed with fæces, and she has general pain and considerable tenderness over the whole abdomen, with rigors, succeeded by hot and dry skin, and a hard pulse, 120.

Hirud. xij. abdom.; Haust. Salin. Effervescence, 4tis horis.

27th.—Relieved by the leeches.

31st.—Some return of pain and tenderness, with more profuse discharge, and mixture of fæces; much perspiration and great debility. The openings in the groin, which had contracted, being again laid open, a considerable cavity was found to have formed in the hollow of the ilium and the outside of the hip.

Vini Rubri, ℥vj.; Jusculi Bovini, Oiss. &c. Fotus Papav. Abdomini.

August 23d.—She has occasionally had the pain in the abdomen, which is relieved by fomentation, and her wine has not been intermitted. She has also again suffered once or twice from diarrhœa, requiring opiate enemata and chalk mixture occasionally. Her cough has been lately more troublesome, and the expectoration more copious.

September 3d.—She has gradually got weaker, and has suffered much from irritation in the bowels, producing frequent diarrhœa. The abscess has lately discharged less pus, but frequently fæcal matter. Died this morning.

On passing a director from the wound, which was much contracted, it was found to lead directly upwards towards the spine; and on laying open the cavity of the abscess, which was reduced to a mere sinus, it was found to lead to exposed bone at the upper part of the sacrum, and the under surface of the body of the last lumbar vertebra, the cartilage of which at the posterior surface was softened, and a probe passed behind to the opposite side of the sacrum, which was also exposed and covered with a small quantity of pus. From the general cavity, which occupied the substance of the psoas muscle, a sinus ran outwards in the substance of the iliacus internus, and a portion of carious bone, of the size of a shilling, was found at the centre of the crest of the ilium, but did not extend beyond. Inwardly the cavity extended over the linea ileopectinea for about an inch and a half, and here communicated with the sigmoid flexure of the colon (which was adherent to the cyst) by two small orifices, about large enough to admit the point of a common director. The intestines were collapsed and generally healthy, but a portion of ilium, six inches in length, was adherent to the side of the abscess at the margin of the pelvis; recent lymph was deposited on its outer surface, and the mucous membrane was highly inflamed and ulcerated in many places. The uterus was also adherent at this part.

The lungs were much diseased, having several vomicæ at the apex of each, and tubercles throughout their whole substance.

Case II.—*Diseased Hip—Abscess bursting into the Bladder.*

Geo. Farrow, æt. 15, admitted July 5, under the care of Mr. Hawkins. He is a weak scrofulous lad, who has been constantly in ill health. About ten weeks before his admission, he had a violent cold, with fever, and an abscess formed over the right tibia, which, when opened, was found to be connected with dead bone; and a few days before his admission a fresh abscess formed in the calf of the leg, with much inflammation, in consequence of the matter from the back of the tibia not being able to escape, two-thirds of the circumference of the bone being dead. He has also complained, for the last three or four weeks, of much pain about the hip of the same side, and has had an issue behind the trochanter, which has been allowed to heal, in consequence of the irritation it excited. On admission, the openings over the tibia were discharging freely, and there was some swelling apparently connected with the femur rather than the hip-joint, which extended some way down the bone.

℞ Haust. Cinchonæ, ℥iss.; Acid. Nitric. ♏viij. M. bis die sumend. Jusculi Bovini, Oj. quotidie. Dieta ordinaria Cerevisiæ fortioris, Oj. quotidie.

July 31st.—Health much improved. Very little thickening about the femur, and the pain is nearly gone. The ulcers on the leg are also healthy, and much contracted in size, and the exposed bone separating.

August 3d.—Bowels much disturbed, with sickness.

℞ Haust. Rhei Comp. h. s. s.; Cont. Haust. Cinch. c. Conf. Aromat. ℈j. vice Acid. Nitric.

11th.—Leg very painful; bowels now confined, and painful.

℞ Hydrag. Submur. gr. iv.; Pulv. Rhei, ℈j. M. h. s. s.

12th.—The wound in the leg is sloughing rapidly, with violent pain.

℞ Decoct. Cinch. ℥iss.; Ammon. Carbon. gr. iv.; Tinct. Opii, ♏xviij. M. 4tis horis.

14th.—Wound still sloughing. The bark heavy on the stomach.

Vini Rubri ℥iv. quotidie.

℞ Mist. Camphoræ, ℥iss; Træ. Opii, ♏xv.; Ammon. Carbon. gr. v. M. 4tis horis.

17th.—Sloughing stopped. Pain ceased.

Cont. Mist. c. Træ. Opii ♏v.

21st.—Wound quite clean, the sloughs having separated.

℞ Infus. Cuspariæ, ℥iss; Acid. Nitric. ♏v.; Acid. Muriat, ♏vj.; Syr. Zingib. ʒss. M. bis die.

℞ Mist. Camphoræ, ℥iss.; Træ. Opii ♏x.; Sp. Æther. Nitros, ʒss. M. o. n. s.

28th.—Swelling and pain in the right groin, where a gland is felt enlarged and tender.

Appl. Hirud. vj.

September 4th.—Since the last note, it was evident that the pain in the groin depended on deep abscess, which has been fomented. He has had a good deal of anxiety and fever, much pain and tenderness in the thigh, which is swelled to half its length, though no matter is perceptible to the touch, and he complains of his water scalding him.

5th.—Yesterday afternoon the pain ceased, and he felt a sudden desire to make water, and discharged a considerable quantity of pus *from the bladder*; in the course of three or four hours, probably about two pints having been evacuated: the portion first discharged being dark-coloured, but the rest becoming subsequently white and healthy in appearance.

Haust. Opiat. h. s. s.; Vini Rubri, ℥vij. quotidie.

11th.—Pain and swelling in the thigh much lessened, and his health is somewhat improved, but there is still a good deal of swelling and much tenderness in the groin and lower part of the abdomen on pressure. The water continues mixed with a great deal of healthy pus, which is discharged rather frequently, but without pain or inconvenience. Once or twice the water has been clear, and he thought the pain was increased by this apparent want of free communication between the abscess and the bladder. No fluid can be felt in the thigh.

20th.—Going on well, the abscess continuing to discharge by the bladder.

CASE III. — *Diseased Sacrum—Paraplegic Symptoms.*

Jane Elwood, æt. 26, admitted August 1, 1832, under the care of Mr. Hawkins.

Ten or twelve years ago she fell against a stool and struck the sacrum, to which a blister was applied. About two months afterwards an abscess formed at the side of the sacrum, and some dead bone has come away since. She has been twice pregnant, and each time the abscess burst open again after having been healed. It has now continued open a considerable time, and a small piece of dead bone is felt at the bottom of the sinus. About two months ago, a fresh abscess formed over the posterior part of the right ilium, which is now of large size. Since this has formed she has become almost completely paralytic in the lower extremities, especially in the left, which was always weak, and she has had paralysis of the bladder and rectum, so that both the urine and fæces are discharged involuntarily. Her health is much impaired, and she is much emaciated.

August 3d.—The abscess was opened, and above a pint of pus evacuated.

5th.—℞ Haust. Cinch. ℥iss.; Conf. Aromat. ℈j. M. ter die.

8th.—The bark not borne well; bowels constipated.

℞ Hydrag. Submur. gr. iij.; Pulv. Rhei, gr. xv.; Pulv. Zingib. gr. x. M. h. s. s. Omit. Cinch.

14th.—℞ Infus. Calumbæ, ℥iss.; Ammon. Carbon. gr. v.; Confect. Aromat ℈j. M. bis die. Cerevisiæ fort. Oj. quotidie.

22d.—Improving; although another smaller abscess is burst in the loins.

September 1st.—Both abscesses healed up. She has regained much flesh and strength, and is able to sit up regularly. She can use her legs so as to walk with the assistance of another person, and the bladder and rectum are recovering their power, so that she can retain all but very fluid evacuations, and can hold the water for several hours, with perfect power of expulsion.

20th.—Nearly well.

CLINICAL OBSERVATIONS.

There are several cases of abscess about the pelvis which have been recently under your observation, which I will take as the foundation for a few remarks, as they are by no means unfrequent, and are sometimes obscure and difficult to manage. In one patient, who died a few days since, you have observed an opening in the front of the abdomen discharging fæces: would you look for the cause of such an occurrence in the sacrum? There is a second patient, under Mr. Keate's care, who has had an abscess opened in nearly the same situation, at the side of the abdomen, from which a piece of the os pubis has come away. In a third, a lad has had scalding in making water, with purulent discharge from the bladder; the cause of which is not in the bladder or kidney, but in disease of the thigh-bone or hip-joint. Here, again, are some preparations, in which abscesses connected with the hip have made their way into the pelvis in other directions. In short, the causes of these abscesses may be so numerous, and their course so varied and extraordinary, as sometimes to render them very puzzling and complicated. In Farrow's case the cause was clearly in the thigh-bone, in a very scrofulous subject.—[The notes of the case which we have narrated were here referred to.]—But let us take the case of Bartholemew more in detail, which has shewn you the necessity of careful examination to ascertain the cause of such an abscess.—[Mr. H. here read some notes of her history at the time of admission.]

Now a large abscess in the iliac fossa, or extensive sinuses in the groin or lower part of the abdomen, in which the probe may be buried, naturally lead one, in the first instance, to suspect psoas abscess, depending on diseased spine. The most careful examination, however, could not detect any tenderness in any part of the back or loins, nor any sensibility to the impression of a hot sponge, which sometimes discovers disease of the vertebral column when mere manual examination fails to do so. She had met with no accident, and expressly denied having any pain in the back, except periodically, which she herself attributed to menstruation. But if there was no disease of the spine, might it be a simple abscess in the cellular membrane of the psoas and iliac muscles; or might it be an abscess connected with some disease of the ovarium? I saw a patient formerly, at the Asylum, with Dr. Seymour, when we were colleagues at that institution, who had a large abscess in the ovarium, which burst and discharged by the vagina; subsequent to which she was admitted, under our joint care, into this hospital since we have have been colleagues here also: the abscess having now ulcerated both into the colon and the bladder, so that part of her urine, and the fluid part of the fæces, with flatus, came through sinuses in the groin resembling those of Bartholemew, and she was always worse at the periods of menstruation, when the discharge used to be coloured, no doubt by communication with the vagina. I recollect another woman also, when I was house-surgeon here, who used to mentruate regularly through the abdomen; in whom the bones of an extra-uterine fœtus were evacuated by abscess several years after conception. Now Bartholemew also said that the abscess used to re-open, with much pain, at each period that she ought to have menstruated, though the discharge was not red, but, thin, neither was it per vaginam. In a few days, however, further light seemed to be thrown on her case, for some fæces were found to come away by the openings; which circumstance, combined with her history of cholera and inflammation of the bowels before the formation of the abscess, and the very fœtid quality of the matter evacuated at that time, seemed to make it most probable that the abscess depended on ulceration of the sigmoid flexure of the colon. It is true a fæcal abscess forms most frequently on the right side, from lodgment of fæces or foreign bodies in the cæcum, but they may occur in other situations also; and you may perhaps recollect a man who was under my care not long since, with an abscess in the umbilicus, which, from the smell and colour of the discharge, I have no doubt communicated with the arch of the colon, though fortunately I had no opportunity of verifying the fact, as the man got well.

On whatever cause, however, the disease depended, the treatment was clear; the indications being to prevent accumulation of matter by giving it a free exit, and to support her strength by medicine and diet, although, from the apprehended state of her chest and her great debility, the chances were much against her recovery. At first, indeed, she improved very much, but observe the remainder of her case. [Mr. Hawkins here referred to the case-book for the detail before given.]

Now you will observe, from this case, that the neighbourhood of an abscess to the intestines is not unattended with dangers which are not usually anticipated from a psoas abscess. The intestines (both the colon and the small intestine) adhered to the side of the abscess—both were inflamed—both were ulcerated in the interior, and the colon was even ulcerated through all its coats, making an artificial anus of a dangerous and nearly hopeless kind. You will observe also, that ulceration once excited, your remedial means are constantly counteracted, repeated attacks of diarrhœa destroy your patient's strength as fast or faster than you restore it, and they frequently sink under the disease from this cause only. But this is not all—there is danger also of general peritonitis; especially of that low and fatal kind which so often occurs in debilitated persons. You observe here some lymph on the small intestine, which was adherent to the abscess, but it was merely local, and was not severe enough even to require leeches more than once; so that, in fact, I considered her in greater danger from diarrhœa and irritation of the mucous membrane, than from peritonitis, and treated her accordingly, endeavouring to quiet this irritation while I supported her general strength. Take care, however, not to mistake the apparent debility arising from inflammation of the peritoneum for real weakness. A man was under my care with an abscess between the external and internal oblique muscles, which I opened. This man was carried off in about forty-eight hours by extensive peitonitis, and I found, on seeing him after, that on the first accession of the inflammation, wine had been ordered for the apparently sudden debility.

We found then that the cause of her abscess was disease of the front of the sacrum, and the junction of this bone with the last lumbar vertebra, the matter making its way along the psoas muscle; and this is a common course when the disease is on the inside of the bone. But it may proceed in other directions. Here is the os coccygis of a patient of mine, which I removed by operation, together with part of the sacrum. He was admitted into the hospital, with a fistula by the side of the anus, for operation, but of course, on finding that the probe touched the sacrum high up in the pelvis, the operation was not performed. I was enabled to make an opening behind the sacrum, (where the rectum had been opened by ulceration,) and afterwards removed this bone; the consequence of which was that the fistula was cured. A boy was under my care with a sinus behind the upper part of the sacrum, for which I could find no diseased bone or other cause; but after some little time, upon making an extensive incision of the sinus through the gluteus muscle, by the side of the sciatic nerve, I discovered that the opening led up again, at an acute angle, through the sciatic notch into the interior of the pelvis; the whole of the inner surface of the sacrum being carious or dead. Sometimes the abscesses from diseased sacrum proceed in several directions, even on both sides of the body at the same time. If, again, the posterior part of the sacrum be the seat of the disease, the abscesses will be over the bone itself, or on the posterior surface of the ilium, or in the loins, as you see in Elwood's case. This patient has also another set of symptoms, which Bartholemew did not suffer from, and which are more rare. [Mr. Hawkins here mentioned the particulars of her case, showing the occurrence of paraplegic symptoms.]

Next, as to the treatment of such cases of abscesses from any cause about the pelvis.

1st. Is there an abscess not yet opened? If the skin is getting thin—if the patient is suffering much irritation from the formation of matter, and especially if he has the peculiar symptoms of a *foul abscess*—*i. e.* one containing sulphuretted hydrogen, either from diseased bone or fæcal abscess, or sloughs, let out the matter as soon as possible. You saw how immediately Elwood was relieved by this means, and how much less Farrow has suffered since the pus came away through the bladder than when it was confined among inflamed parts. You lessen also the danger of peritonitis by taking off the tension of the abscess. A poor little chimney-sweep was mounting a donkey, when he fell off, and trying to get up again he fell over on the other side, and hurt himself considerably on the hip. Three weeks afterwards, he was admitted into the hospital extremely emaciated, and with high irritative fever. During the first few days I could not ascertain the exact cause of his sufferings, and during that time he was repeatedly threatened with peritonitis, which required leeches and other measures. Then I discovered fluctuation over the front of the iliac region, and making an incision through the linea semilunaris, some way above Poupart's ligament, I let out a few ounces of pus from between the peritoneum and the transversalis muscle, the finger passing behind the rectus muscle on one side, and into the iliac fossa on the other side, within the abscess. From this time there was not the least sickness, or tenderness, or tension of the abdomen, and he got rapidly well.

Or 2dly, is the abscess already open, but the openings not sufficiently large to allow a free exit to the pus?—Then enlarge these openings, or make another, if possible, still nearer to the seat of the disease. You saw how much Bartholemew was relieved by this incision two or three times, and at the time of her death the abscess had contracted to a mere sinus, and, but for the diseased bone at the bottom, such a sinus may altogether fill up. It may do so even when some disease still remains, as in Elwood's case, whom I do not consider permanently cured, though all the openings are at present closed. I should wish, if I have an opportunity, to make a counter opening in Farrow's thigh, to prevent the pus entering the bladder, but I do not at present feel sufficiently positive of the situation of the abscess to do so, though I suppose it is in contact with a considerable part of the femur.

3. A third indication is to relieve irritation by opiates, and to support the patient's strength by proper food, by bark or quinine, and other means which I need not enumerate. Even where the abscess depends on diseased bone, and the bone is not accessible, constitutional means alone will sometimes effect a cure. A young woman called at my house a few days ago perfectly well, who was formerly under my care with disease either of the ilium or sacrum, who had two sinuses in the groin like Bartholemew's, one in the course of the psoas muscle, the other leading deep into the pelvis and communicating apparently with the vagina, as pus had escaped in that direction, and who had abscesses also in the lower part of the loins, where I felt diseased bone at some depth.

4. Is the bone carious or ulcerated, but not dead?—If accessible and superficial, stimulating applications, especially of nitric or muriatic acid, certainly assist in restoring a healthy action in the vessels of the part, while you attend to the more important object of altering the state of the constitution, and under their use the ulcer may heal, or some portion may exfoliate, and the surface below become healthy. If not accessible, as for instance, when on the inside of the sacrum, as in Bartholemew's case, something may no doubt be done at the proper period by blisters and counter-irritants, in the same way as you know that caries of the vertebræ is often checked by their employment; some benefit might perhaps have been obtained if a caustic issue had been made early in the case of Bartholemew. But unfortunately, in general, the insidious nature of the early symptoms prevents the early recognition of the disease, and counter-irritation has less power in diminishing suppuration than it has in preventing its occurrence; besides which, issues have less power over scrofulous disease of bones than over ulceration of the cartilages of joints. The issue did no good, for instance, for Farrow's disease of the thigh bone, and they do little for caries of the sacrum or innominatum.

5. Is there some dead bone, separated, or in progress of exfoliation?—Much more good can be done, than is usually imagined, in these cases, by the same treatment which you so often see employed in this hospital in necrosis of the long bones. In Mr. Keate's case, a considerable portion of the pubes has spontaneously separated, but this is generally a very tedious and slow process if left

entirely to nature, and you can hasten the cure by laying open the dead bone, and extracting it with the forceps or bone nippers. An old soldier, whom I have twice operated on for strangulated hernia, received a musket wound at the siege of Badajoz, the ball passing through the side of the abdomen, through the ilium, and out again at the back of the thigh; the wound had remained open ever since, discharging so copiously and exciting so much irritation, as frequently to incapacitate him for active exertion. I cut down to the bone, through the glutei muscles, and removed a portion of loose bone, and cut off some soft carious bone with a chisel, leaving an opening which allowed several fingers to pass through it into the iliac fossa. The wound, which had remained open for nineteen years, healed quickly, and has continued sound ever since. I recollect a patient of Mr. Brodie's, from whom a considerable portion of the tuberosity and ramus of the ischium was removed by an incision through the adductor muscles. Here, again, is the os coccygis of another patient, which I removed by operation, with relief of the same kind.

There are only two other circumstances connected with abscesses about the pelvis which our time will allow me to allude to. The first is the frequent occurrence of phthisis, in conjunction with large abscesses, in this situation, which you have witnessed in Bartholemow's case. The coincidence is very common, and renders it necessary to give a guarded prognosis, even in cases where the local condition seems tolerably favourable. It may be that tubercles in the lungs dispose a person to have abscesses formed in the pelvis, for the same reason that a phthisical state of the lungs so frequently occasions piles and fistulæ in ano; the alteration, namely of the circulation in the extremities of the vena portæ, owing to the mechanical obstruction in the lungs; or else it arises from the debilitated condition of the patient, which calls into activity any latent disposition to disease in the lungs.

The other circumstance, is this—you see that a portion of the ilium, out of the course of the psoas abscess, was exposed and dead in this poor woman, and this is, in fact, frequently found to be a consequence of large abscesses; so that a person with disease in the spine frequently has caries established in another part of the spine, or in one of the bones of the pelvis, from the mere contact of matter. This serves to shew you the propriety of opening ever chronic abscesses tolerably early, lest the addition of dead bone in another situation render the case more complicated, and the cure more uncertain.

TEST FOR COFFEE, &c.

To the Editor of the London Medical Gazette.

Sir,

As that superficial but useful method of examining substances, termed *testing*, is in vegetables attended with difficulty, any attempt to simplify it must be useful: accordingly, I have sent, if you deem it worthy of an insertion in your excellent journal, an analysis of Rye (*Secale cercale*) and Coffee (*Coffæa arabica*,) both raw and roasted; and the former being very frequently used to adulterate the latter, I have indicated a test for the detection of the fraud; and as your journal is, also, I believe, devoted partly to the diffusion of chemical science, I have copied in my analysis of the leaves of the *Arum maculatum*, none having been published since the discovery of its alkaloid element.

	COFFEE.		RYE.	
	Raw.	*Roasted.*	*Raw.*	*Roasted.*
Lignin . .	50·5	49·0	20·6	18·0
Caffein . .	3·0	—	—	—
Tannin . .	—	2·0	—	—
Starch . .	—	—	49·4	—
Resin . . .	1·5	6·0	5·0	7·0
Amidine*.	—	—	—	38·0
Gum . . .	12·0	?	?	—
Albumen .	1·0	—	?	—
Gluten . .	—	—	4·0	2·0
Extractive matter .	26·0	30·0	18·0	20·0
Carbonized do. . . .	—	8·0	—	11·0
Loss . . .	6·0	5·0	3·0	4·0
	100	100	100	100

Now, as it is well known that iodine forms a blue iodide with amidine or starch, and as rye contains 38 per cent. of the former substance (when roasted,) while it is absent from coffee, it follows that a mixture of roasted rye with roasted coffee is easy of detection, of which any one can convince himself by a simple experiment: for this purpose the alcoholic tincture of iodine is the most convenient form of the test.

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* I have here limited this term to starch torrefied below 480°.

The analysis of the *Arum* indicated the presence of the following substances:

1. Aronia }
2. Malic Acid } combined.
3. Malate Lime.
4. Phosphate do.
5. An Ammoniacal Salt.
6. Chlorophylle.
7. Gum.
8. Lignin.
9. Wax.
10. Albumen.

The ashes gave traces of copper.

I am, sir,
Yours obediently,
GOLDING BIRD, JUN.

44, Seymour-Street, Euston-Square,
Sept. 17, 1832.

PAPERS ON CHOLERA.

FACTS AND OBSERVATIONS
Relative to the Nature and Treatment of the
FEVER OF THE PESTILENCE,
COMMONLY CALLED
THE CHOLERA MORBUS.

By WILLIAM JOHN THOMAS, M.R.C.S.

IN my last communication I brought forward a series of facts and observations relative to the communicability of the fever of the pestilence. In my present paper I shall relate, as precisely as possible, the characteristic symptoms of this singular affection; and I shall more especially advert to those symptomatic modifications of the complicated variety of the disease, which I have had an opportunity of observing during my professional investigations. To enter into a detail of all the cases would be impracticable at the present moment, although every case that I have seen has presented shades of variation from the maximum of intensity, which to the investigator of morbid phenomena possess considerable scientific attractions. The fever of the pestilence, unlike any other disease with which I am acquainted, appears to comprise within its comprehensive range a vast variety of symptoms, from the first dawn of the irritability of impending congestion to that solemn and mortal appearance of constitutional agony, when the hand of death is about to interrupt pulsation, and the machinery of life to stand still for ever. Many of the local diseases are occasionally simulated in the symptoms of this pestilential malady. Thus we are presented in this epitome of diseases with the whole of the second section of the first order pyrexiæ, with a majority of the genera of the order phlegmasiæ, with an occasional appearance of the exanthemata, and with parts of the fourth and fifth orders of the first class. Nearly the whole of the comata adynamia and spasmi have occasionally appeared in the train of this disease. The sixty-fifth, sixty-sixth, and sixty-seventh genera of the order vesania, with a collateral supply from the cachexiæ, and also from the fourth class of Dr. Cullen's nosological arrangement, are simulated in the symptomatic modifications of the pestilence. When I reflect upon the long train of evil symptoms, so intense in their action, and so varied in their manifestations, that frequently succeed the slight watery purgations, I cannot but regret that the attention of the physician should be directed to an individual feature of an insidious disease. Having observed with minute attention the progress of the epidemic in some hundreds of cases, I have been led to conclude that the disease which has been called the cholera morbus is, in fact, a pestilential fever of a most formidable character. The purging, cramps, and vomiting, upon which so much stress has been laid in the most fatal forms of this complaint, are occasionally so trifling as to attract little attention. Fevers in general have their different stages: the premonitory stage; the cold stage, or collapse; and the hot stage. The more durable and intense the second stage may be, the more formidable will be the stage of reaction. In the fever of the pestilence we have the premonitory stage, in which the constitutional irritability deranges the sympathies and disorders the specific action of the various organs. This irritability of impending congestion is an effort of nature to throw off the exciting cause of the disease. It is, in fact, the commencement of the fever, and all fevers commence in the same manner; for the cold stage of fever is merely the first manifestation of the predominance of the morbid power over the salutary efforts of nature. This view of the subject may, in some degree, answer the question which has frequently been put to me, How can you consider this person, who died in the blue stage, to have expired in a fever? If the majority of deaths occur in the cold stage, surely you cannot consider such persons the victims of a disease which might have cut them off had they survived long enough to suffer from its effects? That the majority of deaths takes place in the cold stage I will readily grant; for upon referring to my private note-book, I find out of 45 deaths, 33 took place in the cold stage, or stage of collapse, and 12 in the consecutive fever, as it is generally called, but which I maintain to be merely a development of the operations of those latent pestilential powers which were acting with their sedative energies on the nervous system, to produce the vascular collapse. If then we can bring ourselves to consider the blue stage of cholera merely as a part or parcel of that constitutional influence from which it originates,—as one step only in the progress of a most formidable malady, we shall find our views of the nature of the disorder more enlarged, and our treatment more comprehensive and scientific. The disease which we are now considering is, in fact, a most formidable constitutional affection, and not a local malady, as the term cholera would appear to imply. I do not mean to underrate the serious character of the affection of the bowels, but we should dispassionately consider this affection in relation to the constitutional disorder, in order to ascertain precisely whether these watery purgations be sympathetic or idiopathic operations. It is most difficult to trace the almost invisible entanglement of causes and effects, in a disease so mysterious as the present; and it behoves us, in our progressive investigations, to be well aware that our deductions are drawn from incontrovertible facts. It may be expedient in this place to glance with a physiological eye upon the constitutional origin of the disease:—in doing this I shall take as data certain facts which I shall presently adduce. In my communication, page 644 of this journal, will be found a table of twelve distinct instances of the communicability of the fever of the pestilence. From these facts it appears that persons who have been in communication with each other have been affected by the same disease in rapid succession: from which evidence we necessarily infer that some morbid peculiarity must have emanated from the person first indisposed, and have been imparted to other individuals in the order of succession. Taking it for granted that some specific power has been in operation to engender in other animal bodies symptoms identical in character with those exhibited in the person of the original individual, we are led to inquire what that specific power may be, and what is the nature of its operation. In this part of our inquiry we should be compelled to pause, if a most important fact did not present itself as another step in our ascent upwards to the proximate cause of the phenomena of choleroid irritation. I have frequently perceived in persons labouring under the malignant collapse, and also in certain conditions of the fever stage, a peculiar effluvium, apparently produced by a pestilential decomposition of the fluid portions of the diseased bodies. This effluvium possesses an odour of so peculiar a character, as to lead us naturally to suspect that certain specific properties are contained within its essential principles. The effluvium mingles with the atmosphere, and the air which we breathe being impregnated with the volatile virus, is inhaled into the lungs. From the lungs it is, of course, returned with the arterialized blood into the vascular system, and through that medium introduced into the brain. The first impression upon the nervous system is that of a slight sedative; the vital functions, and more especially the nervous energy, being depressed below the standard of natural excitement, the functions of the great circulating organs are impeded. The ramifications of the sympathetic, which supply the capillary arteries of the intestines with nervous power, have their functions, in some degree, suspended, and of course the contractility of the muscular coats of such vasa vasorum must be diminished in proportion as their stimulus of irritability is withdrawn. Instead therefore of exhaling an eliminated fluid, prepared from the serum of the blood through delicate calibres, we shall have these capillary points irritated by spasmodic contractions of their circumvesting tunics—sometimes contracted and rendered impervious in their ducts by the salutary efforts of the vital powers to throw off the morbid sedative, at other times losing the resiliency of their parietes under the negative operations of the rapidly depreciating stimulus of the nervous system. The exhalants, thus relaxed, pour forth the serum of the blood, and suffer the component parts of the vital fluid to escape into the intestines. The same sedative power operates upon all the capillary organs. The excretory ducts have their excrementa completely altered in their peculiar properties. The ramifications of arteries are paralysed, and the veins, dependent in some degree upon the vis a tergo, become congested, —the heart itself becomes, ultimately, deranged in its action. To the different degrees of intensity in the operation of the sedative powers, are to be attributed all the morbid phenomena of choleroid irritations. The sympathetic vomiting and purging, the cramps in the extremities, from the depreciated energy of the spinal nerves, and all that counterbalancement of the capillary circulation, originate from the rapidly subsiding powers of the nervous system.

These opinions might lead us necessarily to infer, that infection must always ensue when the infecting power is applied to the brain. This, however, we find not to be the case; and the reason of the escape of so many individuals may be assigned to some peculiar energy of their vital powers, by which the morbific germs are either immediately decomposed or instantaneously thrown off from the system. When, however, a pestilence of so formidable a character becomes universally epidemic over large tracts of land, there are, without doubt, certain geological causes conspiring to produce effects so universally diffused. It is highly probable that from some of those grand and magnificent movements which occasionally take place in the interior of the earth, the atmosphere impendent above the surface of particular tracts of land, becomes changed in its normal properties. This change, although in many instances inappreciable by the most delicate instrument, may render the atmospherical agents peculiarly predisposed to retain and nourish the effluvial germs.

In the investigation of the causes of those fearful epidemics which occasionally strike terror and dismay from one part of the world to the other, we perceive several natural and physical causes conspiring to produce one effect? The geological condition of the unstratified materials of the primitive rocks, operated upon by unextinguishable subterranean fires, may produce a terrestial effluvium from the volatile substances contained in the bosom of the earth. This effect may in its turn prove a powerful cause in producing an alteration in the normal electricity of the atmosphere. An alteration in the electrical condition of the air may, in like manner, predispose the aerial agent to become a receptacle for the miasmatic exhalations from the surface of the earth. Thus we may have the terrestial effluvium, the anormal electricity of the atmosphere, and the miasmatic exhalations from the surface, conspiring to produce a sedative effect upon the nervous system of individuals, which may only require the specific pestilential effluvium to bring the constitution into that precise condition in which the most trivial local irritant may cause an immediate development of the epidemic. It would be a most desirable point in medical statistics to ascertain the average period that elapses between the period of personal communication with infected persons, and the development of the disease in the individual communicant. By the accumulation of a multitude of facts an average might be obtained, which would throw much light upon the propagation of the disease. In the majority of cases I find that a few days only have intervened from the death of one individual in a family to the attack of a second. In some cases the disease has manifested itself in the course of the following day, as will be observed in No. 9. of the table of the communicability of the cholera. At page 645, a remarkable instance of escape from infection is related; a woman, in a state of intoxication, having removed the dead body of her husband (who died in the blue stage) from the bed upon which he expired, and, without removing the bed-clothes, entered within the contaminated coverings, and slept therein during the night. The man died on the 19th July, and at the date of my last communication the woman remained uninfected. From the period of the death of her husband she was daily intoxicated. To form some idea of the amount of spirits she consumed, I have been informed that she spent 18*s*. in one day in this pernicious practice. I shall now relate the sequel of this case. On the 24th August I was sent for early in the morning to visit this woman, a district patient. I found her extended on the floor of the garret which she inhabited; the place presented, the most perfect picture of desolation—not a single article of furniture was in the place, the bed-posts having been sent to the pawnshop, and 4*s*. the amount lent upon them, having been spent in spirits. The woman was extended supine upon the floor, which was saturated with fluid purgations and the matter vomited. Her hands and arms presented to the touch a sensation of deadly iciness; the radial pulse was extinct; and she exhibited all the features of malignant collapse in the most intense degree. This wretched creature died at half-past one P.M. of the 24th August—precisely 36 days after her husband's death. Here then is a problem which the controversialists may resolve if they can. Did this woman receive the infection from the bed-clothes of her husband or not? If she did, were the sedative effects of the morbid poison prevented from developing themselves by the stimulus of the spirits? I have made most precise inquiries into this case. My opinion is that the stimulus of the spirit might have been withdrawn previous to her attack, on account of the woman's pecuniary inability to procure her favourite viands. The neighbours, however, state that she was tipsy daily, including the night of the attack, since the 19th July. The next question would be, Did she receive the full supply of spirits or not? this however cannot be ascertained; it may be presumed she did not, for nothing of this description was found in the house, and the neighbours state that she had not sufficient money left to pay for a glass of brandy, which they purchased for her before my arrival. The woman assured me, in the most positive terms, that she had not been near a cholera patient since her husband died. I have no reason to doubt this statement, for she would not visit the person who assisted her to remove her husband's body, although she was her intimate friend, and lived in the adjoining house. It may indeed be maintained that this individual was equally under the influence of the choleroid atmosphere with others who may have been attacked the same moment, and that the fact of her having slept in the contaminated bed may have had no relation whatever to the subsequent attack. It must, however, be remembered, that the woman who assisted her to remove her husband's body, took the disease six days after that event, and that this person passed through the cold blue stage of the pestilential fever, from which she ultimately recovered. If, therefore, there is one instance of the apparent communicability of the disease, we must allow that the two individuals *might* have received the contaminating principles from the same original source.

A few days since I was called to a district patient, who acted as nurse to persons attacked with cholera. She had that morning prepared the body of a patient, who died in the cold blue stage, for interment. This old woman had attended to the whole of the family, marked No. 8 in the table on the communicability of the fever of the pestilence. She informed me that she had been attacked twice with the disease. On the morning of the 22d of this month I saw her, in perfect health, at the bed-side of the patient: at 7, P.M. of the same day, my attendance was required at her house. When I arrived, she exhibited the following symptoms:—Cramps in the flexors of the fingers and of the legs; acute spasm, referred to the region of the colon. The eyes were injected and flushed; the tongue baked, brown, and adhesive to the touch. The pulse quick and rather full; there had been no diarrhœa, nor had the bowels been naturally relaxed. The woman was screaming loudly from the spasm of the bowels, and was much incommoded by the cramps in the extremities. I took about a pint and a half of blood from her, and prescribed a dose of calomel and opium (gr. xv. of the former to ½ gr. of the latter): she was much relieved by the treatment; the fever stage became developed, and considerable local determinations to the brain appeared in the course of the following day. The symptoms were put down in succession as they presented themselves by appropriate remedies, and in four days from the attack she was convalescent. The blood drawn from the arm was darker than the natural colour, and, upon being allowed to coagulate, presented the serum of the colour and consistence of milk, with a yellow tinge. Now, although the diarrhœa was absent in this case, yet the choleroid fever was present; and I have no doubt, that had the symptoms not been promptly extinguished, the case would have terminated differently. This case is remarkable from the almost immediate contamination of the patient. She had washed the dead body at 12 o'clock, and at 7 of the evening of the same day she was attacked herself. About the same period I was called to another district patient, who had visited a cholera case, and was seized with giddiness upon entering the room. I saw this person, a few hours after the latter event, and found her labouring under that class of symptoms which I have taken the liberty of calling the irritability of impending congestion: she had vomited repeatedly, and the cramps were commencing. I bled her to a considerable extent, and prescribed gr. xv. of calomel with gr. ½ of opium. Determination took place to the brain, which was obviated by constitutional treatment and the local application of leeches. These two cases are examples of the rapidity with which certain persons become affected by the choleroid irritation, and of the great importance of an immediate suppression of the earliest symptoms.

The three principal stages of the complaint are so well known, that I shall not dwell upon the symptoms which they present. I shall, however, occupy the remaining portion of my paper, which I perceive is already nearly filled, by making a few observations on the phenomena presented in the complex variety of the fever. I have already stated, that I consider the disease, *ab initio*, to be a masked fever of a pestilential type; and that when the cold stage is over, the true character of the malady appears. There are, however, certain points which I have not met with in the course of inquiries from books written upon the subject of cholera, which I shall briefly touch upon. In the complicated varieties of cholera, I have frequently observed anomalous sensations of a most distressing character. When the system has been exhausted by repeated purgings of the watery excrementa, a group of symptoms presents itself, which I shall class under the emphatic title of "the constitutional agony." This classification of symptoms presents a curious epitome of physiological contradictions. I have now a case in point under my care:—A young man, aged 24, was attacked on the 27th of this month with malignant cholera. His mother died on the 24th of the same month in the state of collapse. When I was called to him, the countenance had assumed a leaden hue, the dark areolæ were present, and he had purged the watery motions. Symptoms of local congestion induced me to perform venesection: I could only obtain about two ounces of dark thick blood from the arm, which afforded no relief. Powerful stimuli were then prescribed. The purgations were arrested by cretaceous mixtures and aromatics. Calomel and small doses of opium were administered. In the evening the pulse became extinct. Although the radial pulse had vanished, yet the hands were hot and dry; the tongue, in opposition to this, was cold to the touch and moist. The skin of the fingers and hands was slightly corrugated; but the most remarkable circumstance was, the development of a group of symptoms, to which I have given the name of the constitutional agony. The patient was continually tossing about from one side to the other, and from his back upon his face;—not an instant elapsed without a change of posture. He groaned deeply. The face presented marks of extreme agony. He declared that he had not the slightest pain, and that he could not tell why he was so restless. For several hours not a moment passed without the patient yawning, and the jaw was opened so wide by the convulsive gape, that I seriously apprehended the condyloid processes would have been dislocated from the glenoid cavity. On Monday evening the pulse became extinct, and the hands remained hot and dry until Tuesday, when the icy dampness took place. The radial pulsations have never appeared up to the present moment; and from Monday evening to Friday morning, this individual has existed without a pulse at the wrist. The patient has been under the influence of powerful stimuli. Counter-irritation has been applied down the spinal column, and large doses of the submurias hydrargyri have been administered. The warmth of the hands has partially returned, and inflammation has become developed in the brain: he has had five or six profuse dark-coloured motions during the night, produced by the combined operations of the calomel and castor oil. Counter-irritation is again applied to the nape of the neck, and he is taking a grain of calomel every half hour, without opium. The internal stimuli are partially suspended for the present. Nothing is more paradoxical in the medical art than the appearances of these counter-indicated indications. It indeed requires an extraordinary exertion of scientific skill to navigate such shattered barks between the whirlpool of Charybdis and the yawning abyss of Scylla.

I fear that I have extended this paper to an inconvenient length, but the great importance of the subject may, perhaps, be my best apology.

Liverpool, August 31.

SPONTANEOUS TERMINATION OF CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

Among the numerous communications on the subject of cholera, from correspondents proposing every variety of treatment, I have looked in vain for a suggestion of the possibility of the disease terminating spontaneously. To ascertain the truth of this being the mode of its termination generally, I think an object of so much importance, that I am induced again to request admission into your columns, to urge the necessity of calling attention to this point, which, if proved to be true, cannot be too emphatically made known. My proposition is, "That, in some instances, the disease continues its progress till the death of the patient; but, in the very large majority of cases, when left to nature, the disease ceases when it has produced a state of collapse, varying in intensity."

The arguments I adduce in support of this proposition are these:—

1. That under every conceivable mode and variety of treatment, there have been instances of recovery.

2. The more simple the form of treatment, the greater has been the average success.

We have records of recovery under circumstances of total deprivation of drink, when the system had probably lost half its fluids;—under stimulants, in every degree of excess; — under opium;—astringents and purgatives;—acids and alkalies;—heat and cold applied with every degree of ingenuity;—emetics, and the most anxious solicitude to allay vomiting.

Now, it is a fair inference, that under such contrary treatment, some of the modes must be injurious; yet, as there are some instances of recovery under every mode of practice, it proves, I think, the strong tendency to a natural termination of the disease, which even improper treatment cannot altogether subvert.

The second argument derives support from the host of evidence in favour of the homœopathic system — the saline treatment—the cold-water treatment—and all the more simple forms of treatment.—I remain, sir,

Your obedient servant,
John Geo. French.

St. James's Infirmary,
Sept. 19th.

[The papers which follow have been transmitted by the Central Board of Health.]

ADMINISTRATION of COLD WATER IN THE *Collapse Stage of Malignant Cholera.*

By Hardwicke Shute, M.D.

To the Medical Gentlemen of the Central Board of Health.

Gentlemen,

The following memorial was nearly completed, when the circular, requesting that the particulars of any plan of treatment which appeared to have been unusually successful in the Asiatic cholera, might be transmitted to the Central Board of Health, was put into my hands. It is intended to record the circumstances, favourable and unfavourable, which have attended the cold-water treatment, to state more fully the facts which appear most deserving of attention in the prosecution of that practice, and to suggest certain modifications, by which its success in some particular forms of the disease may probably be enhanced. As such it is, I conceive, strictly consistent with the object of the circular mentioned, and is now transmitted to the Board.

I am, Gentlemen,
Your obedient humble servant,
Hardwicke Shute.

Gloucester, September 15, 1832.

Memorial, No. 3.

From the manner in which my communications to the Board of Health, relative to the cold-water treatment of the second and third stages of the Asiatic cholera, have been received by the profession, I can have no doubt that the result of that practice in other hands, will at no distant period be made known. This great object of my former memorials being fully effected, I shall, on the present occasion, confine myself to some remarks which appear calculated to ensure a more strict adherence to the minutiæ of the plan recommended, and consequently a more satisfactory trial of its effects, and also to throw some light ultimately upon the nature of that formidable disease, with which, as professional men, we are now called upon to contend.

To those who have not had an opportunity of witnessing, and perhaps to those most conversant with the disease, it may appear rather extraordinary to speak of patients continuing in a state of collapse for 24, 36, and 48 hours, as a matter of frequent occurrence, and doubts may be entertained as to the identity of the collapse described by others, and alluded to in my former memorials; it therefore becomes necessary to state, that by the term collapse, I mean to express, the cadaverous expression and remarkable sinking of the features, which so strongly characterize the disease, accompanied by a leaden hue of the face and extremities, shrivelling of the skin, a pulse scarcely, and in most instances not at all, perceptible, and a corresponding state of the animal heat. I have seen patients continue in this state for 24, 36, 48, and even 72 hours. From a collapse of 24 and 36 hours many have recovered, some after a collapse of 48 hours, and even after a longer period, but that period has not in my own experience extended to 72 hours. Relying upon these facts, and comparing them with what I had previously witnessed, I hazarded the assertion, that in all cases of collapse the progress towards death is in a remarkable manner retarded by the cold-water treatment; and I shall take this opportunity of repeating, with the view of inculcating the importance of attending to the advice previously given, that the practitioner should be satisfied, and make not the least alteration in the plan of treatment recommended, so long as his patient is merely not getting worse. This fact, as pointing out the compatibility of ultimate recovery, with a prolonged state of collapse, may suggest to those who are pursuing a different plan of treatment, many difficulties which have arisen from an over anxiety to restore the circulation as soon as possible, and a more temperate view of the disease, by which those difficulties may be obviated. Speaking from my own experience, I should say that the most favourable and satisfactory cases of recovery have been those in which the pulse was not restored in less than 12, or 24 hours, and then as gradually as possible. I may also, upon this occasion, urge the necessity of a strict adherence, not only to the plan but the principle of the treatment—the principle of never administering any kind of stimulant which, in degree or extent, exceeds the impaired vital energy of the system. If my opinion on this subject be correct (and unsupported by the accumulated evidence of others, it does not profess to be more than opinion)—if, I say, the principle of treatment be correct, no practitioner can, in future, witness the death of his patient after the free administration of brandy, or other stimulant, in an advanced stage of collapse, and feel quite satisfied in his own mind that he has not infringed (we mean unintentionally of course) the sixth commandment.

In my second memorial I stated, that when the irritability of the stomach is excessive or impaired, the water taken with eagerness almost immediately rejected, and then again and frequently desired by the patient, reaction is established in a very great proportion of the cases; and I have now to add, that my subsequent experience, which has extended from 48 to double that number of cases, enables me to repeat the statement with increased confidence; and also the importance of not checking, by opiates or similar medicines, the vomiting, which, according to my views of the disease and its treatment, has a conservative tendency. In many instances reaction has been established, when it was necessary to rouse the patient from a state of apparent insensibility, and offer the water frequently and repeatedly, when the water was taken, if not reluctantly, without any evident satisfaction, and therefore the treatment is not at all confined to the former cases; but I felt myself called upon, in my second memorial, to direct the attention of the profession to those cases more particularly, in which I felt assured that the cold-water treatment would lead to the most striking effects, and be attended with the same beneficial results as I had witnessed in my own practice, leaving it to others to ascertain how far the treatment might be applicable to a different form of the disease.

I also stated, that when the natural powers of the constitution are not unusually weak from extremes of age, or other causes (and of these causes previous habits of intoxication and inadequate support are the most prominent), such reaction will be followed by an early convalescence without fever, or the assistance of any kind of medicine. This statement, which has also been confirmed by my subsequent experience, is in itself an acknowledgment that recovery has not always followed reaction; and I shall now state some of the difficulties with which, in the less favourable cases, the practitioner will have to contend, and the reflections to which these cases have given rise, it being distinctly understood that I rely upon the cold water alone in all cases as the agent by which reaction is to be established, and in the more favourable cases as the sole agent by which convalescence is ultimately established.

In the most favourable cases, reaction is accompanied by an improved state of the alvine excretions, a cessation of vomiting and purging, a disposition to sleep, a restoration of the urinary secretions, and an uninterrupted progress towards a healthy state of the constitution. In others the return to health, after the pulse has been restored, is not progressive, and the alvine excretions will, upon examination, be found of an unhealthy character, and sometimes partaking, more or less, of the morbid state so peculiar to the disease. If the bowels are costive (by no means an unusual circumstance), the practitioner will seldom be wrong in suspecting a diseased state of the excretions. I have usually given ten grains of the pil. hydrarg., or of the hydrarg. c. creta, with or without castor oil, according to circumstances, and the advance towards convalescence has been subsequently uninterrupted. The patient, however, is most commonly in a state to which the ordinary principles of medical practice are applicable, and therefore the selection of the medicines employed may be left to the judgment of the practitioner, but with the precaution of avoiding such doses as may be too powerful in their operation. The necessity of this precaution arises from the singular fact, and the fact must never be lost sight of, that the constitutional powers seem to be more under the influence of debilitating causes after the pulse has been restored, than it was during the collapse. The same, or a minor degree of muscular exertion, which was apparently innocuous during the collapse, has, in three instances, brought on a relapse, and a relapse is, I am sorry to add, almost always fatal. If purging continue to an immoderate extent, the administration of one grain of the argentum nitratum has proved highly beneficial. In some instances the vomiting has been obstinate after reaction has been established, but this has generally happened when, at the commencement of the collapse, there was no vomiting at all. In more than one instance the urinary secretion was not restored, but my experience hitherto does not enable me to offer any particular observation upon these cases.

The next circumstance which I deem worthy of particular notice, is a tendency to local congestion, and chiefly of the brain. Vascular congestion of the conjunctiva is a frequent occurrence, but has not in itself appeared to require any particular treatment. The case, however, is sometimes otherwise, and in infants and young children more particularly, such congestion being attended with symptoms of pressure upon the brain. As connected with this subject I may also mention the occurrence of opisthotonos in two infants and one female.

This tendency to congestion, after the restoration of the pulse, naturally suggests the question, whether bleeding would in any way assist the cold-water treatment, and at what period of the disease, if any, it should be adopted. From the present state of the disease in Gloucester (there not being a single case in the hospital), there is little probability of my being able to determine the point, and therefore I must throw myself on the experience of others, merely suggesting that the period of the complaint I should myself select for the experiment, would be that when the cold-water treatment had been persevered in long enough to justify the presumption that the fluidity of the blood had in some degree been restored. In one case a small quantity of blood, which was at first dark and venous, and in half an hour afterwards more florid than usual, was taken at this period of the disease. The pulse was restored at an earlier period than I had observed in other cases, but in the course of the night the patient became restless and irritable, was allowed to get out of bed frequently, had a relapse and died, confirming, as it appeared to me, my former impression, that a very gradual restoration of the pulse was an essential part of the cold-water treatment. I may here mention that in two cases this restlessness and irritability amounted, if not to maniacal aberration, to maniacal impatience and obstinacy, not very different perhaps to the state of mind which is said to occur in extreme exhaustion from exposure to excessive cold or starvation.

The occurrence of opisthotohos in the cases mentioned, naturally suggests the idea that the cold-water treatment might under certain circumstances be rendered more efficacious by the application of stimulants to the spinal canal. The cases recorded by Mr. Whiting are, in this point of view, highly interesting, but when the question is asked how far the application of blisters to the whole course of the spine may be regarded as essential to the cold-water treatment, I can have no hesitation in saying that it is not, inasmuch as the blistering plaisters without the water are wholly inefficacious, whilst the cold water is, in a great majority of the cases, a powerful and efficient agent without their assistance. From the state of the circulation on the surface during the collapse, the effect of the blisters must at that time, I conceive, be trifling and unimportant, but I think it highly probable that in some cases, in infants and children perhaps more particularly, their operation, when the circulation begins to return to the surface, may prove extremely beneficial, but were they had recourse to in all cases, and considered as constituting an important part of the cold-water treatment, consecutive fever would, I fear, be a much more frequent occurrence.

I shall close this part of my subject by stating briefly two general principles which ought to regulate our practice after the pulse and animal heat have been restored; the patient must be considered as having passed from a state of depression to that of exhaustion, and the fundamental principle of not allowing any kind of stimulant which is disproportioned to that state, must be strictly adhered to and enforced.

This candid statement of the difficulties with which, in the less favourable cases, I have had to contend, may possibly convey the idea that my confidence in the practice is not what it was, and that farther experience has not confirmed my former statement. It is quite sufficient to say that such is not my own impression, or the impression which this statement, if read with attention, and due allowance be made for the number of patients in which a practice directly opposed to my own principles had been pursued previous to their admission into the hospital, ought to convey to the minds of others. It is wholly unnecessary to say more upon this subject, because the value of the treatment recommended, must obviously be determined by the experience of others, and not by the representations of an individual. That experience, as far as it has hitherto come to my knowledge, is greatly in favour of the practice.

There is one circumstance connected with the pathology of the disease, and which, from the protracted duration of the collapsed state under the cold-water treatment, I may have had more opportunities of witnessing than other practitioners, highly deserving of mature consideration. The circumstance to which I allude, and which constitutes a distinguishing feature, if not pathognomonie symptoms of the Asiatic cholera, is a marked want of correspondence between the state of the circulation, and the functions of the brain. The fact of the mental powers being comparatively unimpaired, was noticed soon after the first appearance of the disease, but the remark has not, in my opinion, been sufficiently extended to the powers of voluntary motion as connected with the brain, and to the powers of the muscles themselves. The continued and apparently undiminished sensibility of the nerves of taste has been strikingly exemplified in a great majority of the cases which have come under my notice. At a period of the disease when the circulation is to all appearance arrested—when not more than a tea-spoonful of blood escapes from a vein which has been compressed and opened, and that blood is in a state little capable, according to our previous notions, (and these notions are probably correct,) of supporting any function — when the body feels as cold, I may say colder, than any inanimate object in its vicinity, the patient will take with eagerness repeated draughts of cold water, and reject with the utmost pertinacity every other kind of fluid. In one remarkable instance, when the collapse had been confirmed, and the pulse imperceptible for at least 48 hours, the patient complained of her bed, which contained no material more yielding and accommodating than straw, being very hard and uncomfortable. Under precisely similar circumstances the patients will turn themselves in bed, respire deeply and regularly, swallow without the least difficulty, and, if occasion require, get out of and return to bed without the least assistance. In one remarkable case of this kind a female succeeded in getting out of bed in opposition to all the resistance which three nurses could offer. The Asiatic cholera differs in this marked peculiarity from asphyxia, and indeed from any disease which has hitherto been described. The "entasia acrotismus" of Dr. Good approaches nearer to it than any other. This want of correspondence between the state of the circulation and the functions of the brain, as far as sensation, volition, mental action in a higher grade of perfection, and the powers of the voluntary muscles are concerned, demands, on the part of the profession, the most mature and unprejudiced, may I add unbigoted, consideration. It is a fact, which strikes at the very root, and saps the foundation, of the physiological principles most commonly adopted, and if of the physiological principles on which the practice of medicine is based, of medicine as a science, and will, in my opinion, lead ultimately to a greater revolution in that science than is at present anticipated.

The opinion of the profession as to the originality of my views of the disease termed Asiatic cholera and its treatment, is a matter of very secondary importance if put in competition with their correctness and consequent utility; and yet, in justice to myself, I cannot omit throwing out a few suggestions on this subject. In a comparative view of the different modes of treatment it will now be necessary to distinguish between the effects (I allude more particularly to the saline and antimonial treatment) of the medicines themselves, and of the water in which they are conveyed, or which is at the same time allowed. In having directed the attention of the profession to the agency of the water itself, I have materially deviated, as far as I know, from the course of other writers. There is a material difference, I may observe, between not refusing cold water, and urging its use as the agent by which reaction is to be established; between not opposing the desires of nature, and reconciling those desires with a conservative object, and with a long-acknowledged principle of therapeutics. That the vomiting is a conservative action, and as such should not be officiously interfered with or interrupted, is also an important, and, I believe, a peculiar item in the detail of the cold-water practice.

P.S. This memorial being explanatory of those already communicated to the Central Board, and published in the Lancet and MEDICAL GAZETTE, I shall feel obliged by its being transmitted to the editors of those publications.

CALOMEL IN THE STAGE OF BILIOUS DIARRHŒA.—MERCURIAL INUNCTION IN THE STAGE OF COLLAPSE.

To the Secretary of the Central Board of Health.

SIR,

I HAVE the honour to forward a short account of my method of treatment of the epidemic, in obedience to the circulars (No. 1. and 2,) addressed to the profession by the Central Board of Health, Sept. 3, 1832. It may not be improper to observe that, having served in India between six and seven years, where I treated the epidemic on a more extensive scale, it may be possible that I have approached the bed-sides of the sufferers, if not with more confidence, at least with more calmness, and with a degree more of what may be termed rational medicine, than those altogether unacquainted with the disease. I fear that what I have called "rational medicine" has been altogether lost sight of during the panic, which has attacked even medical men at the onset of this most formidable disease. All our hardly-earned pathological, physiological, anatomical and therapeutic experience, has apparently been laid altogether aside in order to seek for new theories, new *modi medendi*, which have been for a time blazoned forth to the public, only to suffer defeat, nay more, by their failure they have increased the dread (already too great) and aggravated unnecessarily the feelings of the public respecting this vampire in the shape of disease. It would be exceedingly ill timed, at this period, to allude to measures which have rendered altogether abortive the wisdom, foresight, and competency of the sanitary regulations of the Central or Government Board of Health; with their permission and indulgence, however, at a fit opportunity, I have it in my power to prove that their measures have been in some instances altogether disregarded. To return to my immediate object—the Treatment of Cholera.

1st. When in the form of bilious diarrhœa.—In this stage, avoiding as much as possible all known lædentia, and employing in an equal degree all known juvantia, I have trusted entirely to the conf. opii, in doses gr. xxxvj. aquæ pimentæ ℥j. to adults, repeated at well timed intervals, watching the patient attentively. The success attendant on this treatment has been most signal.

2d. In that of rice-water evacuations.—In conjunction with the above I have employed doses of calomel and camphor, gr. iij. of each.

3d. In the stage of collapse—Here I have relied, *cæteris paribus*, entirely on full doses (℈j.) of calomel with moderate quantities (gr. j. or gr. ½) of opium, MERCURIAL INUNCTION, and calomel and camphor gr. iij. of each every half hour, or every second or third hour, combining with each dose as reaction became established one grain of pulv. calumbæ. The last case of malignant cholera (this day fortnight) is truly singular and worthy of a place here:—

Jane Fayrer, aged 52, residing at No. 3, Three King's Court, Whitecross-Street, of spare habit, had been ill several days with disordered bowels; on visiting her I found as bad a case of malignant cholera as ever I witnessed. I did not think her capable of bearing calomel at all. I therefore gave her gr. xxxvj. conf. opii, in ʒ vj. of aqua pimentæ. This was in part returned; she however experienced some relief. I then sent her the following—

> Camphoræ ℈j. Pulv. Opii gr. j. Calumbæ gr. vj. M. et divide in pulveres sex; one to be taken every half hour.

This was all the medicine she had. In the evening when I saw her reaction was perfectly established. It continued. She was left exceedingly weak: however, with the aid of the tinct. calumbæ, which I consider highly valuable, she has at length begun to resume her usual domestic duties. I did not expect this woman would survive six hours.

It has been my object to simplify, and at the same time to render effectual, as far as possible, my treatment of the disease, and I can safely say this method has been attended with, *at least*, as much success as any other with which I am acquainted. Viewing the disease, and I do, as one in which the nervous system is primarily and principally affected, I am inclined to place much reliance on moderate (not excessive) and well timed doses of opium, combined with calomel in the stage of collapse. Perhaps somewhat of the success of this plan may be attributed to climate and to the habits of life of the majority of those who have fallen under my care. I consider that the case of Jane Fayrer establishes the well-known caprice of the epidemic in a most extraordinary manner.

I have the honour to be, sir,
Your most obedient servant,
EDWARD HICKMAN,
Late Assistant Surgeon
Honourable Company's Bengal
Establishment.

No. 47, Chiswell-Street,
Sept. 21, 1832.

CALOMEL, GINGER, AND OPIUM, FOLLOWED BY A PURGATIVE—COPIOUS ENEMATA—FRESH AIR, &c.

To the Secretary of the Central Board of Health.

Newcastle, Sept. 19, 1832.

SIR,

I SHALL endeavour to answer the several queries contained in the circular which has reached me from the Central Board of Health with clearness and brevity.

In conducting the treatment of cholera, in all its grades and stages, I have always endeavoured to form a clear conception of the objects to be held in view, that every remedy might be prescribed with a distinct and definitive purpose. In the treatise which I published a few months ago, and which I directed to be forwarded to the Central Board, I endeavoured to explain the rationale of the disease, and the indications of cure which were then, and, with a few modifications, have since been my guides. It is now far from my intention to boast of success—hitherto, I fear, few practitioners have had cause to do so, whatever plan of treatment they may have pursued—but it does not appear that any plan has had a very decided advantage over that which I have pointed out, since the relative mortality has not been any where greatly lessened. It is true that the saline treatment, as recommended by Dr. Stevens, seems, in his own hands, to have been very successful, but, without meaning to throw doubt upon the accuracy of his statements, its efficiency requires to be confirmed by the experience of others. But to revert to the queries which I am required to reply to:—

1. The bilious diarrhœa, which so frequently constitutes the early stage of cholera (*premonitory* is an erroneous term, it is not a premonition of the disease, but a catenation of symptoms constituting the early period of the disease itself) is to be considered as an irritation of the mucous membrane of the bowels, occasioned by the poisonous agency by which cholera is produced. The process of digestion is interrupted, and irritating undigested matter remains in the primæ viæ. Our indications must be to relieve the irritation and expel the irritating matter. Four or five grains of calomel, two of ginger, and one of opium, will very generally answer the first indication, and a dose of castor oil, or other purgative, given an hour or two afterwards, will frequently complete the cure. It frequently happens that this form of the disease is accompanied with headache, and pain or tenderness at the scrob. cord. the pulse being full and quickened; in cases of this description venesection will be found of service. Occasionally the undue action of the bowels continues, the stools being bilious and feculent, and no other symptom of disease remaining. One or two doses of mist. cretæ. with five or ten drops of t. opii, will frequently check a purging of this description altogether.

2. When the discharges have assumed the appearance of rice-water, we must aim at relieving the irritation, checking the discharges, and restoring the biliary and urinary secretions. Rest, calomel and opium, as above, a smaller dose being given at intervals, the opium either being omitted or reduced to a small quantity, *e. g.* calomel gr. ii. p. zingib gr. ii. opii gr. ¼, every hour or two hours, according to the urgency of the symptoms; when the discharges have been entirely restrained the opium should be omitted, and the calomel continued till bile and urine are secreted, often till the patient's gums become affected. Purgatives must also be given to obtain natural excretions. The best article of diet I have found, in these cases, to be gruel salted, of which some patients have drank enormous quantities with apparent benefit. Saline draughts, with an excess of alkali, have also been grateful and useful to the patient. Stimulants of every description injurious. The head and abdomen require close watching, and any symptoms indicating disease in either region, must be promptly attended to. Leeches or general bleeding must be employed on the first appearance of uneasiness. The latter agent must, however, be used with great caution. Some patients who have never fallen into the stage of collapse, have nevertheless ultimately sunk in the febrile stage, with head disease of an obscure character, beginning with slight giddiness and scarcely appreciable uneasiness, but steadily proceeding to coma, delirium, and death; the tongue becoming dry and black, and the patient exhibiting altogether the worst form of typhus fever. By close and early attention, the fatal tendency of the head affection has been successfully grappled with.

3. The stage of collapse I have found always difficult to conduct upon any well-established principles. When well-marked, I fear we can scarcely hope for any large proportion of recoveries, whatever plan is adopted. Still it ought not to be treated at random. Brandy and other stimulants appear to me uniformly to increase the gastric irritation. The calomel and opium, as before recommended, appear to allay that irrita-

tion, as well as to be useful in answering the indications already noticed. The salted gruel is often a very grateful drink: the temperature of this, or any other beverage, may be safely accommodated to the feelings of the patient. Copious injections into the rectum I have so frequently found useful, that I feel no hesitation in recommending their general employment in collapse. Emetics are of more doubtful utility than I was formerly led to believe, unless when crudities are known to remain in the stomach. On the whole, my main reliance for simply restoring vascular action is placed on copious drinks and copious injections, quietude, fresh air, and comfortable cloathing. When patients rally from collapse, it is often most difficult to ascertain in what causes, their emergence from it has depended. I fear various remedies have often obtained the credit which has been due to the spontaneous efforts of nature.

From the preceding remarks it will be seen that the remedial agents which I have employed in cholera (and I believe my practice to have been attended with a full average share of success), have been such as the profession is familiar with, and that their employment has been conducted on established therapeutic principles. It is in this way only, I believe, that any considerable improvement can be effected in the treatment of this destructive malady, and not by experiments which are entered into without any distinct anticipation of their possible effects. I apprehend it is hopeless to expect a specific effect to be produced in cholera by any remedy whatever: in the stage of collapse, the condition of the stomach and bowels renders nearly inert the most powerful agents we can employ, and I am disposed to think they are more sensible to the stimulus of distention than any other, while it is least likely to occasion ulterior injurious results. Hence it is, probably, that large quantities of fluid introduced into the stomach and intestines are often found unequivocally beneficial. I am not certain whether it is really material to impregnate the fluids employed with saline and alkaline matters; but while agreeable to the patient, I am in the habit of doing so—when they cease to be grateful, they are omitted.

The late irruption of cholera in Newcastle has differed, in some of its features, from the former one; more especially in the frequency with which fever, of a bad form, has succeeded, even where the collapse has been incomplete—in the great difficulty of restoring the secretion of bile in cases of rice-water diarrhœa, and in the disease having shewn itself amongst the middling and higher classes of the community. Derangement of the stomach and bowels, diarrhœas, dysenteries, and cholera in its various forms and degrees, have been almost universal in the neighbourhood, shewing how general is the cause by which they have been produced.—I am, sir,

Your obedient servant,

T. M. GREENHOW.

CHLORIDE OF POTASS—COMPOUND TINCTURE OF CAMPHOR.

Douglas, Isle of Man, Sept. 19, 1832.

SIR,

IN compliance with the Central Board's inquiry (No. 2.) we beg leave to submit the following, as the most effectual consistent with our practice.

1st. When in the form of bilious diarrhœa, small and frequent doses of calomel, opium, and ipecacuan, in combination with chalk mixture, &c. &c.; which seems in general to have arrested the progress of the disease.

2d. In that of rice-water evacuations, blood-letting, pediluvia, and the frequent administration of calomel, antimony, and opium. We have also tried the use of croton-oil with apparent advantage, which seems to produce its effect by causing a transfer of action, and consequently changing the state of the serous evacuations.

3d. In the stage of collapse. When this stage of the disease has occurred, we have in general had recourse to the chloride of potass, in combination with soda and compound tincture of camphor, accompanied with stimulants of different kinds. Latterly we have tried cold water, according to Dr. Shute's system, and although a few recoveries have taken place under this treatment, yet we have reason to believe, that when persevered in too far, it induces very high consecutive fever, and consequent congestion of the brain.

GARRETT AND HARRISON,

Surgeons.

FURTHER OBSERVATIONS ON THE BLOOD.

By Dr. Stevens.

To the Editor of the London Medical Gazette.

Sir,

A knowledge of the cause of the red colour of the blood is not a matter of mere curiosity; it is of great value to the physiologist, and of vital importance to the practical physician. It is now well known, for example, that the blood is both black and morbid in the last stage, not only of cholera, but in all other malignant epidemics: and therefore to restore its red colour and concomitant soundness, in such diseases, is, or ought to be, the chief aim of every rational practitioner. Most will admit this, and few will deny that physicians acquainted with the true cause of the redness of the blood in health, and its blackness in disease, will be more likely to accomplish a cure than such as are governed by hypotheses, which are not only in direct opposition to facts now well ascertained, but which have led to a practice of the most injurious tendency.

From the result of certain experiments, I could not help believing that it was not iron, but the natural saline matter of the blood which was the chief cause of its red colour. As one proof of this, I stated that when the blood was allowed to coagulate out of the body, the crassamentum became black in the centre in proportion as the *salt* serum left the coagulum and rose to the surface. This has been denied by some Irish experimentors, but the most perverse hypocriticism cannot disprove a fact which can easily be verified in any hospital.

In about twenty-four hours after coagulation, the greater part of the serum has separated from the clot; but the crassamentum cannot be entirely freed from its saline serum, either by this natural separation or by subsequent immersion in distilled water. But even the pretenders to more minute experiments admit that four-fifths of the saline matter are thus spontaneously abstracted from contact with the red globules; for they assign about three parts in every thousand as the residuary saline matter of the black clot, instead of fifteen, which are known to exist in the serum. Now because three parts of salt will not redden one thousand parts of blood, is it rational to infer that fifteen will not? Every chemist knows that turmeric-paper, a common test for alkaline matter, is not affected by it, unless in a certain concentration; so that the liquid which lessens the red of litmus does not change the bright yellow of turmeric-paper; and as the effect of the salt on the colouring matter depends on the quantity, perhaps it may strike some of your readers that as fifteen parts to the thousand are necessary to give the red colour to the blood of a healthy person, we have then no right to expect any thing like redness in a clot, where there is only three parts of saline matter in place of fifteen, which is about the quantity, I believe, invariably found in the serum of healthy blood.

While the colouring matter is in contact with the salt serum, as in the circulating current, it remains red; but after the blood is drawn from the veins, the clot becomes perfectly black only when the serum has almost entirely left the coagulum. When we cut out a piece of the red clot, before the separation, and suspend it in the air, at first it becomes more red, chiefly, as I believe, from the sudden removal of the carbonic acid; but in proportion as the salt serum drains out of it, the colouring matter becomes perfectly black, even though it remain *in immediate and constant contact with the oxygen of the atmosphere*; but neither air nor pure oxygen can redden the colour of this black clot. The redness can be permanently restored only by the addition of a certain quantity of saline matter. And if any one who has a doubt upon this subject will add the difference betwixt three and fifteen parts of salt, or immerse the black clot in water that contains twelve parts of saline matter to one thousand, he will then find that the black clot almost immediately changes its colour, and becomes as red as it had been previously to the drainage of its saline serum. It has also been proved that an extra addition of saline matter will not only strike a full red, but a hue that is highly arterial: and as this change can be effected *even in an atmosphere of carbonic acid*, can any one hesitate to admit that it is the salts of the serum, and neither iron nor aërial oxygen, which is the chief agent in striking the red colour with the colouring matter? Hence, though we admit that a clot which has been immersed in distilled water may still contain a small quantity of serum, yet this only proves that the portion of salt in the black clot is too minute in quantity to produce any evident redness.

When we immerse a black clot in a saline fluid, it instantly changes from black to scarlet; but when we immerse a red clot in distilled water, it soon changes from red to black. The reviewer of my work, in one of your contemporaries, admits this; but denies positively that the water takes up even one particle of the salt: and this he proves by a careful experiment, about which, he affirms, there can be no deception. The red crassamentum, however, contains a saline fluid, which oozes out of it while immersed in the water; and if the two be well mixed together, as they ought to be, it is impossible that a salt fluid can be agitated in the same vessel with distilled water without the water abstracting a part of the saline ingredients. Is it not known, for example, to every cook, that oversalted meat can be rendered almost fresh by immersion in fresh water, which becomes proportionally saline? And will the critic pretend to prove by an accurate experiment that there is as much salt in the meat after immersion as there had been before? Yet he asserts as much, in reference to the crassamentum of the blood, in which the salt is more loosely combined. Few can be misled, however, by such sophistry; and, for my own part, I can place no confidence whatever in experiments which are in direct opposition to common observation: for the critic might, with the same propriety, attempt to prove by experiment that the river water which runs into the sea does not become impregnated with saline matter, as to say that distilled water does not take up a part of the salts when it becomes mixed with the salt serum of the blood: and if it does, the clot from which the serum has oozed out must naturally contain less saline matter, and of course become blacker than it had been before. In fact, any one who takes the trouble to make this simple experiment, will find that distilled water, in which the red clot had been immersed, acquires a notable proportion of the alkaline salts: and as this must be derived from the serum which oozes out from the crassamentum, it is very evident that the black clot also must contain less salt after immersion than it had done before.

It has been asserted that serum cannot redden the blood; but this is in direct opposition to facts. When we divide a clot that has been coagulated for twenty-four hours, we find that it is all perfectly black, except the thin film on the upper surface, which has been covered with serum. When we remove the one half of this clot, and put it aside, the black surface does not become red, even when exposed directly to the air; whilst the other black surface which remains on the serum, becomes not only red, but as florid or arterial as the upper surface.

The objections to which I have referred are so preposterous that they would scarcely deceive even a tyro; but a more plausible cavil remains to be considered. I have stated that carbonic acid is the chief cause of the dark colour of the venous blood, and that oxygen cannot directly redden the blood except through the medium of its saline ingredients; but that it indirectly reddens the colour, chiefly by its peculiar property of abstracting the carbonic acid, which is the true darkening agent. It has been attempted to disprove this proposition by an experiment, which is at the same time very simple, *and about which there can be no mistake.* A *black clot* is taken and immersed in distilled water; a part of the carbonic acid is then removed by means of an air pump; and finding that the black clot does not become red by removing the acid, it is at once concluded, *per saltem*, that carbonic acid cannot be the cause of the dark colour. It ought to be recollected, however, that, in this instance, the clot was black previous to the experiment—and why? merely because it did not contain a sufficient quantity of salt to strike a red colour with the hæmatosine; and for this obvious reason the mere removal of the carbonic acid cannot produce redness in the clot, particularly when immersed in distilled water. This experiment, therefore, is only a proof of how little its contriver is acquainted with this subject, and so far from "destroying the new theory," it serves to confirm and sustain it. In fact, it was altogether gratuitous; for had the individual who has performed it condescended to read my work, with common attention, he would have found it proved, in more places than one, that the mere removal of carbonic acid cannot produce any thing like redness in a black clot, or in black blood, unless there is a sufficient quantity of saline matter in immediate contact with the hematosine to strike the red colour with this substance, when the acid is removed: and this surely is a sufficient proof that oxygen alone is not the sole cause of the red or arterial colour.

It has long been ascertained, that blood which is drawn from the veins into an exhausted flask, does not become red. This is probably because the carbonic acid remains in contact with the colouring matter; but the moment that this dark blood is exposed to the air, the acid is rapidly removed from its upper surface by the atmospheric oxygen, and the colour changes in the same way that it does in the lungs. But how is the venous blood to become red in an exhausted flask, if the darkening acid be not removed, or if the gas which rises be still in immediate contact with the colouring matter? yet this experiment is considered as affording proof positive that the salt is not the cause of the redness, nor the carbonic acid of the purple hue.

When we agitate a small quantity of venous blood in a phial of atmospheric air, it becomes beautifully florid; but, when we make the same experiment with a small quantity of arterial blood and a large volume of *pure* carbonic acid, the colour darkens into a venous tinge. This latter experiment was first made by Dr. Priestley, who ascertained that this was the fact. Care, however, must be taken to exclude all communication with the external air. When this condition is observed, and when the blood and the acid are sufficiently agitated together, the colour changes from a florid to a purple red. In fact, as every one of the acids blackens the blood, exactly in proportion to its neutralising power, it would be rather strange if the carbonic, which ranks so high in this respect, were to form an exception to this rule.

It is asserted in the last number of the Dublin Journal, that even a black clot from which the serum had completely drained out, became red again when it was cut open and exposed to the air. This may be the case with Irish blood, and in the atmosphere of the sister kingdom, but when the experiment is fairly made on British blood, in the air of England, it does not exhibit this phenomenon. I have already remarked, that the whole serum is never extruded out of a clot by the force of spontaneous coagulation; but that much still adheres, particularly where the mass is large and the drainage imperfect. To make this experiment fairly, it is necessary that the clot should be *very small.* When such a clot is immersed in distilled water, the saline serum flows out of it very readily into the lighter fluid, and after a short period the clot becomes perfectly black. The Irish experiment, however, was made with a coagulum of bullock's blood, which weighed no less than *four or five pounds.* This huge mass was exposed to the air. The external part of it would necessarily become somewhat indurated in a dry atmosphere, and thus impede the free egress of the serum. This experiment, therefore, so far from giving a death-blow to the new theory, is only another proof in its favour; for in such a large coagulum there is evidently enough of the serum left to redden the blood when the acid is removed by the oxygen of the air. Yet this inaccurate and clumsy experiment is brought forward for the purpose of removing the very key-stone of the arch which has lately been erected, and the engineer is only waiting at a respectful distance to witness the entire destruction of the whole edifice. Certainly he will be disappointed, unless he brings forward a different sort of artillery from that which has hitherto been tried with so little effect.

It has not yet been proved that the addition of oxygen reddens the blood, but from the facts that have been stated, it is very evident that though oxygen may have a direct as well as an indirect share in the change of colour, yet it is equally certain that the salts are the medium through which this change is effected; for oxygen without salt cannot redden the colouring matter, still salts without oxygen immediately give an arterial colour to the blackest blood; and though we cannot redden the black blood in cholera by any other means, yet the healthy colour can easily be restored, by the judicious use of the neutral salts; and the knowledge of this practical fact is certainly of considerable importance to every medical practitioner.

Practical results are the best test of the real value of a medical doctrine: for, "such is the close connexion betwixt theory and practice, that good or bad they invariably keep pace with each other." And pray let me ask, what have we gained in practice by the doctrine that oxygen is the sole cause of the red colour of the blood? has it lessened the mortality of malignant epidemics?—not in a solitary instance. Can we remove or destroy the poison of cholera, or give redness and healthiness to the black and morbid blood in the last stage of that disease, by the mere inhalation of oxygen gas, or can we redden this black blood by the internal use of those acids which contain oxygen?—most assuredly not: for these agents blacken even the arterial blood out of the body, and there is no want of evidence to prove that this same prepossession about oxygen and acids has led to a more destructive practice than any other ignis-fatuus creed of the medical profession.

It would be idle to follow the writers on the other side, through the labyrinth of error, wherewith they have attempted to mystify the ignorant. Most of their errors are sufficiently obvious to common sense; there is, however, one mistake which may have a fatal influence over the inexperienced mind, and on which, therefore, it may be proper to animadvert: for the inference which a recent reviewer draws from the experiments of Dr. O'Shaughnessy, is fraught with dangers no less imminent, than certain printed instructions for the treatment of cholera which are now doing so much mischief all over the country.

We are informed that Dr. O'Shaughnessy has analysed the blood previous to the attack of cholera, and finds it to contain its usual quantity of water, salts, colouring matter, albumen, and fibrin; and therefore he infers that the blood cannot be diseased either before the attack or in the first stage. For faith in such a conclusion, a much larger body of evidence than has yet been adduced, would be desirable; nor will any one admit, that after the discharges of the premonitory stage, the blood remains unchanged, as in health, or that the quantity of salts which is found in the dejections is not derived from the blood, and must consequently lessen the quantity of its saline matter. Yet, granting for a moment that the experiments to which he refers are correct, is he entitled to affirm that the blood is not morbid; or is it a proof that nothing has been added merely because nothing has been taken away? But it is very clear that the blood may be injured by addition as well as by subtraction; and the mere fact that it contains its usual proportion of chemical compounds is no proof whatever that a poison has not been added, or that this noxious agent has not lessened the vitality of the blood as well as of the solids. When we analyse the air that broods over a pestiferous swamp, we find that it contains its usual proportion of oxygen, nitrogen, and carbonic acid; but are we to conclude from this that the air must be wholesome because it has not lost one particle of its usual chemical constituents; or when we see its effects in producing fever, are we to deny that this air does not contain a poison, merely because the chemist has not yet discovered any test which can enable him to prove its existence? Have we any test for the poison of small-pox? —but those who deny its existence either in the air or the blood, on such reasoning as this, might tell us, with equal propriety, that small-pox is a delusion, or that the aërial poisons are totally incapable of producing disease.

It is admitted by those best acquainted with the subject, that animal chemistry is still very imperfect; and are we to deny the evidence of our own senses in favour of facts, merely because an experimentalist says he cannot confirm them by tangible proofs? In the month of June last, I selected three of the nurses in the House of Correction at Cold-Bath-Fields prison, who had been longest in immediate contact with the sick: they were all of them apparently perfectly well at the time, and attending to their duties. About six ounces of blood were drawn from each, by venesection, and in every one of them the blood was not merely darker in colour, and of an aspect unlike that of health, but it was especially remarkable, that when their blood was exposed to the air it assumed the blue appearance peculiar to the skin in the last stage of the Asiatic cholera, instead of becoming florid. After the blood had been for some time exposed to the atmosphere, the serum became perfectly clear; but a part of the matter which occasioned the blueness was precipitated and adhered to the inner side of the cup. Now, admitting that this blood contained its common proportion of the usual ingredients, no one will contend that it was not diseased, or doubt the evidence of his own senses, merely because there are no chemical tests to prove the existence of the poison, or, as yet, any analytical account of the agent which forms the blue precipitate.

An extra portion of blood was drawn from one of the above nurses, and a small quantity of carbonate of soda was mixed with this dark blood while it was still warm and fluid; but in place of becoming blue, when exposed to the air it became beautifully florid. and we now know well, that when the alkaline salts are used internally, they act upon the blood and redden its colour in the same way that they effect this purpose out of the body.

It has been supposed by some, that the saline treatment was adopted in malignant diseases on account of the deficiency of the natural saline matter of the blood. This, however, is an error; for the treatment was commenced on practical grounds, before I was aware of this fact; and it was then just as successful in the West Indies as it has been since in other places, wherever this treatment has been fairly tried.

When the blood is healthy, its natural quantity of saline matter is quite sufficient to give it the red colour, to enable it to stimulate the heart, and to perform its healthy functions; but when the vital fluid is under the influence of a narcotic poison, it requires an extra portion of saline ingredients to enable it to resist the effects of any deleterious agents—such as the poison which is the cause of cholera. The salts, however, not only brighten the colour of the black or diseased blood, but they are powerful antiseptics, and some of them, at least, appear to possess the power of neutralising the aerial poisons, or counteracting their agency on the living body; consequently, when given in excess, they increase the secretions and prevent or remove the morbid predisposition, not only in the circulating current, but in the whole system. It is for this reason that their administration is so essentially useful in the first stage of the Asiatic cholera; and when we see that out of fifty patients who are thus treated in the first stage, not more than one sinks into a state of dangerous prostration, and know that similar cases in the same districts, which are treated with opium, stimulants, &c. fall rapidly into a state of mortal collapse; are we then, in the face of such facts, to use opium in place of the saline remedies, even in the commencement, and sacrifice our patients to a vague hypothesis founded on delusive experiments, unhappily pointing to a practice which is well known to be deplorably unsuccessful?

No one who advances doctrines directly opposed to common belief, can reasonably expect that they will fail to excite opposition, if not hostility Such was the lot of the immortal Harvey, and how can others hope to escape it?—My chief object in coming to England was to lay an outline of my views before the public. This I have partly, but I admit very imperfectly, done in my work "On the Blood." I am aware that it contains more errors than one; but being now on the eve of embarking for another country, I must leave it to its fate. I trust, however, that the intelligent and impartial members of our profession will not look merely to its faults, but consider it as a whole, and then, when the influence of their natural repugnance to innovation shall have passed away, they may form what estimate they will of its merits.

I am, sir,
Your obedient servant,
WM. STEVENS, M.D.

Since the above was written, a letter has appeared in the Times, in which it is asserted that the new theory of respiration is "altogether erroneous." This assertion is made on the authority of an experiment something similar, but evidently as ill conducted, as the one with the black clot. There are circumstances, however, connected with the said experiment which render the conclusion which has been drawn from it of no value. When the atmospheric pressure is removed, the serum of the blood in vacuo does not rise to the surface nearly so fast as it does when a similar quantity of the same fluid is exposed to the air; but when the serum, which contains the reddening agent, does rise to the surface, the blood in vacuo becomes quite as florid as if it had been exposed directly to the air. Sir Charles Scudamore is brought forward as an evidence against the new theory; but he might with equal propriety have been brought forward to prove that the muriate of soda blackens the blood. Mr. Prater is also referred to, as a new and impartial evidence in favour of the old hypothesis; but the writer forgets to state that Mr. Prater is, in the outline, not only favourable to the new theory, but a warm advocate for the practical conclusions which have been drawn from it. He expresses also his belief that the facts which have lately been ascertained with respect to the changes of the blood in cholera, would never have been *thought of* had it not been for the paper which was read at the College of Physicians in May 1830.]

ANALYSES & NOTICES OF BOOKS.

Dublin Journal of Medical and Chemical Science. No. IV. Vol. II.

CONTINUING our notice of the last number of our Dublin contemporary, we have next to allude to

Contributions to Thoracic Pathology, by DR. STOKES.

The paper is an interesting one, intended and calculated to illustrate some obscure pathological points, and shewing, in a very remarkable manner, the advantage in diagnosis derived from the stethoscope. The first case is a striking proof of both those positions: an extensive abscess forms in the right lung; after a time it heals, and the cicatrix happens to insolate a portion of the lung. The patient is afterwards attacked with pleuro-pneumonia, and while all the adjacent pulmonary texture participates in the inflammation, that portion which is separated by the cicatrix escapes, and is ascertained by auscultation to have done so. The details are very interesting:—

"In the spring of 1829, a young man, by trade a blacksmith, was admitted under my care to the fever wards of the Meath Hospital, for an affection of the chest, which had been of a few weeks standing, and ushered in with the usual symptoms of an acute irritation of the lung. On examination he presented the symptoms of bronchitis, but to our surprise we found that the right lung, from the clavicle to the mamma, sounded dull. *Here and over the shoulder, all the signs of an extensive cavity were discoverable by the stethoscope, cavernous respiration, gurgling, and distinct pectoriloquism.* Over the rest of the thorax the phenomena of bronchitis existed.

"The patient suffered much from cough and dyspnœa, but under the usual treatment of bronchitis, these symptoms almost completely subsided. He regained in a short time his looks, flesh, and strength; the pulse became natural, and his appetite excellent, although all the signs of the abscess continued unchanged. In this state he left the hospital, declaring himself perfectly well, and, indeed, to an ordinary observer, there was no symptom whatever that would cause a suspicion of organic disease of the lung.

"After a few weeks, having caught cold, he returned with a slight bronchitis, the signs of the abscess continuing unchanged; and, after some days, again left the hospital, and resumed his occupation of a smith. We then lost sight of him for a twelvemonth, when he was again admitted labouring under severe symptoms of pleuro-pneumony, which had been neglected, and were of five days' standing. It appeared that after his last dismissal, he had enjoyed the most perfect health, although toiling at his laborious occupation, until five days before his admission, when he was seized with pain of the side, cough, dyspnœa, and fever: he continued to work until his sufferings obliged him to desist, and on the following day he entered our wards.

"He then presented all the symptoms of the third stage of acute pleuro-pneumony of the right lung. On percussion, the whole of this side, both anteriorly and posteriorly, sounded completely dull, *except in the sub-clavicular region, where it was comparatively clear.* This, it will be recollected, was the former seat of the abscess. Over the whole dull portion, bronchial respiration, mixed with an intense muco-crepitating râle, was audible; but, on examining the sub-clavicular region, we found to our great surprise, *that all the phenomena of a cavity had disappeared, and were replaced by a puerile respiration.*

"Here was a case full of difficulty. It was plain that the greater portion of the lung was solidified, and had passed into the third stage; but why a small portion of it should have escaped the disease *in toto*, when the rest was so far advanced, and that this portion should be that formerly occupied by an abscess, was indeed difficult of explanation.

"All treatment proved inefficacious, and the patient sunk on the third day after admission, the stethoscopic phenomena having continued unaltered.

"On dissection, we found the right lung solid over the whole extent indicated by the stethoscope. From the fourth rib downwards, the pleura was covered with coagulable lymph, which being removed, allowed us to see the lung through the serous membrane of a yellow colour. In the superior portion of the lung, the adhesions were evidently old, as considerable strength was required for their separation. On the summit and antero-superior surface, the peculiar puckered appearance described by Laennec, as resulting from the cicatrization of an abscess, was evident.

The lung was then divided in a line running along the situation of the angle of the ribs, so as to separate it into two portions, connected only at the root of the lung. This gave us at once an explanation of all the phenomena. The supero-anterior portion, for a space of three square inches, was perfectly crepitating, and not all engorged. This was separated from the rest of the organ by the cicatrix of the abscess, which had been obliterated by the adhesion of its walls, so as to form a cartilaginous septum, superiorly of half an inch in thickness, and easily separable through its whole extent into two layers. This septum ran from behind forwards, commencing at the summit of the lung, and terminating where the large bronchus separates to be distributed to the upper lobe of the lung; its whole length being more than three inches. Thus the subclavicular portion was isolated, and we found that its bronchial communication was preserved by a trunk passing from the larger tube immediately below the termination of the cicatrix. The interlobular septa in this portion were remarkably hypertrophied, but in no other respect did it differ from healthy lung. The remainder of the upper lobe, the middle and inferior lobes, were in the state of grey hepatization. No tubercle was found in any part of the system."

Cure of Phthisis Pulmonalis is the flattering title to the portion of the Essay which follows. Dr. Stokes has been for some time convinced that pulmonary consumption was cured more frequently than he had been taught to believe; and within the last year has seen several cures, "in all of which the most unequivocal indications of tubercle existed," and in which, nevertheless, a cure, more or less complete, was effected. The plan usually adopted was that of establishing a drain, or counter-irritation, in the neighbourhood of the cavity. Exercise, travelling, and attention to the general health; especial caution being observed in the administration of purgatives—a class of medicines which he thinks increases much the risk of ulceration of the digestive tube: "Purgatives must be inhibited, and enemata and the mildest laxatives used when necessary." The diet was "bland and nutritious;" but wine was not allowed in any of the cases which recovered. Another point worthy of attention is, that Dr. Stokes thinks harm is often done by the too free use of astringents, especially sulphuric acid, with a view of checking the colliquative perspirations.

On comparing the cases in which recovery took place, Dr. Stokes says:—"There appear certain points of resemblance between them, the consideration of which throws light on the question of the curability of consumption. None of these patients, with the exception of the first, presented that general appearance called the strumous diathesis. Their hair, eyes, and complexion, were dark; their muscular fibre was originally strong; they presented no marks of external glandular disease; their family was not consumptive; the disease had supervened on an inflammatory attack of the lung; *and in none was there diarrhœa, or other signs of gastro-enteric complication.* I shall again allude to this last circumstance, as I look on it as one of essential importance.

"Although, perhaps, the division may not be found to stand a critical investigation, yet I think we may separate cases of phthisis into two classes, the constitutional, and accidental phthisis. The first, where the tubercular development supervenes, either with, or without some precursory irritation in persons strongly predisposed to it from original confirmation. In these the disease generally runs a rapid course, invades both lungs, and is commonly complicated with gastro-enteric disease. In the second variety, we meet the disease in persons not of the strumous diathesis, and who have no hereditary disposition to the affection. In them the exciting cause is generally an inflammatory attack of the lung. The disease proceeds slowly, is long confined to one lung, the hectic is slight, and often wanting, and the liability to enteric disease is much less. These are the cases in which it appears to me rational to hope for a cure, and justifiable to adopt decided measures, even after extensive lesions have been formed in the lung. With a single exception, these were the cases in which I have witnessed a cure, either by cicatrization of the abscess, the cretaceous transformation of the tubercle, or by both of these modes together."

There is also much justice in the following remark, which the experience of almost every man must have verified.

"There is another case nearly equally common, where the life of the patient is shortened, and the chance of cure removed, by the same practice. A young female with symptoms of incipient phthisis, ceases to menstruate. To this, which is the effect and not the cause of the disease, all the symptoms are attributed, and the efforts of the practitioner are directed not to remove the irritation of the lung, *but to force the uterine action.* Emmenagogues, which in British practice means cathartics, are lavished, and the consequences are such as might be expected from this unphysiological and barbarous practice. The pulmonary disease advances rapidly; first, because it is neglected; secondly, because the lung is stimulated by the enteritis induced by the remedies; diarrhœa sets in, and the delicate and amiable patient dies of a '*Galloping Consumption.*'"

The absence of strumous diathesis, the slowness of progress, the freedom from constitutional symptoms, and the integrity of the digestive system, are the great sources of a favourable prognosis.—But, alas! how seldom are these combined in a case of genuine phthisis!

Empyema next comes under consideration, on which the only circumstance we have to remark is, that Dr. Stokes regards it as more frequently curable without operation than most practitioners seem to think.—Repeated blistering is what he mainly trusts to.

The paper on *Thoracic* Pathology concludes with two cases of *abdominal* aneurism.—Is our intelligent author desirous of reminding us that he writes for the other side of the Irish Channel?

Some valuable observations are contained in a communication from the pen of Dr. Graves, and entitled, *Observations on the Treatment of various Diseases.* The account of hooping-cough treated by tincture of cantharides is the only portion for which we can make room.

"The treatment of hooping-cough is of vast importance, as the disease, particularly in young and delicate children, frequently proves either directly fatal, or else lays the foundation of other equally unmanageable forms of pulmonary complaints. The observations of Doctor Mackintosh upon the symptoms, pathology, and treatment of this disease, are excellent, and ought to be attentively perused by every practitioner. It appears to me, however, that he has omitted to mention one of the most valuable remedies, tincture of cantharides.

I quite agree with Dr. Mackintosh in the treatment he proposes for the first or catarrhal stage of the disease; the antiphlogistic regimen and treatment, leeches to the larynx, in some cases even general blood-letting, antimonials, and ipecacuanha in nauseating doses, are the remedies which the first stages of hooping-cough almost invariably require; after the second or third week, and in some cases even after the first, the well-marked symptoms of constitutional fever, and local inflammation, generally subside, while the violence of the cough, and the peculiar sound of the hoop, are far from being alleviated. It is at this period that the true nature of the disease is most evident; it is now that it too frequently becomes an *opprobium* to the medical attendant. The patient's friends complain that no remedy can be found to allay the cough, and abate the peculiar sense of suffering which accompanies the paroxysms of hooping, and they think it very singular, that while the health in all other respects seems unimpaired, the original disease goes on unmitigated. Hence it is that they seek for, and not unfrequently apply, empirical remedies. Many authors have mentioned the tincture of cantharides as a medicine which has occasionally proved useful in hooping-cough. Most practitioners, however, are prejudiced against its employment, and prefer remedies which are not so likely to excite anxiety in the mind of the physician, during the period of their exhibition. Such, I confess, were my former feelings upon this subject, and my antipathy to the tincture of cantharides would have probably continued had I not been persuaded by my friend, Doctor Thomas Beattie, to give the medicine a fair trial. He alleged in its favour, his own experience, and that of his father, who had for many years employed tincture of cantharides in the following formula:—

℞ Tincturæ Cinchonæ Compositæ, ℥v.
——— Cantharidis.
——— Opii Camphorat, aa. ℥ss.
M. Ft. Mistura.

One drachm of this may be taken in linseed tea, or barley-water, three times a-day, and in persons above five or six years of age, the dose may be daily increased one-third, until half an ounce is taken three times in the day. Much smaller doses, however, than that last mentioned, will frequently be sufficient to check the violence of the disorder, and, of course, when this happens, the dose need not be augmented. Thus, in the case of a grown-up woman, named Mahon, admitted into the Meath Hospital on the 17th of May last, the maximum dose was a drachm given five times a-day. This case was treated under my direction, by Mr. James Costello, and was of three weeks' standing when admitted. I treated, along with Mr. Nicholl, a young lady aged nine, who was seized with the first symptoms of the disease on the 13th of May last. On the 6th of June she commenced taking the medicine at the rate of a drachm three times a-day; on the 8th the diminution in the hooping was evident, and on the 13th she did not hoop more than twice a-day. There was no occasion to augment the dose of the medicine in this case.

If necessary, I could relate many other cases in which this medicine produced a most decidedly good effect; but at present I shall content myself with observing that the tincture of cantharides, exhibited in the formula recommended by Doctor Beatty, has the peculiar method of producing its good effects without giving rise to urinary irritation, at least in a very great number of cases no such irritation takes place. Doctor Beatty informs me that he has been equally successful in curing the disease in infants by means of this remedy exhibited in appropriate doses; such cases are seldom placed under my care, and therefore I cannot speak from my own experience on the subject. What influence is exerted by the cochineal and other ingredients contained in the compound tincture of bark, and camphorated tincture of opium, I am unable to determine. A very important observation is made by Dr. Mackintosh: "Change of air is extolled by some individuals, but is often productive of great mischief, by occasioning a return of the disease It is an important fact, that during the late epidemic, which was the most severe I have ever witnessed, all the children that were moved for change of air, had the disease the longest." Now it is very remarkable that the late Dr. Beatty made it an invariable rule to keep his patients confined to their bed-room until the cure was completed, and he used to lay great stress upon an accurate observance of his directions with regard to this point. I have reason to believe that in the great majority of cases the recovery of the patient is much accelerated by this means. This fact alone is sufficient to demonstrate that hooping-cough is owing to something very different from mere inflammation, for after the first violence of catarrh, laryngeal inflammation, or bronchitis, has subsided, change of air often acts like a charm in removing the disease.

Since I have adopted the remedy above-mentioned, I have laid aside the tartar emetic ointment altogether; indeed I never derived any evident or permanent advantage from its application in hooping-cough, either over the stomach or the spine."

MEDICAL GAZETTE.

Saturday, September 22, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

CLOSE OF THE VOLUME—A RETROSPECTION.

THE present number brings to a close another volume of the Gazette — *the Tenth,* — and thus forms a resting-place whence we may be allowed to look back, for a moment, on our past career, and to glance at our future prospects. The past speaks for itself: our journal, with whatever good or evil it may contain, is before the public, and though we cannot suppose that they will take our estimate of its merits instead of judging for themselves, yet to some points connected with its history it may not be impertinent, nor altogether uninteresting, to advert. In judging of the manner in which any undertaking has been executed, it is necessary to keep in mind the circumstances under which it was begun, and the ends which it had in view; nor, perhaps, ought even the motives of its conductors to be entirely lost sight of. To most of our readers the early history of the Gazette is sufficiently well known; but the recent accessions to our subscribers have been so considerable, that we may fairly presume. many who now do us the honour of perusing our pages, are unaware of the occasion which first called forth our humble efforts as medical journalists.

At the time to which we allude, a tyranny of a new and intolerable kind had been set up over practitioners generally, but particularly over those in the metropolis—such as we believe had no parallel in the records of any other profession. This state of things, which was rapidly bearing down the respectability of our calling, and degrading medical science by holding up its cultivators to the public as men destitute alike of talent, honour, or humanity, cannot perhaps be better described than in the words of a late eloquent writer, to whose pen we were frequently indebted: after alluding to the once peaceful tenor of the medical career, he continues,—and the remarks on the subject of lectures especially have recently found a most conspicuous illustration,—

"But a few years ago a set of literary plunderers broke in on the peace and quiet of our profession. Lecturers who had spent their lives in collecting knowledge—arranging it for communication—and acquiring the difficult art of oral instruction, saw the produce of their lives suddenly snatched from them, and published for the profit of others, with the additional mortification of finding what they had taken so much pains with, disfigured by bad English and ridiculous or mischievous blunders. Whoever attempted to arrest these piracies became the object of furious and unrelenting abuse. Hospital physicians and surgeons, who have to prescribe and operate in public, and at stated times, in whatever condition of bodily health or mental feeling they may happen to be, and exercising in the face of critics (not always competent to decide on their merits)—a science so avowedly imperfect as to afford abundant scope for uncandid and ill-natured remarks, however judiciously practised,—were held up to public scorn for errors to which, even if actually committed, the ablest men are occasionally liable, while those who leagued in secret with their calumniators, and who, with one or two exceptions, were as insignificant in station and talents as they were equivocal in character, were represented as at the summit of science and professional eminence.

"We do not deny that public exposure may have, in some few instances, done good; it may have abolished some foolish custom, or led to the reformation of some trifling abuse; but weigh the evil against the good—it has deprived eminent men of their intellectual property, and destroyed the mutual confidence between pupils and their teachers; it has lowered the respectability of the profession, and has spread general distrust; it has broken up private friendships; it has placed man in hostility to man; and has set so many bad passions into ferment, that well-disposed men become disgusted with the state of their profession, and vow that they never will inflict it on their sons."

Such was the state of our profession at the time this journal was projected, and such the nature of the evils it was intended to remove—or at least to mitigate. So serious, however, were the difficulties which presented themselves—so many had been the previous failures in the same attempt—such was the dread of encountering that remorseless persecution with which *they* were sure to be visited, who dared the wrath of a paper which had always waged war to the knife, against whoever presumed to oppose it—such, in short, from various considerations, was the reluctance to the undertaking, that both the publisher and the editor, with whom the plan originated, abandoned it as hopeless. A different view, however, was taken by the spirited House which afterwards ushered this journal into the world, and the result has shewn the superiority of their discernment. For ourselves we always confidently anticipated the success which has followed, because we looked upon it as a calumny on our profession to suppose that there existed not in it a sufficient number of men willing and able to secure the triumph of a journal conducted on principles of integrity and good faith, if any degree of talent were displayed by its contributors, and any tolerable diligence exhibited in its management. The praise of talent we willingly bestow where it is justly due; namely, on those numerous and eminent members of the profession who originally became, and have subsequently continued, our steady and valued contributors; the merit of industry, —and it is all we claim—will not we trust be denied to ourselves. Fearful of becoming egotistical, we shall allude to ourselvse no farther than this:—the only difficulty which we have experienced in regard to our editorial conduct has been how far we should restrict our pages to mere details on points of science, and how far grapple with our opponents, and practise retaliation, by speaking of them, personally, so as to make them individually experience the smart of those weapons they were wont to make use of against others. We could only do this, it is true, on unequal terms; for while the whole profession was open to one side for malicious caricature or malignant detraction—to us, only those concerned in conducting this atrocious system were presented as objects of attack; and even here, unlike our adversaries, we had of course to confine ourselves to what was true. The subjects to be depicted, as we have hinted, were few, but fortunately they were of a very striking description—they only required to be faithfully displayed—neither caricature nor detraction were required in the exposure, nor could they have added one iota to the effect of exhibiting the parties *in puris naturalibus*, unmasked, and stripped of all those disguises with which the press enables artful men to cloak the loathsome nakedness of their secret motives. To no part of our labours do we look back without pleasure except to those devoted to such delineations, for although they were defensive and in exposure of the unworthy, we did and do feel, that the necessity which occasioned them was one derogatory to science—but the shame rest with those in whose violation of all those decencies which had previously been held sacred, that necessity originated.

No one who has paid attention to the events in our profession during the last few years, can fail to have perceived, that a great change has taken place in the state of general feeling—there exists not now that prurient appetency for slander to which the novelty of the system which we deprecate had gradually led. How far the change to which we refer has been effected by any labours of ours, is a matter of little moment; though it is not unreasonable to suppose that the contradiction of falsehoods, almost as soon as penned, had some influence in rendering the manufacture less agreeable and less profitable; at all events, the article was soon found less marketable, and a smaller supply was consequently provided. In proportion as this turn in the public taste continued to advance, in the same ratio did the competition between the journals become more equal, because more confined to the same kind of supplies;—and it is in the highest degree gratifying to us to be able to refer to the past year as having produced a remarkable increase in the number of our correspondents—as it also has a great augmentation of our subscribers. This statement, with regard to the state of the concern, considered in a mercantile point of view, is one which those acquainted with our editorial character (to which at the end of ten volumes we may, perhaps, be permitted to appeal) will of course believe on our simple asseveration; albeit, we perceive that just the reverse is gravely declared by a contemporary, who either pertly prates of what he knows nothing about, or else asserts that which he knows to be untrue. Again, then, we refer with gratification to the original communications which have appeared in this journal during the past year; comprising, in the number and eminence of the contributors, such a list as no medical periodical published in this country has ever before had to boast of; and we would add, that by "original" papers *we* mean those really furnished by the reputed authors—an explanation which we feel called upon to make in contradistinction to a method recently adopted by some others, of advertising as contributors to their work distinguished men, who have not written one line of what is given in their names, and some of whom have actually adopted legal proceedings in consequence of this disgraceful piece of knavery.

We have said that we should take a retrospective view of the past, and a glance at the future. The former we have in some measure done—but as to the latter, it is an unopened book, and we may not speak of its contents so positively. No considerable change is contemplated in our general plan, for we are chary of altering that under which we have flourished. The departments in which so much important matter has already been accumulated, will be continued; and we hope with undiminished interest. The valuable Lectures which will constitute a complete *System of Medicine*, by Dr. Elliotson, will be resumed in our next number; and we shall give, in the ensuing volumes, some short *Courses*, particularly one on the *Diseases of* [illegible] *Eye*, by Mr. Middlemore, of Birmingham; which, from the interest of the subject, the extensive opportunities of the writer, and the manner in which the essays are executed, we doubt not will be acceptable to our readers. We shall also, from time to time, adopt such suggestions of our correspondents as we find practicable, and calculated to be of interest;—to such course, for instance, we owe the introduction of the *Weekly Meteorological Reports* and *Bills of Mortality*; which, though they occupy but a nitch, present an interesting record connected with the science of public health.

As to our *principles*, we have no professions to make—but take leave to refer to the past: if the articles which have recently appeared on the three medical corporations be not admitted as evidences of our entertaining liberal opinions, and being ready to express them with freedom, no declarations of ours could be of any value;—besides, we have always observed that those who are loudest in their protestations of independence, are, in reality, the most subservient to what they think is the most popular side of the question: with freedom on their lips, they are the very slaves of party.

We have only further to add, that, desirous of making our external appearance keep pace with our internal improvements, a new fount of types has been procured expressly for this journal, and that the present is the last appearance we shall make in our old suit.

Here, then, we close our volume, and, with respect to that which is to come—if we may borrow a phrase, which the allusion to the arrangements of our printer has suggested, and thus *typify* the subject—we would express our earnest hope, that the *impression*, which each reader receives, may be a good one.

SUPPLY OF THE SCHOOLS OF ANATOMY.

The principal anatomical teachers of the metropolis have had a meeting this week, at the Freemasons' Tavern, to arrange matters of common concernment for the ensuing campaign: Mr. Stanley in the chair. Resolutions were passed expressive of abhorrence of the old system, and determination to discountenance altogether the disgusting race of exhumators. It was then agreed that a deputation should wait on the Home Secretary, to obtain from him, if possible, assurances that no favour or partiality should be observed towards any person or persons in particular; but that an equitable distribution of subjects should be secured, in proportion to the wants of each school: and for this purpose a subscription was entered into. We think there cannot be a doubt that it is the bounden duty of every teacher of anatomy, at the present moment, to come forward and make common cause with his brethren, and to see that no private or underhand trafficking be pursued: it is the only way in which all parties can be permanently benefitted, and placed in a reputable and proper light before the public. Why, then, may we ask, were not the gentlemen of Guy's and St. Thomas' present?

EFFECTS OF DARKNESS IN PRODUCING DEFORMITIES.

A correspondent writes us the following curious fact. There is at present in Paris an artist of the Louvre, an eminent historical painter, of the name of Ducornet, who paints with his feet. He was born without arms, of poor parents, at Lille. There are also about the French metropolis a number of beggars, twelve or thirteen of them at least, all deformed in various ways, and all born at Lille, in certain dark caverns under the fortifications. The effect of these places, from their want of light producing malformed births, is so notorious, that the magistrates of Lille have issued strict orders to prohibit the poor from taking up their abode in them. It is added by our correspondent, that he had a conversation with Mr. Edwards on the subject, and that gentleman was greatly struck with the confirmation which the above circumstances afford to his views, stated in his work, *Sur l'influence des agens physiques sur la vie.* Mr. Edwards's experiments of detaining tadpoles in darkness, and thus causing them to grow into gigantic and monstrous *tadpoles*, instead of being transformed into frogs, are well known.

WEEKLY ACCOUNT OF BURIALS,

From the BILLS OF MORTALITY, *Sept.* 25, 1832.

Abscess	1
Age and Debility	45
Apoplexy	4
Asthma	8
Childbirth	7
Cholera	39
Consumption	75
Constipation of the Bowels	3
Convulsions	30
Croup	2
Dentition or Teething	8
Dropsy	22
Dropsy on the Brain	9
Dysentery	2
Erysipelas	1
Fever	13
Fever, Scarlet	5
Fever, Typhus	4
Gout	3
Hæmorrhage	2
Heart, Diseases of	1
Hooping-Cough	6
Inflammation	31
Inflammation of the Bowels & Stomach	10
Lungs and Pleura	3
Insanity	7
Liver, Diseases of the	4
Measles	5
Miscarriage	1
Mortification	7
Paralysis	2
Small-Pox	17
Sore Throat and Quinsey	2
Thrush	1
Tumour	1
Unknown causes	79
Stillborn	1.

Decrease of Burials, as compared with the preceding Week . . . } 187

METEOROLOGICAL JOURNAL.

September 1832.	THERMOMETER.		BAROMETER.	
Thursday . 20	from 30	to 61	30 42	to 30 44
Friday. . . 21	38	65	30·46	30·44
Saturday . 22	39	64	30·42	30·31
Sunday . . 23	40	67	30·29	30·24
Monday . . 24	38	73	30·30	30·33
Tuesday . 25	40	71	30·35	30·30
Wednesday 26	39	71	30·24	30·16

Wind variable, S.W. and N.W. prevailing.

Except the 22d, clear.

The unusually fine, and very warm weather of the past week, is worthy of particular remark.

CHARLES HENRY ADAMS.

LIST OF LECTURERS.

We cannot comply with the request of a Pupil, by devoting any part of the body of the journal to the announcement of lectures; we refer him to the advertisements on the wrapper.

NOTICE.

The case of Schirrus of the Intestine did not come to hand: we shall be happy to insert it if our correspondent can furnish us with another copy.

BOOKS RECEIVED FOR REVIEW.

Mr. Corbyn on the Epidemic Cholera, as it has prevailed in India.

Mr. Sheldrake's Animal-Mechanics applied to the Cure of Spinal Curvature and other Deformities. Part I.

Dr. Keir on Cholera, as it prevailed in Moscow.

Mr. Percivall's Anatomy of the Horse, embracing the structure of the Foot.

ERRATUM.

In Mr. Morrah's paper, page 801, line 15, *for* "a few doses," *read* "a full dose."

W. WILSON, Printer, 57, Skinner-Street, London.

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